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THE MONTHLY MICROSCOPICAL JOURNAL:

TRANSACTIONS

OF THE

ROYAL MICROSCOPICAL SOCIETY,

AND

RECORD OF HISTOLOGICAL RESEARCH

AT HOME AND ABROAD.

EDITED BY

HENRY LAWSON, M.D., F.R.M.S.,

Assistant Physician to, and Lecturer on Physiology in, St. Mary's Hospital.

VOLUME VII.

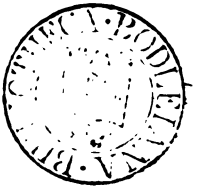

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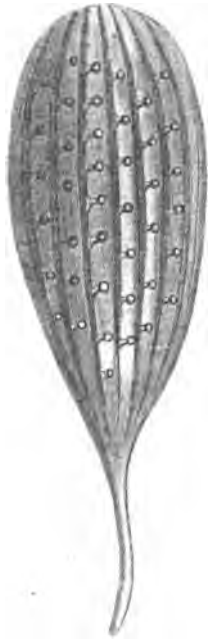
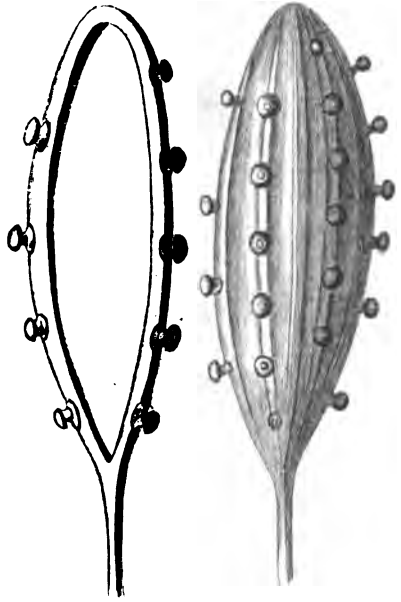
Fig. 1



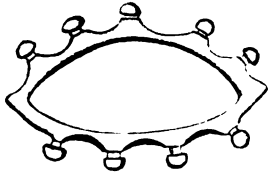
Front view.

Fig. 2.



Profile and Section of Profile.

Fig 3



Section at right angles to Axis of Scale.

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W.West & Co. imp

Imaginary construction of Battledore Scale of Polyom. Alexis.

Fig 1 nearly as observed under most perfect conditions.

THE

MONTHLY MICROSCOPICAL JOURNAL.

JANUARY 1, 1872.

I.—*The Markings on the Battledore Scales of some of the Lepidoptera.* By JOHN ANTHONY, M.D. Cantab., F.R.M.S., &c.

(*Read before the* ROYAL MICROSCOPICAL SOCIETY, *Dec.* 6, 1871.)

PLATES I. AND II.

HAVING long felt assured that the ordinary coarse representations of the markings on the "battledore scales" of butterflies were most imperfect, if not altogether erroneous, I last summer prepared a large number of slides of these scales, from the wings of the common meadow-blue butterfly (*Polyommatus Alexis* of Stainton), with a full determination to examine them critically, under varied conditions of light, and with the aid of the very best modern optical appliances.

I carried on this examination at intervals for several months, and the interest I felt in this self-imposed task increased greatly as I went on, inasmuch as, although I had been pretty familiar with the microscopic appearances of most of the scales of the Lepidoptera, I most certainly had never seen anything at all resembling what I made out on these battledore scales. In what I saw there was nothing incompatible with the usual representations of the markings, it was simply the reduction of chaos to order, though the clear, definite structures I made out were so singular, that I hesitated for some time to accept them as other than "Fata Morgana," of which the microscope is too often justly accused. However, a more and more careful elimination of all probable sources of error, as from spectra, shadows from obliquity of light, &c., showing me constantly the same definite appearances, I feel a confidence in the

EXPLANATION OF PLATES I. AND II.

PLATE I.

Imaginary Construction of Battledore Scale of *Polyom. Alexis* :—
Fig. 1.—Front view.
„ 2.—Profile and section of profile.
„ 3.—Section at right angles to axis of scale.

PLATE II.

Details of Battledore Scales of *P. Alexis*.

reality of what I observed, which causes me to bring the matter before the notice of my brother microscopists.

Let me state then, that I see the markings on the ribs of the battledore scales of the "meadow-blue" butterfly as elevations, very much resembling in shape the vegetable glands seen on the petal of the Anagallis—a well-known microscopic object; that is, the elevations have a base, a column, and a rounded head, or capital, as I have represented in the drawings on Plate II., and which drawings were carefully made at the time of observation from objects which can be referred to again by means of "Maltwood's finder." On Plate I. I have given an imaginary construction of the battledore scales, with sections, and which are intended to show, what I believe to be the arrangement of these columnar elevations on the ribs of the scales. Placed as these columns are, perpendicular to the plane of the scale, when they are looked at with the scales lying flat on the glass slide, head, column, and base being in the same axial direction, and in different planes, the appearance is much as it is usually represented, a dot upon a flatter dot, and of course more or less indistinct. But if a large number of scales be looked over carefully, some of them will be sure to be found slightly tilted over to one side, and others from accidental causes slightly bent or distorted, and it is on these scales that the elegant little columns show most distinctly, being seen more or less in profile. Supposing that a promising scale has been found, it requires, in order to see these markings well, that the illumination should be carefully managed, so as to avoid all obliquity, and the consequent production of heavy shadows, for *the purer and more direct the light*, the better I have always found true structure to be shown. To get this purity—if I may be allowed to call it so—I always work with a rectangular prism in the place of the usual plane mirror, and I am careful to place this prism, with its axis strictly at right angles to the direction of the illuminating ray, and I do this more readily by always placing my microscope facing the light, and having a small curtain just in front of the eye-piece. I should not do this except I found an advantage in it; and in this position the markings on the battledore scales seemed to me to be more clearly made out when the scale was in a vertical, or nearly vertical, position on the stage of the microscope. For very perfect vision, light should be most carefully centred, and the proper sized diaphragm stops employed, to cut off all "milkiness" and "glare" from the object, and then compensation should be very exactly made for the refraction of the covering glass, even though this nice operation should be facilitated by the use of the "immersion" principle. When a good scale has been selected, and these delicate adjustments attended to, I think the appearances I have de-

Fig 1.

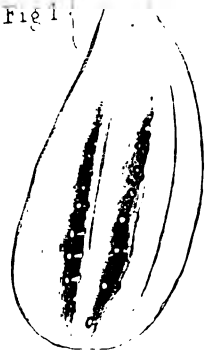


Fig 2.

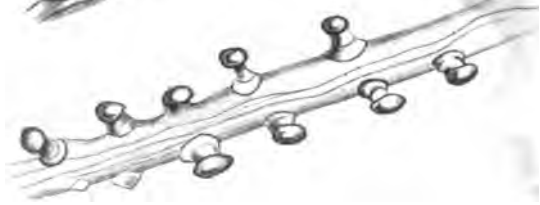


Fig 3.

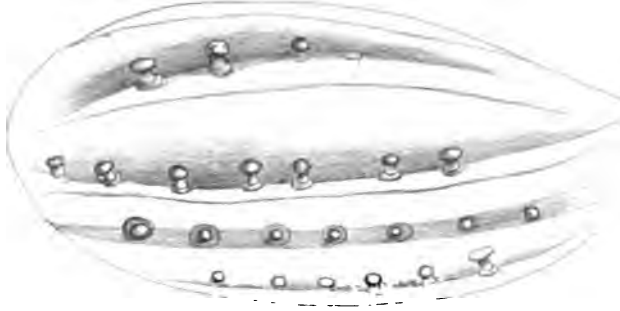
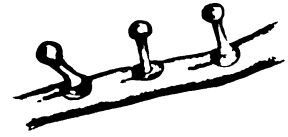


Fig 4

Fig 5.



scribed will be readily made out, particularly as they do not require at all a high power for a general view of them. The modern $\frac{1}{8}$th of Powell and Lealand, which is a superb objective, shows these small columns distinctly as I have drawn them in Fig. 5 of Plate II., but they can be well seen with a Beck's $\frac{1}{5}$th, which is so good as to bear a D eye-piece most satisfactorily. I wish it to be understood that I am not advocating the use of low-power objectives and high-power oculars in a general way, but now and then, as in the objects in question. Such an arrangement has its advantages, inasmuch as the low-power objective allows us to see a greater number of planes in focus, and so to get the general shape and relative position of an object which we can afterwards examine in detail, with all the fine definition obtainable by a deep objective and shallow ocular.

I hope I have not been wearisome in the description of these columnal elevations in the ribs of the battledore scales. The examination may seem trivial—time apparently thrown away upon an insignificant object, but the investigation has a value in this, if in nothing else, as showing that we must not take for granted that we know all about a structure, because it presents certain appearances when viewed in one position. Also, we must not lose sight of the fact, as we re-observe our well-known objects with our magnificent modern appliances, that, though our glasses are vastly improved, yet they are still far from perfect, and that probably those who come after us in examining these same objects, may give a half-pitying smile at what we thought we saw, and still more at what we failed to see.

II.—*The Nerves of Capillary Vessels and their Probable Action in Health and Disease.* By Dr. LIONEL S. BEALE, F.R.S., Fellow of the Royal College of Physicians, Physician to King's College Hospital.

(*Read before the* ROYAL MICROSCOPICAL SOCIETY, *Dec.* 6, 1871.)

PART I.—ANATOMICAL INVESTIGATION.

PLATES III., IV., AND V.

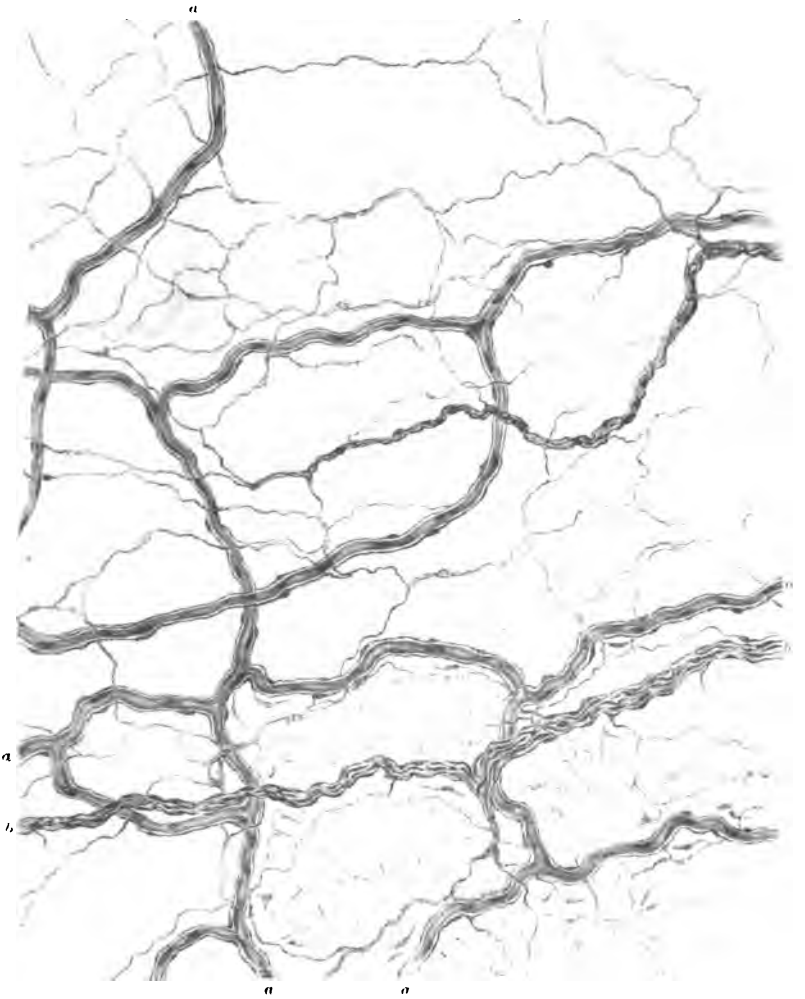
WHEN studying the distribution of nerves to striped muscle in 1860,* I had seen nerves lying close to capillary vessels, and during the next two years obtained several preparations from the frog, toad, and newt, in which pale delicate nerve fibres, which had been followed from trunks containing dark-bordered nerve fibres, were discovered running by the side of almost every capillary vessel ramifying over an extensive area of tissue. In 1863 some drawings were published,† and in my Croonian lecture to the Fellows of the Royal Society in May, 1865, the distribution of nerves to capillary vessels was briefly described, and the function performed by them discussed. Although I have incidentally referred to the fact in several lectures and papers published since this period, I have not collected my observations, nor until now have I entered into the matter so fully as it deserves. The subject appears, however, to have been almost completely passed over by other anatomical observers in this country and on the Continent.‡

* "On the Distribution of Nerves to the Elementary Fibres of Striped Muscle," 'Phil. Trans.,' June, 1860.

† "On the Structure and Formation of the so-called Apolar, Unipolar, and Bipolar Nerve Cells of the Frog." May 7, 1863. 'Phil. Trans.': Part II., 1863. Published separately. Churchill, 1864.

‡ It is not a little remarkable that the opinion should, until very recently, have been entertained that *all* the arteries even are not supplied by nerves, and that the contraction of the unstriped muscular fibre is induced independently of nervous influence. It is some years since I was led to the conclusion that all forms of muscle are supplied by nerve fibres, and I am of opinion that every small artery possesses nervous supply even in those instances in which I have myself failed up to this time to demonstrate nerves. I do not think, however, that all capillaries have nerve fibres distributed to them, and it is quite certain that, as in the case of different tissues, the number of nerve fibres over a given area varies greatly. Kölliker made the remarkable observation that some of the arteries are destitute of nerves, and that the walls of arteries are not in such need of nerves as is usually supposed. Eberth, in Stricker's 'Handbook,' just published, says, "with the *exception of the capillaries*" the presence of nerves has been demonstrated in (upon ? L. S. B.) all vessels, but he remarks that he has not been able to convince himself of the precise mode in which they terminate, especially as regards the muscular fibres of the arteries and veins.

After this paper had been read on December 6th, Dr. Klein showed me some plates to illustrate a memoir by him upon the same subject, which will appear in the January number of the 'Quarterly Journal of Microscopical Science.' His specimens were prepared by the gold process, and give a different idea of the distribution of the nerves to that afforded by mine, some of which are ten years old. My own conclusions on the ultimate distribution of nerve fibres were formed several years ago, at a time when terminal nerve networks were denied in



Connective tissue covering part of the mylohyoid muscle of the frog and extending from its posterior portion, showing capillaries and nerve-fibres distributed to them. *a*, Capillary vessels with their nerve-fibres. *b*, Bundles of fine dark bordered nerve fibres, from which fine nerve-fibres may be traced to the capillaries, as well as to their distribution in the connective tissue, where they form networks of exceedingly fine compound fibres. The engraving represents the specimen as if magnified only 1.0 diameters, but the original drawing was taken from it when placed under a much higher power. The specimen was mounted in 1863, and still (1871) demonstrates the points represented in the drawing.

RELATION OF FINE NERVE-FIBRES TO CAPILLARY VESSELS.

Structure of Capillaries.

It is scarcely possible to consider this question of the distribution of nerves to capillaries without referring to the structure of the capillaries themselves. It is curious to note the remarkable vacillations of doctrine with regard to some of the simplest matters of fact. Thus it has been lately affirmed by Eberth, in his article in Stricker's 'Anatomy,' that the walls of capillary vessels are *contractile* and composed of *protoplasm*. Now the evidence advanced in favour of this view is most defective—indeed, as regards the capillary vessels of the adult, there is none. No one has seen the membranous part of the wall of an adult or old capillary vessel contract, while it would be difficult to pick out a tissue less like protoplasm than the transparent material composing the capillary wall. If the thin elastic membrane of the capillary is to be called protoplasm, why should not the posterior elastic lamina of the cornea, or the thin transparent elastic membrane lining an artery be also regarded as of this nature? But these things, and living growing moving protoplasm, are quite different in their properties. It is indeed difficult to conceive how anyone who had really studied the matter, could speak of the oval "nuclei" or bioplasts of the capillary wall and the tissue of the wall itself which intervenes between them, and was formed by them, as being composed of the same substance "protoplasm;" unless he admitted that this wonderful material might, for example, constitute the moving, changing, semi-fluid sarcode of the amœba, as well as the firm, passive, unchanging material of which the yellow elastic tissue consists. In that case we should be applying the same name to things distinct in their nature and properties from one another, and we shall mislead ourselves and others by endeavouring to establish resemblances which do not exist in nature, and by ignoring differences which are observable by everyone who will simply examine with care and without preconceived theory. If the oval nuclei are protoplasm, and the membranous wall is protoplasm, the student will of course ask us how we account for the important differences between these two things, and then we shall be reduced to the unfortunate expedient of suggesting that one is protoplasm, and the other protoplasm "modified." If the inquirer has the bad manners to press us further and ask "how modified?" the only

Germany, and when it was supposed that only in a few exceptional cases did the axis cylinder of a nerve extend beyond the white substance. Not only are my networks of pale nucleated nerve-fibres now accepted, but it is maintained that much finer networks of nerve fibres ramifying upon and amongst epithelial cells and other elementary parts, and even upon an individual mass of bioplasm (nucleus), have been demonstrated. At present, however, I cannot regard the observations upon which it is thought to establish this view, more conclusive than those which a few years since led many to the conclusion that the axis cylinder sprang from the nucleus or nucleolus of the central nerve cell.

philosophical answer to give is "variously." This we shall find perfectly convincing, and not followed by any further troublesome inquiries concerning the nature of the physical nuclei or basis of capillaries.

Nuclei, or Masses of Bioplasm of Capillaries.

More or less connected with the wall of the capillary vessel are numerous "nuclei," consisting of *living matter* or *bioplasm*, which vary greatly in number in the capillaries of different tissues. Of these there are at least four distinct sets which may be distinguished in well-prepared specimens.

1. Those in the capillary wall itself, which have taken part in its formation, and which are intimately concerned in nutrition as long as blood circulates through the vessel. These vary greatly in number in different capillaries, as represented in many of my drawings, and in size also at different times. In some specimens they are extremely numerous, and, as I pointed out in 1863, project into the interior of the capillary vessel. Probably from these are detached particles of bioplasm, which pass into the blood current and may grow into white blood corpuscles.

2. Masses of bioplasm outside, but at a varying distance from the capillary wall with which they are connected by extensions or processes. These have no doubt originated from the first by fission, and are indeed an early stage in the formation of new capillaries, as may be proved by examination of the capillaries of adipose and some other tissues which undergo great and rapid changes even in the adult.

3. Oval masses of bioplasm, generally, but not invariably, smaller than the above, and, unlike them, sometimes crossing the vessel obliquely. These are connected with the delicate nerve fibres distributed to the capillary vessels. In some cases these bioplasts, as well as the fine nerve fibres connected with them, are almost embedded in the wall of the capillary, but oftentimes they are seen to be separated from it by a distinct interval, which varies much in extent, as is well demonstrated in some of my specimens.

4. Elongated and stellate masses of bioplasm which belong to the connective tissue. These vary greatly in number, size, and appearance in different tissues.

Nerve Fibres.

I have demonstrated that nerve fibres are distributed to capillary vessels in almost all the tissues of the frog and newt. Among these I would particularly mention the *skin* and *mucous membranes*, the capillaries of the *lung* and *kidney*, those of the *pericardium* and *fibrous membrane near the liver*, and those of the *mesentery*, as well as the capillaries of *muscle and nerve*.

Fig. 1.

Capillary vessel with peculiar dilatations [illegible] Opposite a the terminations of [illegible] dark bordered nerve-fibres is seen, and at [illegible] divided numerous branches of the terminal net-work of very fine pale bordered nerve-fibres are represented. × 700.

Fig. 2.

[illegible] × [illegible]

Fig. 3.

From an interval between the fibres of the mylohyoid muscle of the hyla. a. Trunks of fine dark-bordered nerve-fibres, with fine fibres coming from them, some of which may be traced to the capillary, while others are distributed to the muscular fibres, which are not represented in the drawing. The arrangement of the nerves supplying the capillary vessel is well seen. From a specimen nearly ten years old. × 315.

Fig. 4.

[illegible] × 700.

RELATION OF FINE NERVE-FIBRES TO CAPILLARY VESSELS.

The nerves distributed to capillary vessels are much more difficult to demonstrate in mammalia, but I am sure that they exist, and in considerable numbers. The tissues of man and the larger mammalia are very unfavourable for so delicate an investigation, in consequence of the very diaphonous character of their nerve fibres and the great density of the connective tissue in which they are embedded, but in the mouse, shrew, mole, and some other small animals, they may be distinctly seen in very thin preparations. But during the last few years I have obtained some most excellent preparations from the bat's wing. In these, nerves to the capillaries may be demonstrated conclusively with the aid of a $\frac{1}{12}$th. The $\frac{1}{25}$th brings them out still more clearly. In the preparation I have with me, which should be placed under the twelfth of an inch object-glass, many delicate nerve fibres can be seen with great distinctness running very close to some of the capillary vessels. In order to demonstrate this fact, it is necessary to remove the dark cuticular covering from both surfaces of the membrane of the wing—by no means an easy operation or always followed by success.

Arrangement of the Nerve Fibres distributed to the Capillaries.

With regard to the general arrangement of these delicate nerve fibres, it is to be remarked that in many instances a fibre may be seen running on each side of a capillary vessel. The two fibres are often connected by short branches which pass over or under the vessel. Plate III., Plate IV., Fig. 3.

Not unfrequently the nerve is so close to the capillary that it cannot be seen distinctly in all parts of its course, but oftentimes the capillary shrinks after death, and then a distinct interval is left between its walls and the nerve fibre (Plate IV., Fig. 4). In some cases the nerves are still more numerous, and in the bat's wing I have seen three or four very fine fibres ramifying over a capillary for a short distance. Over the capillary vessels of the mucous membrane of the frog's palate (Plate IV., Fig. 1), these fine nerve fibres are easily demonstrated, and oftentimes may be seen a complete plexus of delicate nerve fibres with numerous oval and triangular masses of bioplasm connected with them. Upon the capillary loop of the fungiform papillæ of the human tongue (young subject) I have seen very fine nerve fibres in considerable number. Over the capillaries of the ciliary processes of the eye fine nerve fibres ramify very freely. All these nerve fibres are connected with oval masses of bioplasm, which vary in size and number in different animals and in different tissues of the same animal. In some cases the bioplasts are separated by a considerable distance from one another ($\frac{1}{50}$th of an inch), but often they are not more than $\frac{1}{500}$th of an inch apart. At the point where a fine branch divides into two others the mass of bioplasm is triangular;

and specimens in which four, or even five excessively delicate nerve fibres diverge from a mass of bioplasm with as many angles is occasionally met with. Thus, as in other situations, lax networks are formed, the meshes of which are for the most part long and narrow.

Central Origin and Peripheral Connections of Nerve Fibres distributed to the Capillaries.

As regards the origin and connections of nerve fibres ramifying upon the capillary vessels I have some important facts to record.

1. In many instances, particularly in the fibrous membrane about the bladder of the frog, I have followed fine nerve fibres from ganglion cells to the smallest arteries, where they form a plexus from which pass branches direct to the capillaries.

2. I have traced nerves direct from the ganglia embedded in connective fibrous tissue to the capillaries. See Plate V., Fig. 3.

3. A specimen under the microscope this evening proves that a fine nerve fibre given off from a bundle of dark-bordered fibres distributed to voluntary muscle may be followed direct to a capillary. See Plate IV., Fig. 3.

4. From the ganglia between the muscular and mucous coats of the small intestine of any small animal (mouse, mole) fine nerve fibres can be followed in considerable numbers, and traced to the capillaries of the mucous membrane. I have never been able to see them on the vessels of the villi, but feel convinced they are to be demonstrated as far as this point. Even in the human subject I have succeeded in making some good, but not perfectly demonstrative specimens.

5. "I have seen a dark-bordered nerve fibre divide into two branches, one of which ramified upon an adjacent vessel, while the other was distributed to the elementary fibres of the muscle." Also "nerves distributed to arteries and to elementary fibres of striped muscle have been seen to be derived from the same trunk of dark-bordered fibres."*

With reference to the peripheral connections of nerves distributed to capillaries, I have to remark—

1. That in the papillæ of the frog's tongue, I have followed fibres from the expansion of the sensitive nerves above the capillary loop to the capillary vessels, and also from a somewhat similar structure in the mucous membrane of the snake's mouth to the capillaries. In both cases fibres are also given off from the bundles of dark-bordered fibres before they break up to form the reticulated sensitive expansion.

2. The bundles of sensitive fibres which break up and form expanded networks in the meshes of the beautiful capillary net-

* Croonian Lect., 'Proceedings of the Royal Society,' May 11th, 1865.

Fig. 1.

Fig. 2.

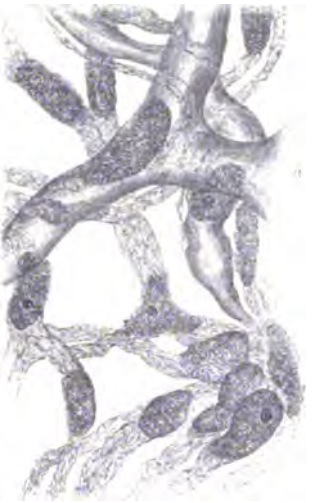
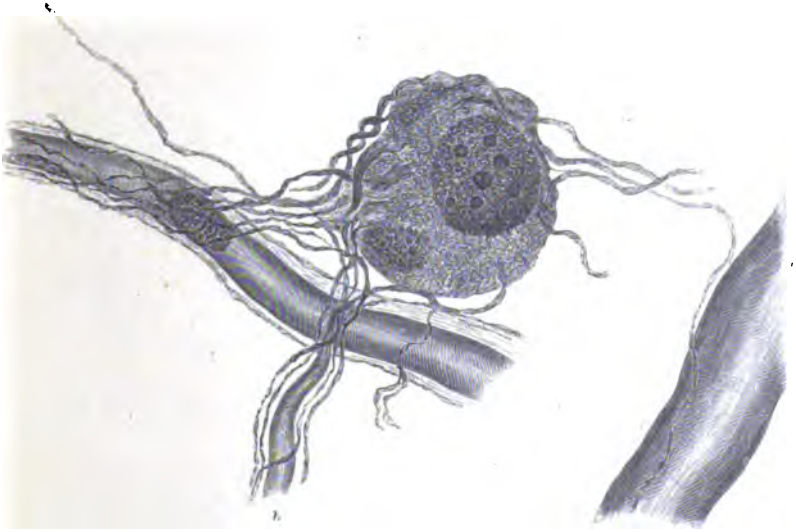


Fig. 3.



Ganglion cell with fibres connected with it passing off in three different directions. The large dark-bordered fibre immediately below the cell has no connection with it, and is probably not influenced by it. One of the fibres passing from the cell becomes a fine dark-bordered fibre (*b*), *a* is a capillary vessel. From the bladder of the hydatid. × 1,500. December, 1862.

RELATION OF FINE NERVE-FIBRES TO CAPILLARY VESSELS.

work at the extremity of the mole's nose give off fibres which may be traced to adjacent capillaries.

3. Branches from the nerve fibres ramifying over the minute arteries may be frequently followed from these to the capillary vessels.

4. In the case of the striped muscles of the chameleon's tongue, I have succeeded in following a fine fibre from the so-called nerve tuft to the neighbouring capillary vessels.

5. In the muscular coat of the frog's bladder, in that of the oviduct, and in the muscular coat of the small intestine, the fine nerves form an intricate interlacement, some fibres of which are distributed to the muscles, while others ramify upon the little arteries, veins, and capillaries.

These nerve fibres distributed to the finest capillaries of many tissues have therefore been traced from *ganglia*, from *sensitive and motor nerve trunks*, from the *peripheral ramifications both of sensitive and motor nerves*, and they are intimately related to the ultimate ramifications of some of the *nerves of special sense*.

These anatomical facts suggest many considerations bearing upon the mode of action of the peripheral portion of nerve fibres, but the subject is too extensive to discuss in this paper. It may form the subject of a separate memoir.

Method of Demonstration.—I have already described the method pursued in the preparation of the specimens. It is that which I have followed for more than ten years, and which in my hands has been most successful.* I feel sure that it is capable of further improvement in practical details, and that, upon the principles which I have laid down, delicate structures, which have not yet been seen by man, will be demonstrated by patient and well-practised observers. The process is troublesome, and for this reason it has not been in much favour. In these days investigation must be conducted with such haste, and new facts discovered so quickly, that there is little chance of getting many persons to spend sufficient time in mere practice to enable them to gain the requisite skill for the very much more minute investigation of the structure of the most delicate textures which is now so much required, and which must be carried out before we can hope to arrive at positive conclusions on fundamental anatomical questions of the greatest importance.

*** The probable mode of action of the nerves described in this communication will be considered in the Second Part of the memoir, which will be published in the next (February) number of the Journal.

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* 'How to Work with the Microscope.' 4th Edition. 'The Physiological Anatomy and Physiology of Man.' By Dr. Todd and Mr. Bowman. Second Edition. By Dr. Beale. Part I., page 57.

III.—*Note on Dr. Barnard's Remarks on "The Examination of Nobert's Nineteenth Band."* By J. J. Woodward, Assistant-Surgeon, U. S. Army.

My own paper "On the Use of the Nobert's Plate"* and Dr. Barnard's reply,† state so fully the arguments in favour of a complete count of the band on the one hand (my method), and on the other, of a count of a measured portion of the band only, say twenty to thirty lines (Dr. Barnard's method), that I think the question may now advantageously be left by both of us in the hands of those who are competent to judge of the comparative certainty and accuracy of the two methods.

Nevertheless I think justice to myself requires that I should call attention to the contradictory interpretation which my distinguished friend has been led to give to the same series of observations in consequence of what I must regard as his misplaced confidence in a method which I believe to be deficient in the accuracy required for this purpose.

Dr. Barnard's original observations were related by him in a private letter to Mr. Stodder, dated January 29, 1868, and Mr. Stodder published them in a note to his paper "On Nobert's Test-plate and Modern Microscopes" in the 'American Naturalist,' vol. ii. p. 93.‡ Mr. Stodder tells us that Dr. Barnard resolved the nineteenth band with a Spencer's $\frac{1}{12}$th and a Tolles's $\frac{1}{8}$th, *both dry lenses*. With the former objective he made a series of counts of a measured fraction of the band, which gave by reduction a series of estimates of the number of lines to the English inch, 106,226 lines being the smallest, and 115,474 lines being the largest estimate.

At the time Dr. Barnard made these observations the subject of the spurious lines so readily developed on the plate had not received due consideration. I had myself about the same time supposed that I had resolved the nineteenth band with a dry Wales's $\frac{1}{8}$th, but subsequently convinced myself that I had been deceived by the spurious lines.

The observations of Dr. Barnard recounted by Mr. Stodder as well as some made by Mr. Stodder himself with a Tolles's immersion $\frac{1}{8}$th, reported in the same paper, became therefore the subject of a private correspondence, and several conversations between Dr. Barnard and myself, and that gentleman became so thoroughly convinced that he also had been misled by spurious lines, that in a public lecture delivered before the American Institute in New York city, November 25, 1868, he announced his conviction that the

* This Journal, July, 1871, p. 26.
† Ibid., October, 1871, p. 194.
‡ Reprinted in the 'Quarterly Journal of Microscopical Science,' July, 1868.

finest lines of the nineteenth band plate "have never been as yet fairly resolved." *

In the following spring, however, having received an immersion $\frac{1}{16}$th from Messrs. Powell and Lealand, I was so fortunate as to effect undoubted resolution of the refractory bands, and to furnish a count of the number of lines; and now Dr. Barnard, as his recent paper shows, *reconsiders* his own judgment of his own work, and claims to have effected true resolution with the *dry* Spencer's $\frac{1}{12}$th, though he writes me he still admits the lines shown by the dry $\frac{1}{8}$th to have been spurious. I confess I am not convinced by his present reasoning that the judgment deliberately announced in his public lecture of November 25, 1868, was erroneous, and must continue to believe that a method which has permitted so careful and conscientious an observer to contradict his own conclusions in this manner, ought not to be preferred to the more accurate and absolute criterion of resolution which I have proposed for the higher bands of the plate.

IV.—*A New Erecting Arrangement, especially designed for Use with Binocular Microscopes.* By R. H. Ward, M.A., M.D., Professor of Botany and Microscopy in Rensselaer Polytechnic Institute.

For dissecting and other manipulations under magnifying powers, the simple microscope is awkward and unsatisfactory, and has been made to serve as a binocular only with very low powers; but the superb field of the compound microscope has been comparatively little used for these purposes, because few persons can work to advantage under an inverting arrangement; the erectors usually furnished are not good, and the use, otherwise satisfactory, of a good objective as an erector has not as yet afforded the advantage of binocular vision. The simple expedient now proposed is designed to increase the usefulness of the stereoscopic binoculars in ordinary use by rendering them easily available for purposes which require an erect image.

Last summer, the writer proposed, at the Indianopolis Meeting of the American Association, to place, for certain purposes, an erecting objective below instead of above the regular objective of the microscope. Then, of course, the regular objective becomes the erector, and the accessory one below acts as the objective. This simple expedient, applied to Wenham's and other non-erecting binoculars, leaves little to be desired for the purposes of a dissecting microscope. The lenses of a $1\frac{1}{2}$ or 2 inch objective (preferably a solid or single-combination one) may be packed or screwed into

* 'Annual Report of the American Institute of the City of New York for the year 1868,' No. IX., page 43.

the upper end of an adapter which, when screwed into the nose-piece of the microscope, carries them up close to the binocular prism, and into the lower end of which, lengthened more or less by two or three adapters of various lengths, the object-glass may be screwed. A more elegant but scarcely more satisfactory arrangement is an adapter with a sliding-tube adjustment, which varies to the extent of an inch or more the distance between the erector and objective. Different powers and distances will, of course, be used, according to the wants of different observers. The combination which has proved most convenient in my hands consists of a 2-inch erecting lens close to the binocular prism and a $\frac{2}{3}$-inch objective at a distance, measured to its lower end, of from 3 to $4\frac{1}{4}$ inches below the erector; giving powers of 10 to 50 diameters, and requiring a working distance between the stage and the binocular prism of $4\frac{1}{2}$ to 5 inches, which is quite practicable with large stands. A shorter working distance may be gained at a slight disadvantage. With a 2-inch erector and a $\frac{4}{10}$-inch objective, powers of 8 to 50 diameters can be secured without raising the binocular prism more than 4 inches above the stage; and with a 1-inch erector and $\frac{2}{3}$-inch objective a power of 40 diameters is obtained with the prism $3\frac{3}{4}$ inches from the stage. When, however, sufficient working distance cannot be obtained, the object may sometimes be placed upon the sub-stage, or oftener the sub-stage removed and the body racked down, so as to focus through the empty stage upon the table, a block or box, or an extemporized stage occupying the usual position of the mirror and illuminated by the mirror after the method suggested by Mr. James Smith. In this case it is often desirable to increase the working distance between the prism and the object by varying the lenses employed. Thus a $1\frac{1}{2}$-inch objective at from $3\frac{3}{4}$ to $5\frac{3}{4}$ inches from the erector will give powers of 6 to 50 diameters, and working distance from prism of 7 to 10 inches. The erector may also be removed an inch or more from the prism. When this latter arrangement is to be used exclusively, the apparatus is further simplified by screwing a 2-inch objective into the nose-piece in its usual position, as an erector, and screwing or sliding over it an adapter carrying a $1\frac{1}{2}$ or 2 inch objective from 4 to 6 inches lower down. Some contrivance is required to illuminate transparent objects under the lower powers; but opaque and translucent objects on a black ground can be dissected and manipulated with great facility.

The same erecting arrangement can be used in connection with monocular microscopes that have no draw-tube. It may also be used as a means of working Wenham's and other binoculars with high powers. With powers of 500 or 1000 diameters, however, it is still difficult to obtain good definition or to fully light both fields.

TROY, NEW YORK, U.S.A., *Nov.* 6, 1871.

V.—*On a New Micrometric Goniometer Eye-piece for the Microscope.* By J. P. Southworth.

After a few experiments by Dr. H. T. Porter and myself, we have succeeded in making an eye-piece micrometer and goniometer which equal in accuracy and surpass in simplicity and cheapness any we have seen, and we have used those of some of the best makers in this country. The objection to the eye-piece micrometers in use is the want of boldness in the division lines, which makes them fatiguing and hurtful to the eyes. To overcome this objection we were led to experiments in making micrometers by the aid of photography, which have resulted in success. The steps of the process are these:—

1st. A scale of 100 heavy India-ink lines of about $\frac{1}{8}$th of an inch apart is drawn on a dead-white surface of Bristol board. The lines marking every ten divisions are 6 inches long, and extend 1 inch each side of the scale; those marking every five divisions are 5 inches long and extend one-half inch beyond the scale; the remaining lines are 4 inches long.

2nd. By photographic process for copying engravings, a negative is taken, on which the scale equals about 2 inches in length, and is intensified by mercuric chloride and potassium cyanide.

3rd. With a copying camera and lens for taking transparent positives for the magic lantern, a transparent positive of this negative is taken on micrometer glass, reducing the scale to the length of one-half inch. In this the lines are $\frac{1}{100}$th of an inch apart. After intensifying, washing, and drying, a cover of thin glass is cemented on with Canada balsam, and the slide cut to fit the slit in the micrometer eye-piece. It can be also mounted with a spring and micrometer screw, like Jackson's micrometer. In our micrometer the lines appear to stand out in relief, and are jet black, while the spaces between them are translucent enough to admit of the accurate measurement of the details of minute algæ and fungi to the $\frac{1}{2500}$th of an inch.

Regarding the goniometer:—

1st. A circle about 18 inches in diameter is drawn with India-ink, divided into degrees. The centre is indicated by a dot, and one diameter is drawn. Every 5 and 10 degrees are indicated by longer lines than those indicating single degrees. Every 10 degrees of each quadrant are numbered from 0 to 90.

2nd. A negative 2 inches in diameter is taken by the process referred to above, and from this a transparent positive is taken on a circle of micrometer glass cut to fit the tube of the microscope. It is covered with a circle of thin glass cemented with balsam, and mounted to fit the tube at the focal point of a positive eye-piece. A cobweb is drawn across the diameter of the lower lens. When a

crystal is to be measured, the stage is moved till the apex of the angle coincides with the centre of the goniometer and the diameter with one side. The eye-piece is now turned till the cobweb crossing the diameter at the centre coincides with the other side of the angle. Now the number of degrees of the angle can be read at the circumference. The advantage of this over the ordinary microscopic goniometers is, that in ours the angles of the crystal and the degrees of the goniometer are on the same line of sight within the tube of the microscope, while in the ordinary goniometer the degrees are marked outside the tube. The photographic processes by which the above are made can be learned by consulting any of the standard works on photography, under the sections that treat of copying engravings and taking transparent positives.—*Silliman's American Journal, December*, 1871.

VI.—*On the Action of Hydrofluoric Acid on Glass viewed microscopically.* By H. F. Smith, Esq.

Having had occasion some time ago to use hydrofluoric acid (in solution) for the purpose of etching on glass, I observed that in some lights the varieties of colour presented while the action was going on were very brilliant. This led me to examine it microscopically. The hydrofluoric acid was prepared in the ordinary method, from calcium fluoride by the action of sulphuric acid. The solution was then diluted and kept in a lead bottle for use when required.

When the acid was first dropped upon the glass, no action was evident, the appearance presented being simply that of a drop of water on glass. In a very short time, however, the drop became a little duller, but this almost immediately cleared away, and several small particles, seemingly of glass, were seen floating in the drop. These seemed to be undergoing a process of *fusion;* the appearance being similar to that seen when a small portion of metal is thrown into some of the same substance in a state of fusion; it is tossed about for some time, and then finally disappears. This was what evidently appeared to me to be going on here, the hydrofluoric acid having apparently a solvent action on the glass. What strengthened this opinion was the presence of magnificent colours, changing every moment as these small portions of glass were liberated from the larger piece, and were undergoing the process of solution, thus leading one to suppose they consisted of small glass prisms, the colours being more perfect than those obtained by *water-prisms* simply. Some of these particles were completely surrounded by a halo of colour, as if they had been thrown into a

variegated solution. The principal colour evident in such cases was a deep green, but dark blue was also seen at rare intervals.

The above observations were repeated several times, and always with the same results, with the exception that the small particles of glass floating in the drop of acid *exploded* now and then, causing a great commotion in the liquid and throwing up little jets of finely-divided acid, behaving as if the small glass particles were hollow spheres. I may also mention that when these explosions occurred, bright flashes of light were visible, resembling closely the appearance of rainbows seen in waterfalls.

NEW BOOKS, WITH SHORT NOTICES.

Traité du Microscope, son Mode d'Emploi; ses Applications à l'Étude des Injections; à l'Anatomie humaine et comparée, etc., etc. Par Ch. Robin, Membre de l'Institut (Académie des Sciences) et de l'Académie de Médecine, etc. Paris. J. B. Baillière et fils. 1871.—Many works as we have in our language upon the microscope, and manifold as are the instruments described, the apparatus mentioned, and the tissues displayed, it is nevertheless a fact that there is in the English language no book which deals more with the subject of human anatomy than with those of the comparative branches of histology. Doubtless this is because those who devoted themselves to the subject of comparative anatomy were so much more numerous than the students of, let us say, human histology. Hence, doubtless, the reason why in such admirable works as those of Dr. Carpenter, Dr. Beale, and Mr. Hogg, much relating to the human microscopic anatomy has been omitted to make room for facts in the anatomy of Protozoa, Cœlenterata, Annulosa, and Mollusca. 'Tis true that Dr. Beale's work enters more upon human histology than either of the others, but then he does not go fully into the subject for the very natural reason that those who pursue it are vastly fewer than those who delight in the study of Rotifera and Infusoria. Of course we do not refer to Dr. Richardson's American work, for it deals with human anatomy almost exclusively, and furthermore it is small in extent of pages, and is almost devoid of illustration. There is therefore room for such a work as the present one, which attempts to deal with the whole of microscopy, and which does fair justice to each of the departments it takes in hand.

The work now before us is a large 8vo volume of more than 1000 pages, and with over 200 woodcuts, besides three or four plates at the end. It is divided into three parts. The first deals with the subject of injections; the second part treats of the various forms of microscope constructed in France, England, &c., &c.; next it deals with the instruments and apparatus to be employed in the production of cells and the general mounting of microscopic objects, the making of sections, the chemical agents employed in microscopy, the modes of using the microscope, and of reflecting the light from the mirror, &c. The third and last part, which forms about half the volume, was, we may say, almost absent from the work which appeared in 1849, and hence the present volume may be looked upon as a comparatively new treatise. It deals with the application of the microscope, and its auxiliaries to anatomy, physiology, medicine, animal and vegetable natural history, chemistry, and agricultural economy. It is this part of the volume which, in our opinion, gives it a special character, and helps to add to our information facts which do not exist in any of the English works already noticed.

The opening chapter deals with the subject of injection, and goes into the question very fully. The author gives a preference to Dr.

Beale's Prussian blue fluid, but he suggests some modifications which do not appear to us of any importance. After going very fully into the subject of the different forms of injection, the various fluids, the transparent and opaque, he enters on the subject of pursuing injection upon the living animal. He gives a section to the method, and describes fully the process employed by himself and M. Onimus. Then follow the precautions to be taken in injection, the differences between injection and infiltration, the rupture of parts during the operation, the injection of veins, partial injections, and last of all, of physiological injections.

Then comes the chapter on microscopes and apparatus. This is not so good as we should have expected. It is confined to French and English microscopes, and it figures none of the latter save a couple of the instruments of Ross. The chief illustrations it contains of advantage to English masters are those of the dissecting lens of Lacaze-Duthiers; the new great microscope of Nachet, which is a peculiar instrument; and a prismatic eye-piece, which reverses the position of the rays, and makes an object appear in its ordinary position. The rest of the chapter deals with the various methods of correction, and it gives a figure of the instrument for measuring the angle of aperture of objectives; it deals well and fully with its subject. It is followed by a chapter detailing at considerable length the various chemical agents employed in microscopical research. Then follow various chapters describing the drying of tissues, the preparation of microscopic objects, microscopic dissection, thin sections of hard tissues, the instruments employed in preparing the sections, the method of illumination, followed by a discussion upon the different modes of lighting up an object, and dealing tolerably fully with the mathematical bearings of the subject; and lastly there is a very short chapter which leaves unnoticed much of the work done on the subject of photographing microscopic objects, &c., &c.

Finally there comes the remaining half of the volume which is devoted to the methods of preparing specimens of the various tissues already named, for microscopic purposes. This is by far the most valuable and likewise the most novel part of the work. Of course the reader will find much in it which he has already learned from Dr. Carpenter, and Dr. Beale, but he will also find abundance of novel matter which we will venture to say he cannot discover elsewhere. Thus the book is on the whole a useful and valuable addition to our microscopic literature. It has only one fault, and that is the paucity of its illustrations. Dr. Carpenter's, which is really a smaller work, contains very much more than double the number of illustrations which M. Robin has thought necessary, thus rendering it most valuable to the student. We trust that M. Robin will see to this in next edition, and we hope that that period may soon come round, for the volume is really a most valuable addition to our microscopic literature.

PROGRESS OF MICROSCOPICAL SCIENCE.

Structure of the Mole's Snout.—This is given very fully in the last part of Max Schultze's 'Archiv,' in a paper by Herr Th. Eimer, of Würtzburg,* which is briefly abstracted in the 'Lancet.' The muzzle of the mole in part supplies the place of its extremely defective organ of vision. The sensory area is situated at its anterior extremity, and presents even to the naked eye a papillated surface. When examined with the microscope, the papillæ are found to be low elevations varying from a tenth to a fifth of a millimètre in height. The papillæ are composed of epithelium, which not only projects from the surface, but dips into the corium: the cells occupying the little fossa in the corium are ribbed cells. Each papilla is traversed from base to apex by an hourglass-shaped canal, the periphery of which is of course bounded by the epithelial cells. The canal is filled by a structureless homogeneous mass, probably of the nature of mucous tissue. The nerves traverse this tissue, and end by free extremities in the outer division of the hourglass-like cavity, which Eimer terms the tactile cone. The disposition of the nerves is very peculiar. If the skin of the snout be treated with chloride of gold, the corium is seen to be richly supplied with a plexus of medullated nerve-fibres. From these, small fasciculi containing twenty or more are given off, which run to the base of the papillæ and enter the inferior cone, at the same time losing their medullary layer. They then appear to arrange themselves in the form of a circle, presenting in transverse sections very much the appearance of the copper wires in the electric cables. As the fibres ascend the tactile cone towards the surface of the skin, they are connected by a slight nodal enlargement or varicosity with each successive tier of cells, terminating as near the surface as the third or fourth epithelial cell-layer. Here and there a nerve-fibre (cylinder axis) runs amongst the epithelial cells of the papillæ external to the cone. As the number of papillæ is about five thousand, and as about twenty nerves enter each tactile nerve-cone, the total number of fibres must amount to about one hundred thousand, and their very superficial termination is a matter of great interest.

Various Investigations on the Dust of the Air.—We have of late had various reports on this subject. The first is by Mr. J. Sidebotham, and was read before the Manchester Philosophical Society at a late meeting. It appears that while travelling by rail between Saltley and Camp Hill, he spread a paper on a seat of the carriage, near the open window, and collected the dust that fell upon it. A rough examination of this with the $\frac{2}{3}$rds power showed a large proportion of fragments of iron, and on applying a soft iron needle he found that many of them were highly magnetic. They were mostly long, thin, and straight, the largest being about $\frac{1}{50}$th of an inch, and, under the power used, had the appearance of a quantity of old nails. He then with a magnet separated the iron from the other particles. The weight alto-

* Band 7, Heft 3.

gether of the dust collected was 5·7 grains, and the proportion of those particles composed wholly or in part of iron was 2·9 grains, or more than one-half. The iron thus separated consisted chiefly of fused particles of dross or burned iron, like "clinkers"; many were more or less spherical, like those brought to our notice by Mr. Dancer from the flue of a furnace, but none so smooth; they were all more or less covered with spikes and excrescences, some having long tails like the old "Prince Rupert's" drops; there were also many small angular particles like cast iron, having crystalline structure. The other portion of the dust consisted largely of cinders, some very bright angular fragments of glass or quartz, a few bits of yellow metal, opaque white and spherical bodies, like those described by Mr. Dancer, grains of sand, a few bits of coal, &c. After the examination of this dust, he could easily understand why it had produced such irritation; the number of angular, pointed, and spiked pieces of iron, and the scoriæ or clinkers, being quite sufficient to account for the unpleasant effect. He thinks it probable that the magnetic strips of iron are laminæ from the rails and tires of the wheels, and the other iron particles portions of fused metal, either from the coal or from the furnace bars. The large proportion of iron found in the dust is probably owing to the metal being heavier than the ordinary dust, and accumulating in cuttings such as those between the two stations named. If he had to travel much by railway through that district, he should like to wear magnetic railway spectacles, and a magnetic respirator in dry weather.

American Researches on the Microscopy of the Atmosphere.—Mr. Charles Bailey drew the attention of the Manchester Philosophical Society to the subject *à propos* of the above paper by Mr. Sidebotham. He stated that Mr. Charles Stodder, of Boston, U.S., had been recently making a series of researches on the microscopic contents of the atmosphere of that city. Amongst other investigations he was led to examine a fine black dust from a beam in the polishing shop of the United States' Armoury, at Springfield. He found it to contain a few vegetable fibres, some apparently organic fragments, and some broken crystals; but the great mass of it was made up of amorphous fragments of iron, of the $\frac{1}{100}$ mm. and upwards in size, as well as curved and irregular fibres and masses of iron, with sharp jagged edges, from 5 to 15 mm. in size; there were also some very minute perfect spheres, probably iron. In trying the effect of the magnet upon this dust he found it removed it from a sheet of paper as completely as if it had been swept off with a brush, and he concluded that the non-metallic portions adhered to the iron particles by the thin layer of oil with which all the particles of dust were coated. To prevent this dust passing into the atmosphere of cities, Mr. Stodder recommended a plan which had been put in practice many years ago in this country, but abandoned from the indifference of the workpeople, *viz.* the fixing of magnets in the immediate neighbourhood of grindstones and polishing wheels. In the same report, Mr. Stodder alludes to the labours of two members of the Manchester Society—Dr. Angus Smith and Mr. J. B. Dancer—in examining the contents of the air, and points out an important matter considerably affecting the results of such investigations, *viz.*

the method employed for filtering the air through water. The usual method has been to place a small quantity of pure water in a large bottle, and shake it in the air under investigation, repeating the operations with renewed volumes of air in the same water; but Mr. Stodder shows how impossible it is to intercept all the foreign particles in the atmosphere in this way, inasmuch as the smallest bubbles of air which pass through the water very much exceed in size the particles of matter which are sought for, and myriads must elude observation. A greater difficulty, however, is to obtain absolutely pure water for such experiments; and whether filtered or distilled water was used, a drop evaporated on a glass slide always left a deposit of scaly and granular particles. This result, as Mr. Stodder justly says, puts an end to this mode of investigation, and throws a cloud of suspicion on all reported researches in this line, when water was the medium used.

Description of new Genera and Species of Australian Polyzoa.—Mr. P. H. Mac-Gillivray, M.A., read a paper on the above subject some time ago before the Royal Society of Victoria. Still, as the journal containing the paper only reached us a couple of months since, we think the subject may be new to some of our readers. In this paper are given descriptions of forty-eight species, including two genera, of Australian polyzoa, which cannot be satisfactorily referred to any of those hitherto described. The identification of polyzoa by the aid of descriptions alone, however accurate these may be, is often extremely difficult. The species here described, as well as the others existing in Victoria, will be figured in Professor M'Coy's 'Memoirs of the Museum,' where Mr. Mac-Gillivray hopes to be able to give descriptions of all those with which he is acquainted. Specimens will also be deposited in the National Museum. He has added a list which contains all the Victorian species he has in his collection, with the exception of a few not yet determined. The descriptions then follow, but are far too numerous for our space. See Reports of the Royal Society of Victoria.

The Tissues of Man and Apes.—Dr. Lionel Beale, F.R.S., imagines that there are material differences between the tissues of man and apes, and he calls on naturalists to investigate them. In a very able and lucid address to the Quekett Club, he says:—"It is wonderful what haphazard assertions are made in these days concerning the likeness or identity of dissimilar things. Observers, who should test these assertions, and ascertain whether they are accurate or not, permit them to pass without comment, and the public accepts them as literally true. We are told, for example, by Mr. Darwin, that it is 'scarcely possible to exaggerate the close correspondence in general structure, in the minute structure of the tissues between man and the higher animals, especially the anthropomorphous apes.' But Mr. Darwin does not tell us that he or anyone else has made the observations upon which the statement is founded. A careful comparison of the tissues of man with the corresponding tissues of apes in minute structure is much to be desired, but it has never been made, and it is quite premature to speak of the supposed 'close correspond-

ence' as if it had been proved to exist. As to the close correspondence in chemical composition, asserted to exist between man and apes, the same remarks may be made. Such correspondence has yet to be shown."

Vitality of Disease Germs.—In a recent number we referred to Dr. Grace Calvert's numerous experiments on this subject. Since they were made, Mr. H. B. Yardley writes as follows to the 'Chemical News':—"Dr. Calvert (in previous numbers) in his paper says, that at 400° germ life cannot exist, but I take it that his experiments were conducted at 100°, 200°, 300°, 400°, 500°, and 600° Fahr. only, and that the intermediate temperatures were not tried. If this is the case, it might happen that even at 330° Fahr. germ life would be destroyed, which will consequently render disinfection by heat practicable. Not having the time or apparatus to repeat Dr. Calvert's experiments myself, I take the liberty of mentioning this." We may mention that the letter was *à propos* of a statement by Mr. Richard Weaver in a former number of the Journal, that 350° is the highest heat that may be used for disinfecting without destroying the fabrics.

How to Destroy Disease Germs in Clothing.—In a letter to the 'Chemical News,' Mr. G. E. Davis states his own experience on this subject. *A propos* of Dr. Calvert's researches, he says he has devoted a considerable portion of time to the study of disinfectants—in fact, sanitary chemistry generally, and the fact struck him as soon as he entered the field that the miasm was not destroyed by the heat of most disinfecting chambers. In fact, in some, where the heat employed is very low, owing to a badly-constructed oven, very little good can accrue from the baking process alone. At the Sanatorium attached to Eton College the beds from the scarlatina patients used to be baked at rather a high temperature, but owing to scorchings having taken place the temperature has been considerably reduced. The heat now employed, if used alone, is, in his opinion, only just enough to warm the disease germ and make it feel very comfortable. The heat cannot be raised too high owing to its irregular working, the temperature being sometimes 40° or 50° higher than the thermometer indicates. In December, 1870, he found the heat was only got up to 120° Fahr., but it was supposed to rise to 160° Fahr. He gave as his opinion that the heat had very little action upon the disease germ, and proposed placing a vessel containing carbolic acid mixed with an equal volume of water in the hot chamber. He has now written to know whether the system proposed by him has been fully carried out. He finds that for the last nine months, when articles have been placed in the disinfecting chamber, the heat has been averaging from 120° Fahr. to 160° Fahr., and a vessel of dilute carbolic acid (1 to 1) has been placed in the next compartment to the clothes. The vapour of the phenol has been carried over by the combined effects of heat and steam, and has saturated the clothing. This he considers a good method, certainly better than baking alone, for ensuring complete destruction of any lower organism. Previous to December, 1870, the method of disinfecting the patients' linen before washing, was to steep in solution of bleaching powder, but owing to the solution being

sometimes made improperly, holes were *burnt* in the clothing: he thereupon proposed a dilute solution of carbolic acid; it is easy to mix, perfectly soluble, and has no action upon fabrics in the diluted state.

Nucleated Sporidia.—On this subject, of great interest to fungologists, Mr. M. C. Cooke gives a capital paper in the last number of the 'Quekett Club Journal.' If, he says, we take one of the cup-shaped fungi (*Peziza aurantia*, or *P. vesiculosa*) when mature, in its fresh state, and cut it down through the centre, we shall observe first that the outside and inside of the cup varies somewhat in texture, as well as in colour. That the inside has usually a smooth, delicate, waxy appearance, and, if exposed to the light for a few minutes, little smoky puffs of the minute spores will from time to time be ejected from the surface, as of miniature discharges of fairy artillery. By cutting a thin slice from the cut surface of the section and placing it under the microscope, a good notion of its general structure will be obtained. The slice should be as thin as a sharp knife and a steady hand can accomplish. If this slice be placed in a drop of water, or pure glycerine, on a slide, and covered with thin glass, then submitted to a slight pressure, it may be examined freely with a quarter-inch objective. Towards the inner surface, which is the *hymenium*, numerous transparent tubes, or cylindrical sacs, present their upper extremities, whilst their lower ends coalesce with the cellular substance of the cup. These cylindrical bodies are the *thecæ* or *asci*, mixed with thread-like filaments called *paraphyses*. Sometimes the paraphyses are simple, at others branched, and either attenuated or clavate at their tips. In a few cases the club-like extremities of the paraphyses are coloured, but usually the asci and their contents, as well as the paraphyses, are colourless. In this genus the paraphyses are important features in the determination of species, since they offer considerable variation in different species. What may be their special function has not been satisfactorily determined. Some authors have suggested that they may be barren asci; but this suggestion is far from confirmation by fact, no observations having yet traced the development of paraphyses into asci, or explained why they are so distinct from asci even in the earliest stages at which they can be traced. Similar bodies are also present in the cups of lichens. The asci in their earliest stages are filled with a granular matter, which ultimately is collected (normally) into eight spherical, elliptical, or elongated sporidia, which fill the ascus, and when mature are discharged by rupture at the apex, in little puffy clouds of sporidia, as already intimated.

NOTES AND MEMORANDA.

Death of M. Edouard Claparède.—Owing to the press of various matters for the past two or three months, the notice of the death of this eminent zoologist has been "crushed out" for want of space. His death occurred some time since at Sienna, at the early age of thirty-

nine years. His memoirs, begun in 1857, have been issued with remarkable rapidity. His principal works consist of his monographs on the Infusoria and Annelids. In all his papers, his thorough physiological and anatomical training is perceptible, his details being always discussed in all their general bearing. Living in Geneva, and a pupil of Johannes Müller, he wrote with equal facility French and German: an admirable draughtsman, his many papers, which have appeared in the principal German and French scientific periodicals, are excellently illustrated and wonderfully accurate. His qualities as an original and independent observer are best seen in his larger memoir on the Annelids of the Gulf of Naples, and his observations on the Anatomy and Embryology of the Invertebrates made on the coast of Normandy. His style was remarkably clear and his information very extensive, as is shown from his scientific reviews in the Archives de Genève. Thoroughly independent in his scientific opinions, he never allowed himself to be carried away by weight of authority, and no scientific charlatan protected by eminent names was allowed to pass current; his reviews and criticisms were often sharp, but always just, and never personal. The Academy of Geneva, where he was Professor of Anatomy, will find it difficult to fill the place of one who, in spite of his failing health, showed an enthusiasm for his science rarely equalled.

On two new Lamps for the Microscope.—These lamps, constructed by Mr. E. Richards, F.R.M.S., are designed for the purpose of those who are in the habit of taking their microscopes with them to their friends. The following description will make clear their properties. One is intended to burn benzoline, the other parafine. The receptacles are both of same size: the former is filled with a sponge, saturated and drained off in the usual way, and will burn for three hours; the other is filled likewise with parafine, and will burn for six hours. The stand gives a height of 12 inches, and weighs only 6 oz., and is arranged as follows:—Half of the stem screws into the burner of the lamp, thereby preventing any escape of the liquid or spirit; the other half screws through the bottom of the stand—forming a short pendulum in appearance—convenient for carriage. The lamp is enclosed in a neat case for the pocket. These lamps were exhibited at the last meeting of the Royal Microscopical Society, and gave great satisfaction.

Col. Horsley's Cylinder for Oblique Illumination.—The cylinder is 1½ inch long, and 1¼ inch in diameter, made of brass tube silvered inside. The light is thrown on to it either directly, or from prism or bull's-eye used as a prism. A dark ground is easily got with low angle ¼-inch, and without trouble or loss of time (so say several witnesses) the hemispheres of Angulatum are shown very beautifully. The first apparatus was made by silvering the inside of the tube of the sub-stage which carried the polarizer; this was done by rubbing on it a solution of nitrate of silver, to which hyposulph of soda had been added. This silvers brass with a good surface, and moderately permanent, in a moment.—[This note was received from Dr. J. Miller, V.P.R.M.S.]

CORRESPONDENCE.

Butterfly Scales.

To the Editor of the 'Monthly Microscopical Journal.'

Brussels, *November* 22, 1871.

Sir,—I think it is most important for everyone that he should be able to see through the microscope all that is to be seen, and nothing else.

Having for some months past made a constant use of the paraboloid with immersion objectives of Ross and of Hartnack ($\frac{1}{12}$th Ross, Nos. 9 and 11 of Hartnack), and studied the scales of many butterflies and moths of European, Chinese, and Brazilian origin, I have been struck by the elegance of the beaded appearance those objects presented.

I may thus entirely confirm Mr. S. J. McIntire's views: *those scales have almost all of them a beaded appearance, perfectly defined;* but where I differ totally with this most distinguished Fellow of the Royal Microscopical Society is when he asserts in his paper "On the Minute Structure of the Scales of Certain Insects," read the 9th Nov., 1870, "that those beads have no real existence as *beads*, but are due to the interference of the rays of light by corrugated membranes; that they are, in fact, *ghost-beads*."

Reasoning upon this hypothesis, it is not at all wonderful that Mr. McIntire should have attributed his failures, when "he wished but could not call into existence *those almost palpable beads*," as he calls them himself, so distinctly they are visible, "to a predisposition of his mind, and to the want of that necessary adroitness in manipulating which everyone knows very well is not always at one's own command."

Be that as it may, it must be admitted that his whole reasoning on the matter in hand shows very well that there exists a very serious difficulty of making a proper use of high-power objectives, and of selecting that kind of illumination which is best adapted to the nature of the object which is to be seen, and in particular which is wanted to show the beaded appearance of the scales of the wings of insects.

But all those reasonings seem not to me to be conclusive. One assertion alone, if it were justified by facts, would be very serious; it is that these beads disappear altogether without leaving any traces or remains whatever, when the scale is crushed.

I should beg Mr. McIntire to reconsider this his last assertion, because I am positive that the beads subsist as distinct and isolated things after the scale has been crushed; but it may happen that the scales which he has seen were partially so crushed as to leave in the parts which were crushed no remains at all. Then, but then only, the assertion of Mr. McIntire seems very well justified by facts, but in that it is no conclusion at all.

Now for what I have observed, and the mode of illumination I have made use of.

I pass the light through a paraboloid, of Mr. Nachet's make, of Paris: this glass is similar to the paraboloid of the principal English opticians, but somewhat smaller in size.

When well centred, and in focus, the paraboloid gives a very white light with high power; and every object that comes under the eye is wonderfully clearly and perfectly illuminated; but if the object-glass is not well corrected for the thickness of the glass cover it will at once be apparent, because then you will have no perfect image at all, but an unseemly object, the markings of which will become the more apparent the more you approach the true correction to be made, and then you will see every particle of the object strikingly well defined, and every scale of many, if not of all insects, will appear covered with dots or beads.

I think it was due to an imperfect correction that Mr. McIntire was not always able to see the beaded appearance of the scales.

Here follows a list of butterflies or moths whose scales I have particularly observed, and which wear most clearly the beaded appearance, with nothing more to do to show them but a just correction of the object-glass:—*Grapholytha minutana*, *Pterophorus pterodactylus*, *Platyptilus pilosellæ*, *Alucita hexadactylus*, *Clecophora luctuosella*, *Harpella bractiella*, *Tortrix Hoffmanseggana*, *Vanesse Atalanta*, *Moro-sphinx*, *Crambus*, *Minophora metaxella*, *Vanesse gamma*, *Cosmophora composana*, *Sericosis urticana*, *Liparis chrysorhœa*, *Podalirius*, *Yponomerita malinella*, *Vanesse urticæ*, *Yponomerita padilla*, *Orthosia Gothica*, *Coccyx Buoliana*, *Colyade-Hyale*, *Apatura Iris*, *Agraulis vanillæ*, *Rhétenor*, *Deilephila Elpenor*, *Melanippe*, *Atlas*, *Morpho Helinor*, *Junonia Genoveva*, *Lycorea atorgatis*, *Alcandor*, *Erythonius*, and many more, which are either unknown to me, or whose names are of no scientific nomenclature to be here inserted.

In confirmation of the present letter, I beg the Royal Society to accept a dozen of my slides of butterflies' scales, some of them *selected*. They are of the *Melanippe* (Brazil), *Alcandor* (China), *Deilephila Elpenor*, *Rhetenor*, *Papilio agaris*, *Junonia Genoveva*, *Lycorea atergatis*, *Atlas*. I hope they will be graciously accepted.

Using the same mode of illumination with the paraboloid, I have tried to discover the real nature of the markings of the *Surirella gemma*, that most beautiful puzzle, and I have clearly seen the dots which this interesting diatom wears; they are similar to those of the *Pleurosigma Balticum*, and similarly disposed; that is to say, they make lines which intersect each other at right angles.

This appearance of the *Surirella gemma* must be the true one, because, having tried that same diatom with a double reflector obliquely disposed at right angles to each other, I have obtained the same appearance.

So I cannot sufficiently recommend the use of the paraboloid; it gives a beautiful dark-ground illumination with low power, and with high power a very white one, and its effect is more secure; I should say the true one, in opposition to every other mode of illumination, direct, oblique, with condensers or diaphragms.

Its effect is particularly striking if you observe the flow of the vegetable sap (rotary circulation in plants), as in the *Tradescantia*

virginica or *Campanula sylvestris*; the corpuscles which are seen floating in the fluid have the appearance of little spheres, so clearly defined that it seems impossible that human skill can ever contrive to make an optical instrument which will show more or better than our microscopes when well and judiciously employed; and yet it is evident that there is something more to be seen, because those spheres are not only dragged by the force of the stream in which they are immersed, but they have their own and individual motion; so they must have some sort of propellers as yet unknown and unseen.

I am, sir, yours truly,

CHEVALIER HUYTTENS DE CERBECQ.

THE ANGULAR APERTURE OF TOLLES' ⅛TH.

To the Editor of the 'Monthly Microscopical Journal.'

BOSTON, MASS., *November* 15, 1871.

DEAR SIR,—Permit me to reply to the question of your correspondent B. in the November issue of your Journal. The angular aperture of the Tolles' objective ("nearly ⅛th") used by Dr. Woodward as "immersion" varies from 140°, at "uncovered," to 170°. How Dr. W. used it I do not know. Used "dry" (⅛th) it ranges from 110° to 170°. With another front "system" it is a ¼th 110° up, and upwards. The aperture as well as the power increasing as the lenses are brought nearer together.

A few words may be added on B.'s remarks on aperture. He refers to the "confusion which exists in the minds of most microscopists about the classification of glasses," and conveys the idea, though he does not express it in words, that angular aperture is not taken into consideration. That such is the case with *some* makers, and *some owners* of microscopes, is undoubtedly true; but so far as my experience goes it is not the case with "*microscopists*." With them aperture is as much considered as focal length; and I find that with "microscopists," "focus and aperture are in fact both essential factors in the denomination of an object-glass." This subject was elaborated by Dr. W. B. Carpenter in the first edition of 'The Microscope and its Revelations,' published more than sixteen years ago—a work that is in the hands of most microscopists, and which B. has probably seen. Since that time, however, skilful opticians have made object-glasses of very high angular aperture and retaining greater penetrating power than such as were known to Dr. Carpenter then, and consequently better adapted for purposes requiring that quality than he probably expected they ever could be, and the same glasses retain the advantages derived from the great aperture. All opticians have not progressed in this direction, but some continue to make their lenses as they did twenty years ago.

As a trip to the United States is now a favourite one with Englishmen, I hope that B. will make us a visit. Americans have been reported as having "made some very good instruments," and it will afford

me pleasure to show B. objectives of large aperture with good penetration. Still the fact remains that objectives of very high aperture, if good, are different things from those of very low aperture; and when two glasses are to be compared the angular aperture should be taken into consideration.

It must also be remembered that glasses of great aperture, *if good*, are more costly than those of small aperture; because the proper corrections for sphericity and colour involve, and require, more skill, and a better knowledge of pure optics in the optician, and also more time and more care in the merely mechanical work; so that the cost of a "one-quarter" of 170° is nearly or quite twice that of the same focus of 100°, of equal quality in all other respects.

CHARLES STODDER.

REPLY TO MR. STODDER, "B," AND MR. EDWIN BICKNELL.

To the Editor of the 'Monthly Microscopical Journal.'

WASHINGTON, D.C., *November* 21, 1871.

MR. EDITOR,—There is nothing, I think, in the argumentative parts of Mr. Stodder's letter of July 20th * which calls for any reply from me, but the paragraph on page 203 concluding "all these facts I presume Dr. W. will in due time communicate to the public," demands a few words.

In January last I ordered, through Mr. Stodder, a high-power immersion objective of Mr. Tolles for the Army Medical Museum, "to be constructed with special view to the study of lined test-objects," leaving details of precise power, angle, price, &c., entirely to the maker, and stating to Mr. Stodder that it was my desire "to have in the Museum an objective which you will be willing to have me exhibit as a selected specimen of Mr. Tolles's very best work."

In the latter part of June I had the pleasure of a visit from Mr. Tolles, who brought with him the new immersion $\frac{1}{8}$th referred to in such complimentary terms in my Note on the resolution of *Amphipleura pellucida*.† Mr. Tolles had with him at the same time two immersion $\frac{1}{10}$ths and an immersion $\frac{1}{15}$th, the performance of which was exceedingly creditable; but as it was essentially similar to that of other objectives of his previously referred to by me in this Journal, I limited my published remarks to the one of which I could conscientiously speak as superior in definition to any similar power which I have examined.

At the same time I engaged Mr. Tolles to undertake the construction of a higher power objective for the Museum, which, however, has not yet come to hand. Should it fulfil Mr. Tolles's expectations it will give me as much sincere pleasure as it can to him or any of his friends, and I will promptly make the facts public.

My September paper also contained my determinations of the true

* This Journal, October, 1871, p. 201. † Ibid, Sept., 1871, p. 150.

magnifying power of my Powell and Lealand's $\frac{1}{16}$th when used with its immersion front, so that it now only remains to say a word on the difference which Mr. Stodder refers to between his Nobert's plate and mine. Mr. Tolles had Mr. Stodder's plate with him during his visit; it is mounted on a thick bottom glass, and is a facsimile of one belonging to Dr. Barnard, which served for my early work on the plate. I was therefore perfectly familiar with its characters. On the other hand, the plate now used in the Museum is mounted on a very thin bottom glass, permitting therefore the use of a condenser of short focus and high angle. The chief points of difference between the two forms of plate are given in a printed 'Description of the Nineteen Band Test-plate of Nobert,' issued nearly two years ago from the Museum, with a series of photographs of the lines, and a copy of which is in Mr. Stodder's possession. I am at a loss therefore to understand his remarks in this connection, but may add to what I have heretofore written, that on thick-bottomed plates, like Mr. Stodder's, the spurious lines (counting from thirty to forty in the nineteenth band, for example) are even more deceptively like the true lines than on thin-bottomed plates, and that a count is even more indispensable in the first instance than in the second.

I must also ask of you space for a few words with regard to two communications in your November number. On page 241 a correspondent, who signs himself B., and who seems to write very fairly, asks of me the aperture of the Tolles's $\frac{1}{8}$th mentioned in my Note on *Amphipleura*. I take pleasure in giving the information desired. The lens in question has two fronts—one of moderate angle for use dry. I make its aperture 90° at uncovered, 110° when corrected for cover by one full turn. The other front may be used either dry or wet. When the posterior combination is separated from the front as far as the screw collar will move it, the objective is fitted for the study of dry uncovered objects, and has an angle of 110°. One full turn of the screw collar corrects the objective for the thickest cover through which it will work dry. Its angle is then 140°. Without further change of adjustment a drop of water may be introduced, and it is fitted for the study of uncovered objects wet; another turn and a half may then be given, when it is corrected for the thickest cover through which it will work wet, and has an angle of 170° or upwards. The price paid Mr. Stodder for this objective, including both fronts, was one hundred dollars (United States' currency).

In your November number, also, on page 225, is a paper by Mr. Edwin Bicknell, of Massachusetts, who wilfully misinterprets my published measurements of the wet front of Powell and Lealand's $\frac{1}{16}$th, and thinks proper to use the following words (p. 226):—"I call it nothing more nor less than *deception* in Powell and Lealand (or any other makers) in marking (SIC) an objective nearly 33 per cent. less than its actual power, thus misleading people who cannot make actual comparisons; and I consider Dr. Woodward guilty of an equal amount of deception in *knowingly* putting forth work done by that objective as having been done by a $\frac{1}{16}$th." This language is *vulgar*, *insolent*, and *offensive*, and I should take no further notice of the paper in which it occurs if it did not seem to me that fair play to Messrs.

Powell and Lealand required a full statement of the facts with regard to such of their objectives as I have measured.

The Surgeon-General's Office possesses a number of objectives by the leading American, English, and Continental makers. Among these are the following by Messrs. Powell and Lealand. One $\frac{1}{50}$th and one $\frac{1}{25}$th, both dry lenses. Two $\frac{1}{8}$ths and two $\frac{1}{16}$ths, each furnished with two fronts, one for use wet and one dry. One of the $\frac{1}{16}$ths was originally made for dry work only, and the wet front subsequently added by the makers; the other $\frac{1}{16}$th and the two $\frac{1}{8}$ths were furnished in the first instance with two fronts. The following table gives the magnifying power of each of these objectives without eye-piece at four feet (48 English inches) distance from micrometer screen. The micrometer used was a broken Nobert's plate mounted with the lines uppermost, and the image of the micrometer was projected on the screen by direct sunlight.

Objective.	Magnifying power when uncovered at 48 inches.	Magnifying power at full correction for thickest cover same distance.
Powell and Lealand's $\frac{1}{50}$th dry	2200	3100
" " $\frac{1}{25}$th "	1250	1425
" " dry $\frac{1}{16}$th (No. 1)	790	930
New immersion front to ditto	975	1180
Powell and Lealand's immersion $\frac{1}{16}$th (No. 2) ..	900	1100
Dry front to ditto	770	910
Powell and Lealand's immersion $\frac{1}{8}$th (No. 1) ..	450	500
Dry front to ditto	425	490
Powell and Lealand's immersion $\frac{1}{8}$th (No. 2) ..	460	505
Dry front to ditto	450	510

The above determinations were made with the utmost care, each observation being several times repeated. No eye-pieces or amplifiers were used in order that the conditions might be as simple as possible, and a long distance was taken, that small unavoidable errors in its measurement might have as little influence as possible on the results. The magnifying powers were obtained by dividing the dimensions of the part of the micrometer selected into the dimensions of the image, carefully measured on the screen, and the results therefore fairly represent the performance of the objectives named *under the conditions stated.*

How are these results to be interpreted, and how ought the objectives in question to be named? Mr. Charles Stodder tells us* that "objectives are named when adjusted for uncovered objects, a fact not generally known by purchasers. The power increases, *i. e.* the focus is shorter as the collar is turned to work through the covering glass." But how in the case of a single objective with wet and dry front, or one which by mere change of the screw-collar works from dry to wet? I believe that the commercial practice in this case is to name the objective by its performance at uncovered dry, and this practice

* This Journal, October, 1871, p. 203, *note.*

may be justified by its convenience, as enabling a single trade-name to be given to the combination. Scientific accuracy would seem to demand that the maker should at least state the limits for each combination, but I know of no maker anywhere who has as yet adopted this plan.

Different modes of computing the focal lengths of objectives from their magnifying power under given conditions have been proposed. I have been in the habit of using a very simple rule, *viz.*:—The equivalent focal length of the objective in fractions of an English inch is of course equal to the distance from the optical centre of the objective to the screen, divided by the magnifying power. Since the optical centre cannot be determined with precision in compound objectives, we may substitute, without material inaccuracy, the distance from the micrometer to the screen, provided this be made so great that the unmeasured quantity represents a very small fraction of it. For all objectives from the $\frac{1}{8}$th upwards, therefore, if the distance from micrometer to screen be considerable (say such a distance as I have taken above), the magnifying power at that distance divided into the distance will give the focal length with reasonable precision.

For longer focal lengths, or in any case those who demand the nearest approach to accuracy may use Mr. Cross's formula,* $f = \frac{n\,l}{(n+1)^2}$ where n is the reciprocal of the magnifying power, and l the sum of the conjugate foci or the distance from micrometer to screen. Taking then the magnifying power at dry uncovered as the basis of the calculation, the above lenses of Powell and Lealand may be characterized as follows in decimals of an English inch.

Name of Objective.	Value by the Simple Method.	Value by Mr. Cross's Method.
Powell and Lealand's $\frac{1}{10}$th	·0218	·0218
" " $\frac{1}{12}$th	·0384	·0383
" " $\frac{1}{16}$th (No. 1)	·0607	·0606
" " $\frac{1}{16}$th (No. 2)	·0623	·0622
" " $\frac{1}{4}$th (No. 1)	·1129	·1124
" " $\frac{1}{4}$th (No. 2)	·1066	·1062

It will be perceived that all the above objectives therefore closely approximate, when worked dry uncovered, to what their nomenclature would demand, the $\frac{1}{4}$ths deviating most from the standard, and being in fact, more nearly $\frac{1}{5}$ths than $\frac{1}{4}$ths. It must also be borne in mind that with the most sincere endeavours on the part of the maker, it is not easy to give precisely the same magnifying power to any two objectives, and if any commercial titles are adopted a certain amount of deviation from the standard indicated must be expected.

With regard to the QUALITY of the performance of the above

* 'On the Focal Length of Microscopic Objectives.' By C. R. Cross. Boston, 1870.

lenses I may add the following remarks. Worked with their immersion fronts, I regard the $\frac{1}{8}$ths and $\frac{1}{16}$ths above mentioned as superior in defining power to the dry $\frac{1}{75}$th and $\frac{1}{50}$th of the same makers. This is shown on test-objects, such as the *Amphipleura pellucida*, &c., and on anatomical preparations, as well as on the Nobert's plate. The immersion objectives in question combine, with exquisite defining power, *unrivelled flatness of field* and *depth of penetration*. In all these qualities the two immersion $\frac{1}{16}$ths excel any objectives it has been my fortune to obtain from any source. I make this statement with pleasure, out of justice to the makers, with whom I have no personal acquaintance. I should take still greater pleasure in being able to announce that I had obtained from them, or from any other makers, still better objectives.

Very respectfully,

J. J. Woodward, U. S. Army.

Correction of Dr. Boyd Moss' Paper.

To the Editor of the 'Monthly Microscopical Journal.'

Ceylon, *Nov.* 15, 1871.

Dear Sir,—Kindly supply in your next No. the following corrections to my paper in that for October 1st.

For the "cilia are raised on a substructure of a *cave*-like form," read "*wave*-like."

I was in error, too, in saying that the corpuscles of the blood agreed in size with the musk deer of India, as these latter are only about $\frac{1}{12500}$ diameter, the same as those of the small so-called "moose deer" of Ceylon.

Boyd Moss.

PROCEEDINGS OF SOCIETIES.*

Royal Microscopical Society.

King's College, *December* 6, 1871.

W. Kitchen Parker, Esq., F.R.S., President, in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations to the Society was read, and a vote of thanks passed to the respective donors.

The Secretary called the attention of the Fellows to a small lamp devised by Mr. Richards, which could be easily carried in the pocket; and also to a portable stand to which the lamp could be fitted. The light given by this lamp was sufficient for all ordinary purposes of

* Secretaries of Societies will greatly oblige us by writing their report legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. 'M. M. J.'

the microscopist, and the flame though small was sufficient, with the help of the bull's-eye condenser, for the illumination of most opaque objects. The price with stand was, for the paraffin lamp, 10*s.*; for the spirit lamp, a modification suited to benzoline, 7*s.* 6*d.* The thanks of the meeting were presented to Mr. Richards for exhibiting this lamp.

The Secretary announced that a communication had been received from Dr. Anthony, of Birmingham, relating to the "Markings of the Battledore Scales of the Lepidoptera;" and also from the Chevalier Huyttens de Cerbecq in reference to the minute structure of the scales of insects, and especially bearing upon Mr. McIntire's observations upon this question. The Chevalier's note was accompanied by a present of slides of selected scales.

Mr. J. Beck suggested that when papers were "taken as read" through press of time, an opportunity should be afforded the Fellows of discussing at the following meeting any points of interest that might be found in such papers.

Dr. Beale then read a paper "On the Nerves of the Capillary Vessels and their Probable Action in Health and Disease."

The President proposed, and the meeting unanimously accorded, a vote of thanks to Dr. Beale for the paper which he had read.

Dr. Berkart said he perfectly agreed with Professor Beale on his throwing some doubt on the supposed influence of the nervous system on nutrition. There was a tendency at present to refer almost all pathological changes primarily to the nervous system, without there being sufficient anatomical facts to prove the assumption. There was, for instance, a class of diseases called *Tropho-neuroses*. The substance of the muscular tissue was entirely disregarded, and the atrophy observed in the muscles traced to disease of the nerves. Now he had lately seen a case of atrophy of the right pectoral muscles in a sailor who had been in the habit of pressing the rudder always against the right side of his chest. After death he (Dr. B.) could not find any trace of disease in any part of the nervous system, though the whole muscular tissue was replaced by connective tissue. It was therefore, and especially in this case, more natural to suppose, that the pressure that primarily acted on the muscle itself (the *myoline*) had destroyed it, without the active intervention of the *Trophic nerve*. Though in very many cases of nutritive changes the direct influence of the nervous system could not be made out, there were, however, many physiological experiments which forcibly showed the influence of the nerves on secretion. He would remind the Fellows of the experiments of Claude Bernard, Ludwig, Eckard, and Adrian on the salivary glands. The difference in quantity and quality of the saliva obtained in the various experiments could not well be explained, without allowing the nervous system some share in the cause of it.

Dr. Murie said Dr. Beale had very clearly divided the contents of his paper into two headings; under the first head treating of facts, under the second of theories. With regard to facts, Dr. Beale's name was a sufficient guarantee that what he said might be taken on trust. As respects his theories, more liberty of judgment might be

exercised. Dr. Beale's facts appeared to him very feasible in many points. His reasons for this assertion he would state. Many present were doubtless aware that the capillaries of the larger-sized vessels which formed the *rete mirabile* of the porpoise were of a very remarkable character. In them we had appearances similar to those described in some of Dr. Beale's diagrams, *viz.* contractile vessels of moderate calibre, and minute vessels supplying these; the capillaries sometimes running parallel, sometimes crossing each other at various angles. It was very difficult to comprehend and explain what was the cause of the steady and uniform circulation in these masses of blood-vessels considered in the light of the older ideas on the question. Amongst these capillaries we had also nerves to a certain extent freely intermingled: and when tracing them by ordinary scalpel manipulation, similar ramifications to what had been demonstrated by Dr. Beale in his minute dissections were apparent, wherefore we might look for some nervous influence as the cause of the blood's propulsion by the contraction of the multitudinous vessels of the *rete*. The doctrines nowadays were different to those which obtained in former times, in consequence of the closer study which had been bestowed upon microscopic anatomy. Each organ was found to be a distinct piece of machinery. All present well knew that if the connecting cord of an electrical battery be cut, motion at once ceased at the farther end, and what Dr. Beale had shown reminded him (Dr. Murie) of what obtains and could be readily seen in the body of the torpedo. But before alluding further to the electric organ of that remarkable fish, he would ask Dr. Beale whether, in speaking of the section of the mole's nose, it was a vertical aspect to which he referred? (Dr. Beale explained that he referred to the end of the nose looking down upon it.) That fact then convinced him (Dr. Murie) that the same thing as Dr. Beale described occurred in the nerves and electric battery of the torpedo. In the organ in question there were to be seen a series of hexagonal areas or vertical columns, containing jelly-like or protoplasmic substance precisely similar to what was shown in one of Dr. Beale's smaller diagrams. These in the fish's battery could be discerned with the naked eye; and moreover, when using the microscope, a multitude of delicate nervous twigs was revealed, which ramified between and abutted against the sides of the hexagonal battery cells. Max Schultze and he believed Kölliker among others traced these terminal nerves in the same position as Dr. Beale had shown in the mole's nose. He (Dr. Murie) wished to point out that if there was in the case of the torpedo a vast electrical battery supplied by nervous influence of gigantic power, was it not very probable that the same kind of thing obtained in the arterial capillaries, modified of course to the limited exigencies of their contractile powers? Having regard to the existence of the mole's nose, he (Dr. Murie) was disposed to agree with Dr. Beale in the demonstration he had given of the subject in general. The theoretical portion of Dr. Beale's paper was not altogether "plane sailing." He believed, however, that the doctrines enunciated did explain very many things both of a pathological as well as physiological character.

He was inclined to agree with Dr. Beale, that nerves did not enter those epithelial tissues when the epithelium is thrown off continuously; for in such a case the nerves would be unfavourably exposed, and undoubtedly placed in a dangerous position, to retain their normal stimulus. Dr. Murie, in conclusion, eulogized the paper just read as one of those valuable contributions to microscopic anatomy unfortunately too seldom laid before the Society, where, according to the number of its members, such practical researches ought to be the rule and not the exception.

Mr. Stewart said he wished to mention, in reference to the question of the existence of nerves amongst epithelium cells, that having recently had the opportunity of examining some very beautiful specimens of the cornea, prepared by Dr. Klein, he was firmly convinced of the existence of a fine plexus of nerves between the cells of the conjunctival epithelium, directly continuous with the coarser plexus of nerves situated in the middle layer of the cornea.

With regard to the influence of nerves on secretion, he thought that the altered character of a secretion when the nerve supplying the gland was severed or irritated, could not be entirely explained by the influence it would have upon the blood-vessels. There could be little doubt that there was a direct influence upon the cells, but the question was, how that influence was exerted. Admitting the close analogy between nerve force and electricity, to a certain extent some of the phenomena observed might be explained by the example of a capillary tube, which retained water under ordinary circumstances, but if, when full, an electric current be passed through it, the water immediately flowed out: in dialyzing membranes also, the character and quantity of the fluid which passed was modified if an electric current was passed through the membranes.

Dr. Beale asked whether the section Mr. Stewart had seen was transverse or not.

Mr. Stewart said it was not. The fibres seen could be traced down through the various forms of plexus most distinctly.

Dr. Beale: What was the specimen mounted in?

Mr. Stewart: Glycerine.

Dr. Beale: I should like very much to see a specimen in glycerine; I have never been able to satisfactorily develop the nerves in the conjunctival epithelium.

Mr. Stewart: The surface of the epithelium could be distinctly discerned, and from thence the fine nerves could be traced down until they joined the nerve situated in the middle layer of the cornea.

The President asked Dr. Beale whether he meant to deny altogether the contractility of the capillary vessels.

Dr. Beale said the exception occurred in the case of young creatures, such for instance as the tadpole, whose tail was contractile.

Dr. Lawson said he thought the gratitude of the Fellows was due to Dr. Beale for the paper he had read. He considered it one of the most valuable papers which had been brought forward on the subject for the past five or six years. He thought that their views of microscopical anatomy had been very much advanced by it. So far as he

could judge, the works written by foreigners had not gone into the matter so far as Dr. Beale had gone in observations upon the nerves. Their glasses had not the power which Dr. Beale had brought to bear upon the subject. He was for this reason disposed to give a distinctive prominence to Dr. Beale's researches. He had advanced considerably beyond what had been done on the Continent, and especially in regard to the distribution of nerves upon the blood-vessels. He would just ask, in reference to the small particles described as projecting from the edge of the capillaries, whether those small particles might be converted into blood corpuscles. On another point, he would mention in regard to the capillaries in the frog's foot, that he quite agreed with what Dr. Beale had said. A great number of experiments had been made in regard to the effect certain substances produced on the capillary circulation of the frog, and he (Dr. Lawson) had gone over them himself invariably with the same results as Dr. Beale had shown; *viz.*: that the effect on the blood-vessels was due entirely to the action of the nerves, and not to the influence of the substance employed in the experiment.

Mr. Hogg said perhaps it might not be known to every member of the Society just how the question in debate stood. The subject, although a moot one, was a very interesting one. For years it had been a matter which greatly puzzled physiologists, to know how the blood was propelled from the capillaries back again into the circulation. It had been attributed in a great measure to the action of the heart, but it was very well known that amongst the *invertebrata* many have no heart at all. There we had creatures that propelled their blood from some cause that we could not explain. There was also in vegetable forms a circulatory movement that went on continually without any apparent nervous structure, or without any centre of motion whatever. It was reasonable to suppose that the circulation in minute capillary vessels was carried on in a similar manner. It was known that the rate at which the blood moved through the capillaries was about 2 inches per minute. The question as to how this circulation was carried on was a very important one, having special bearings upon pathological subjects, such, for instance, as inflammatory action. He thought Dr. Beale had done good service in treating as he had done on the nerves of the capillaries, because so far microscopists had not been able to discover any contractile power in the capillaries at the adult stage of existence. He wanted much to know how the blood was squeezed out of the capillaries and carried back into the veins again. He had undertaken some few years ago a series of investigations, on behalf of the late Mr. Guthrie, into the nature of those capillaries, Mr. Guthrie being at the time much interested in the structure of non-striated muscular fibre, and he (Mr. Hogg) well remembered the fact of having passed over the nerves in connection with their structure. He had attributed the swellings observed to swellings in the non-striated muscular fibre, entirely ignoring the presence of nerves. Dr. Beale had mentioned that he believed that the nerves acted upon the muscular fibres rather than upon the walls of the capillaries. Perhaps that was so. He, however, should think there was direct action upon

the capillary vessels. Then with reference to the cornea, where we had a beautiful example of nervous structure; nerves existed there probably for the purpose of preventing the entrance of blood into its structure. Since it had not been demonstrated that there was contractile power in capillary vessels, it was reasonable to suppose that the nerves acted in the manner indicated by Dr. Beale.

Dr. Leared asked whether Dr. Beale had extended his observations to the minute vessels of the brain, and whether he had been able to make any deductions in regard to the ordinary but not satisfactorily explained phenomenon of sleep. He thought Dr. Beale's views would throw great light upon the question of sleep, and also as to the action of certain drugs, such as *bromide of potassium* (which probably consisted in their power of controlling the cerebral circulation), found so effectual in restraining epilepsy and allied affections.

Dr. Beale, in answer to Dr. Leared, said he had not been able to demonstrate conclusively the existence of nervous fibres in connection with the capillary vessels in the brain, up to the present time. He thought the turtle's brain would be the best one to study with reference to this question.

Dr. Beale then said, with regard to the remark of Mr. Hogg as to the direct action of the nerves on the capillaries, that in his opinion it was doubtful if nerves acted directly upon capillaries, or upon the elementary cells of secreting glands. He should explain the action as reflex. It would be difficult to account for direct influences until some theory had been propounded to explain the way in which the nerves might be supposed to act upon cells and non-contractile tissues. He did not see how we could admit the doctrine.

The remarks made by Dr. Berkart concerning Eckard's observations on the salivary glands were to the point, but he thought that, after all, change might be due to the influence exerted by the nerve fibres in the small arteries. He did not think any explanation yet offered wholly satisfactory.

Dr. Murie had in the course of his remarks advanced the opinion that many nerve organs were like electrical apparatus, and in this opinion he fully concurred. He (Dr. B.) differed from many of those who brought forward hypotheses of nervous action, and he was quite prepared to admit that nervous action generally might be really electrical action. Admitting this, however, it must be borne in mind, that there was a great difference between the electrical machine and the nerve machine of the organism. The first was made by man, the other made itself, or was evolved as the saying was. He thought Dr. Murie quite correct in the argument he had adduced with regard to the structure of nerve organs being like the battery of the torpedo. Many other animals possessed forms of apparatus which undoubtedly did produce electricity in the organism, and it was only reasonable to infer that other forms of apparatus exhibiting the same type of structure produced electricity in smaller amount. The way in which the circulation was carried on was very much the same in the highest as in the lowest animal; and so it was probable that with regard to the distribution of the nerves a general agreement in fundamental

character would be found to obtain. All facts bearing upon this general question were undoubtedly of the greatest interest, and too much time could not be devoted to their elucidation. He did not feel disposed to agree with the views now in favour and supported by Mr. Stewart with reference to the termination of the nerves in the epithelium. He was ready to be convinced on seeing specimens, but those he had examined were not satisfactory, nor had those who entertained these views shown how the nerves reached the ultimate points to which they traced them. Did the nerves get into the epithelium when it was young, or wait till the epithelium was formed? In discussing the subject a great number of difficulties would present themselves in the way of giving a feasible account of the distribution of nervous fibres in the epithelial structure. He thought many of the drawings now made in Germany open to very severe criticism. For instance, there was a paper on this question by Schöbl, considered by many who had seen it a very elaborate and beautiful memoir on the distribution of the nerves of the bat's wing. He thought that Schöbl must have drawn upon his imagination, or had trusted the designing as well as the execution of the beautiful lithographs accompanying his paper to an artist ignorant of structure. He (Dr. Beale) had worked at the subject for a long time, and he would defy anyone to prove that the nerve fibres in the trunks of the nerves ran in a direction parallel to one another, as represented in all Schöbl's drawings. He challenged anyone to show him the smallest portion of nerve fibre in which the nerves lie parallel to one another, as represented in the drawing, say for a distance of only the fiftieth part of an inch. No one who had carefully examined specimens in which the nerves were clearly demonstrated would have permitted them to be delineated in the way represented. Everyone who had dissected nerves knew that they did not lie one beside the other; for if that were so, nerves would tear longitudinally. He thought that those who undertook the office of critics should examine the published representations of different anatomical observers in this country, as well as abroad, and carefully compare the various conclusions before they committed themselves to a doctrine which within six months might be supplanted by another of which they would speak with equal confidence and equal favour. They should see the specimens which have been prepared by observers who have worked at the subject, and in different ways; and when possible, the critics should make themselves acquainted with the points at issue, and endeavour to represent, without favour, the views of different observers, instead of, as is too often the case, siding with one, and endeavouring to carry the opinion of others one particular way. In England the views of foreigners had, he thought, a very undue advantage; they were accepted and excessively praised, while many English critics scarcely noticed the conclusions of English observers. He alluded to the subject because the general indifference shown in certain quarters to the investigations of students in this country discouraged the study of minute anatomy here. He would be happy to show his specimens to anyone who would be willing to devote an hour or two to their investigation.

The meeting was then adjourned to the 3rd January, when W. Carruthers, Esq., F.R.S., will read a paper.

Donations to the Library and Cabinet, from Nov. 1st to Dec. 6th, 1871:—

	From
Land and Water. Weekly	*The Editor.*
Society of Arts Journal. Weekly	*Society.*
Nature. Weekly	*Editor.*
Athenæum. Weekly	*Ditto.*
Spot Lens for Low Powers	*Mr. Chas. Baker.*
Journal of the London Institution, No. 8	*Institution.*
Quarterly Journal of the Geological Society, No. 108 ..	*Society.*
Two Slides of Micro-ruling on Glass. By John S. Stanistreet, Esq.	*Author.*
Twelve Slides of Insects' Scales	*Chevalier H. de Cerbecq.*

The following gentlemen were elected Fellows of the Society:—

Joseph F. Payne, B.A. Oxon.
John Rogers, Esq.
Charles H. Roper, M.R.C.S.
Charles Croydon, Esq.

WALTER W. REEVES,
Assist.-Secretary.

BRIGHTON AND SUSSEX NATURAL HISTORY SOCIETY.

October 12th.—Ordinary Meeting. Mr. W. M. Hollis, President, in the chair.

Messrs. E. H. Moore, A. Creak, J. Cobbett, and Helmsley, were elected ordinary members.

Mr. Wonfor announced the receipt of 'The Cruise of the Norna,' by Mr. Marshall Hall, from the author, to whom a vote of thanks was given.

Mr. C. P. Smith announced the discovery of an umbelliferous plant, new to Britain, near the Race Hill. It was not only a new plant, but a new genus and species as well.

A paper by Dr. Stevens, of St. Mary Bourne, "On the Flint Works at Cissbury," in the absence of that gentleman, was read by Mr. Wonfor.

After alluding to the peculiar botany and zoology of the South Downs, and pointing to the evident connection existing between some of the highest peaks, both in a geological point of view and also as positions likely to be occupied, either as camps of defence at one time, or as a series of encampments to keep down a subjugated race, at another, especial attention was directed to Cissbury, as bearing marks not only of having been occupied as an entrenched camp during Roman days, as evidenced by implements, pottery, &c., found in and about the sixty acres enclosed within the ramparts or entrenchments, but to its having been what might be called the "*Flint* Sheffield of Sussex," where an early people fashioned rude flint implements, and where they or a later people, used a very rude kind

of pottery. Colonel Lane Fox found about 600 chipped implements of different kinds, which he considered might be referred to the *neolithic* period. Upon a recent visit, in company with Mr. J. P. M. Smith and Mr. Wonfor, he had taken away 100 wrought flints, many of rude type, but all exhibiting manifest marks of design. Some were well shaped, especially some lance heads, while flakes, picks, slingstones, and edged tools, abounded in the circular pits, and on the surface of the encampments. Nowhere had he found the rubbed implements, which are not rare in Hampshire. The implements recently thrown out of the pits showed fresh surface patination, while those scattered about the hill, having undergone long atmospheric action, showed a change of colour, to the depth, in some cases, of a quarter of an inch. The question how the occupants of the hills could obtain water, their possible hunting grounds, together with the cattle probably kept by them, were next discussed, and reference was made to a Roman building and a subterranean trench and caves beneath it, occupied by a flint-using people, lately excavated by him at Finkley, Hants.

October 26th.—Microscopical Meeting. Mr. W. M. Hollis, President, in the chair.

The subject of the evening, "The Scales of Insects," was introduced by Mr. Wonfor.

By the term "the scales of insects," was understood those epidermal appendages found on the head, thorax, legs, and abdomen of some insects; on the wing cases or *elytra* of some beetles; and on both surfaces of the wings of butterflies and moths; to which they gave that wonderful beauty of colour which rendered them objects of admiration even to the non-entomologist.

In general terms, scales might be designated as flattened hairs; though, in examining the multitudinous forms and varieties found among insects, every stage between the round hair and the purely flattened scale might be found. The analogy to hairs was also seen in their horny character.

Taken as hairs, they seemed to present three different types: one very common one being those in which there was an upper and lower surface, more or less rugose, striated or wrinkled, with an inner structureless membrane between them, which seemed to act as a kind of foil to throw up the brilliant colour of the scales. In another type, the upper and under surfaces, without the inner structureless membrane, were welded together; for, where the scales were broken or damaged, no trace of the intermediate membrane could be detected. In a third type, the scales were more or less round, sometimes tasselled, and devoid of the inner membrane. In these last, and possibly also in the first type, there seemed to be a power on the part of the insect of inflating or puffing out the scales, so as, possibly, to render them more buoyant. Certainly these scales, when taken from fresh-caught specimens, were rounded, but, when pressed between glass, they became flattened.

Each scale was inserted in the wing membrane by a stalk, and here, especially among the lepidoptera, the creatures appeared able not

only to inflate, but also to raise the rows of scales. Some of the "tasselled" scales, beside their points of insertion, had also a kind of ball-and-socket movement. When scales were taken from recently-caught specimens, a slight pressure of the covering glass caused an oily substance to ooze from the point of insertion; whether this was pigmentary matter or a circulating fluid, was doubtful.

Considerable difference of opinion prevailed as to the markings on scales, and the learned were at issue on this point. It would seem there were several types. In some, longitudinal striæ or ridges were crossed at intervals by transverse striæ; in others there were longitudinal ridges with puckerings or wavings of the membrane between them. Some had urged that the longitudinal striæ were of a beaded character, and in some scales the lower beads could be seen through the structureless membrane. He had found that, when either the upper or under surface was torn or damaged, the so-called under beads could not be seen, leading to the idea that they were only spectral reflexions of what had been termed surface beading. While it was difficult to disprove the beading and to demonstrate the continuous ridges in the Thysanuridæ, they might be well seen in some of the large and well-marked scales from the larger foreign and English butterflies and moths. Some markings were evidently due to pigment, which in many cases presented quite a granular appearance.

He was prepared to exhibit and illustrate what he had advanced by a large collection of scales, *in situ* and separated, and some of the larger ones broken up to show structure; and by the kindness of some London microscopical friends, Messrs. T. Curties, McIntire, and Marshall, he was also able to show some very choice test-scales from Thysanuridæ, Poduridæ, &c.

The meeting then became a conversazione, at which a number of very beautiful objects in illustration of the paper were exhibited by Dr. Hallifax, Messrs. Glaisyer, R. Glaisyer, Sewell, C. P. Smith, and Wonfor.

Mr. C. P. Smith exhibited a very compact, cheap, and handy portable microscope, by C. Baker, of Holborn. This instrument, which was much admired, possessed a revolving stage so contrived that the object under examination always remained in view during revolution. This was an important adjunct, and hitherto had only been applied to the more expensive form.

Mr. J. Robertson exhibited a wing of *Sirex gigas*, showing the spines.

South London Microscopical and Natural History Club.

An Ordinary Meeting of this Club was held on Tuesday, October 17th, at Glo'ster Hall, Glo'ster Place, Brixton Road. Mr. Deane, F.L.S., presided.

Dr. W. M. Ord read a paper "On Polyzoa," of which the following is an abstract:—

Polyzoa are microscopic animals that one finds tolerably abundantly both in fresh-water ponds well shaded by trees, and on all sorts

of fixed structures at the sea-side. The fresh-water polyzoa, which you will encounter readily about here, will be perhaps those most familiar to you, and so I propose to introduce one of these fresh-water kinds as a type of the rest. All this great fungus-looking mass is composed of a number of animals congregated together; this was captured in Mr. Thrale's lake on Tooting Common. If now you look at the hard structure, you will see that it presents at all points little cavities, which are the openings of long tubular cells into which the softer substances are retracted. These cells are lined by delicate animal matter; and this matter projects beyond the mouth of the cells in the form of a soft tube of a somewhat conical shape. The cone, at its termination, suddenly expands into a structure called the *lophophore*, or plume-bearing structure. In most of the familiar forms, this is in the shape of a horse-shoe placed flat, and along the sides of this horse-shoe run two series of tentacles parallel to one another. These expand into a beautiful crown, which may be compared to the body of one of the old-fashioned chariots. On the disk where the lophophore is attached, we find the commencement of the alimentary canal—the mouth—enclosed by the arms of the lophophore, and lying between the double row of the tentacles. On viewing the living animal you will see that these tentacles by means of their cilia cause little whirling currents, so arranged as to bring solid matters suspended in the water down into the hollow cup, and thence into the mouth; from the mouth they go to the *pharynx*, which is richly fringed with cilia, and thence to the stomach. In the walls of the stomach we find usually a number of little cells, containing dark-brown matter, which are supposed to discharge the function of a liver. Surrounding the alimentary canal is a cavity called the *perivisceral* cavity. This contains a fluid, which is all the creature has to represent blood; this fluid circulates in all parts of the body. As to their respiration, they draw oxygen from the water around them; this is done in part by their tentacles, also by drawing in water, with regular alternating actions of suction and ejection. With regard to the organs of reproduction, one may note that these creatures reproduce themselves in at least three ways. First, by eggs, the result of the impregnation of ova with spermatozoa; the ova undergo their development within the body, as far as can be made out. The embryo is first a hollow sphere; a layer is then thrown off from the surface at the same time that an opening is made in the wall of the sphere; a second sort of little sphere is thus formed within the first, and here little polypides are gradually developed. A second way of reproduction is that on the side wall at some point or other a projection occurs, and grows to a tube in which all the parts of a new polypide are by degrees developed; and by a third form of development, we find towards the latter part of the autumn dark, flat, oval bodies forming themselves. They are composed of two slightly convex plates like watch-glasses placed face to face, with germinal matter inside. These eggs live through the winter from the autumn, nearly all the composite structures dying as winter comes on. These bodies receive the name of *statoblasts*, from the idea that they remain in the dead organism silently awaiting the spring. Their forms

are very varying and beautiful, and some of them are most lovely microscopic objects.

A word now as to the systematic position of the polyzoa. The more we know of their homologies, the more reason there is to class them with the lower class of mollusks—those called the *brachiopoda*. It is suggested that we can join the brachiopods and polyzoa, just as among the *tunicata* we find isolated forms as clearly as possible connected with forms as social and interdependent as are the polyzoa. I will now say a word, for the benefit of those who know perhaps little about the subject, as to the zoological position of these polyzoa. They seem first to have been noticed by the celebrated Abbé Trembley, the same who taught us about the little fresh-water hydra; how it may be cut in pieces and otherwise mutilated without any apparent harm, and how each fragment grows for itself the parts which have been removed. In investigating these undoubted polyps, he discovered some forms which he called the *polypes à panache*, or plumed polyps. Some years after, Ellis took them up, and in his system they were still associated with the ordinary polyps. In 1828, Milne Edwards, the French zoologist, first pointed out their distinct homologies with the tunicata, and since that time they have been put at the lowest part of the sub-kingdom mollusca. But while general consent classes them with the mollusca, some reasons seem to exist for classing them with the worms; Dr. Strethill Wright having discovered a fixed marine *annelid*, which had a kind of horse-shoe plume at its free extremity; still, when we take the great resemblance of the adult polyzoa to the structures of the brachiopoda and tunicata, we may be satisfied in our minds of the wisdom of placing this class among the mollusks, at the bottom of the list of the classes included in that sub-kingdom.

Dr. Braithwaite observed that many of the specimens exhibited had the tentacles expanded. He would be glad to know how this was managed.

Mr. Stewart said he had succeeded in keeping them out by adding a few drops of the best French brandy to the water in which they were living. They seemed to appreciate this beverage so highly that they were overcome by the liquor, and died with the plumes expanded. They could then be mounted in the ordinary way.

A vote of thanks was unanimously accorded to Dr. Ord for his interesting paper. The President announced the first soirée of the club, to be held at the Horns Assembly Rooms, Kennington, on Thursday evening, November 30th; also a paper for the next meeting, on Tuesday, November 21st, by Mr. Jackson, "On the Barks of Trees."

An Ordinary Meeting of this Club was held on Tuesday, November 21st, at Glo'ster Hall, Glo'ster Place, Brixton Road. Mr. Deane, F.L.S., presided.

Mr. Jackson, of the Kew Museum, read a paper "On the Barks of Trees," of which the following is an abstract:—

I propose to-night to say a few words upon the variable structures and characters of a few of the most remarkable barks of foreign trees. I would remind you that in *exogenous* structure, to the con-

sideration of which I shall confine myself, there are three distinct series or layers; first, the inner bark or *liber*, called the *endophlœum*, which is composed of long, fibre-like cells; secondly, the cellular portion or green bark, called the *mesophlœum;* and thirdly, the corky envelope or *epiphlœum*, which is sometimes very thick, as for example the cork of commerce, which is certainly a rather unusual development of the outer layer. From the inner barks are derived most of the fibres used for making into cordage, matting, or similar articles. One of these barks, the Lace Bark of Jamaica—*Lagetta Lintearia*,—is exceedingly beautiful and interesting, and it is moreover useful to the natives of the West Indies for many economic purposes. It is composed of a series of concentric layers of very fine and strong fibres, which, by crossing and interlacing each other, form a complete network, the beauties of which are quite hidden till the bark is beaten out, and the fibres partially separated by carefully pulling them in a lateral direction, when a piece of vegetable lace, a yard or more in width, will be produced. This natural lace is used in Jamaica for making caps, hats, collars, frills, &c., first being bleached by sprinkling with water and exposure to the sun. It is said that Charles II. was presented by the then Governor of Jamaica, with a pair of ruffles and other articles of dress made from this lace bark, and also that, in former times, the whips used for flogging slaves were mostly made from this bark. The bark of the Paper Mulberry of the South Sea Islands is another of the fibrous kinds; it is very strong and tough, and is used in the Pacific Islands for making what is called tapa cloth, which serves the natives for various articles of clothing. Another remarkable fibrous bark is the *Antiaris Saccidora*, called the Sack Tree in Western India and Ceylon. The bark of this tree is used for making sacks, hence its common name. A trunk is selected of the requisite diameter, and a piece is cut off, of the required length; the bark is then soaked and beaten, loosened from the wood, and turned back or inside out; if it is entirely stripped off, it requires simply to be sewn up at one end, but it is usual to leave a small piece of the wood to form the bottom. In the Natural Order *Myrtaceæ*, some very variable bark structures occur, for instance, in the Stringy Bark Tree of Tasmania—*Eucalyptus gigantea*,—which is toughly fibrous or stringy, while in the Iron Bark it is of such a compact solid nature, and so hard, that it might be taken for a close-grained wood, rather than a bark. Another very remarkable bark is that of the Pottery Tree of Para. It is the *Moquilea utilis* of botanists, and is a large straight-growing tree. A microscopical examination of the bark shows all the cells of the different layers to be more or less silicated. The name of Pottery Tree has been given to this plant in consequence of the uses to which the Indians apply the bark for making into a kind of earthenware. The bark is burnt, and its ashes mixed with clay, in proportions varied at the will of the operator. All sorts of culinary articles and cooking utensils are made from it; they are very durable, and will bear any amount of heat. Having now brought before your notice a few barks of very dissimilar structure, I will leave the matter in your hands to work

out more fully. I have mentioned only a few of the most interesting and peculiar barks that have come under my notice in the course of my duties at Kew; but I am persuaded that if microscopists would examine them more carefully, and devote even a portion of the attention to them that they have given to woody structure and to diatoms, some beautiful objects would be the result, and our knowledge of these matters would be considerably increased.

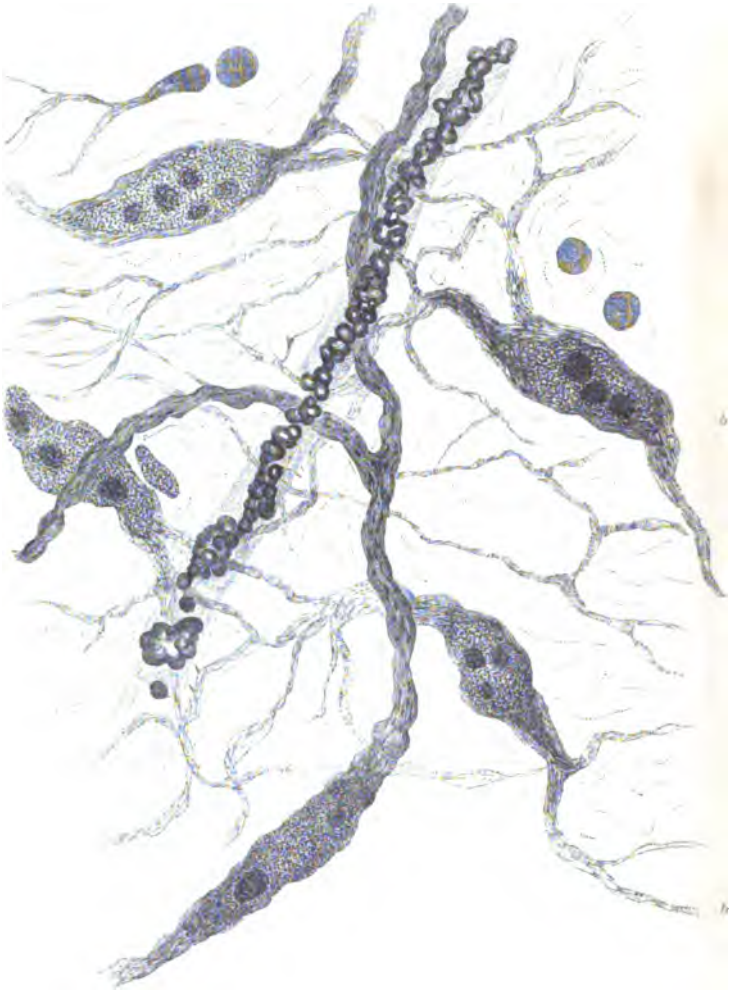
A vote of thanks was unanimously accorded to Mr. Jackson for his interesting paper.

Mr. Suffolk said, that although Mr. Jackson seemed to have hinted that the subject of barks would furnish little of microscopical interest to the members, he could assure them, from personal experience, that the study of the *liber* cells was most interesting. Shortly after the formation of the Quekett Club, he was appointed on a committee for the investigation of the fibres of flax and hemp, with a view to their discrimination in mixed fabrics. The committee did some little work, but the inquiries were ultimately abandoned, the fibres being so much alike that it was impossible to distinguish them under the microscope. He advised those persons who examined these fibres not to neglect the use of polarized light. In the hope of obtaining some further knowledge as to the structure of the fibre, he had immersed it in a solution of copper in ammonia, but found that the fibre was too rapidly destroyed. The use of nitric acid brought into view some secondary deposits, principally spirals; but, by simply placing the flax fibre under a thin glass with a little turpentine, and applying a power of 100 to 150 diameters, with polarized light, these structures can be plainly seen. Mr. Suffolk remarked that in his cabinets there were fifty specimens of fibres, supplied to him by the Indian Government, duplicates of which were to be found in the cabinet of the Royal Microscopical Society; and, in conclusion, he said that he brought forward this subject to show that it contained much of microscopical interest, and he could assure Mr. Jackson that his lecture was most welcome to the members.

Mr. James Love exhibited a new triangular prismatic aquarium, of his own invention; and explained, by means of a small model, the principles of its construction.

It was moved by Mr. Newman, seconded by Mr. Cottrell, and carried unanimously, "that the hour of meeting be in future eight o'clock, instead of half-past seven o'clock in the evening."

The President announced a paper for the next meeting, on Tuesday, December 19th, at eight o'clock in the evening, by Mr. J. Traill Taylor, "On the Photographic Delineation of Microscopical Objects."



...fine nerve-fibres with connective tissue corpuscles and pigment cells, ramifying in the connective tissue at the base of the heart of the frog. *a* Nerve-fibre with nucleus. Its relation to the connective tissue corpuscles, *b* and to the processes of pigment cells, *c*, is well seen. The fine nerve-fibre, less than the $\frac{1}{1800}$ of an inch in diameter, is in continuous with any of these structures. It passes near them, but is not incorporated with them. All are developed *pari passu*, but are structurally independent.
× 2800. 1862.

RELATION OF FINE NERVE-FIBRES TO PROCESSES OF PIGMENT CELLS OF THE FROG.

THE

MONTHLY MICROSCOPICAL JOURNAL.

FEBRUARY 1, 1872.

I.—*On the Relation of Nerves to Pigment and other Cells or Elementary Parts.* By Dr. Lionel S. Beale, M.B., F.R.S., Fellow of the Royal College of Physicians, Physician to King's College Hospital.

(*Read before the* Royal Microscopical Society, *Dec.* 6, 1871.)

Plate VI.

The tendency of opinion in these days seems to be in favour of the conclusion that the finest branches of the nerve fibres come into structural relation with the active elements of other tissues. Many profess to have traced nerves into epithelial cells. A number of authorities agree in asserting that the ultimate nerve fibres come into actual contact with, and are probably in very close relation with the contractile tissue of striped muscle. Not a few assert that the nerve fibre may be traced into the nucleus of unstriped muscular tissue. Kühne concluded that the nerve fibrils were continuous with the prolongations of the connective-tissue corpuscles of the cornea, and M. G. Pouchet published a paper in the December number of the Journal with the object of convincing us that fine nerve fibres were continuous with—in fact ended in the pigment cells (chromoblasts) of the skin of fishes.

Upon the other hand, my observations have led me to the general inference that in no case do the finest terminal ramifications of the nerve fibres end in the manner described; and whatever may be the nature of the influence produced by the nerves upon the structure, and the action of various tissues and organs, I do not think that it is dependent upon continuity of substance between the nerve and the tissue affected.

From what I have seen I feel confident that at least in many cases the contraction of a muscular fibrilla depends upon a change in the nerve which runs *near to* it, but is distinctly separated from it—upon such a change as a varying intensity in the electrical current traversing the nerve fibre might occasion; and when contraction of protoplasm (bioplasm) follows upon irritation of nerve fibres, I believe the result is also due to the same circumstance, and

not to any direct influence propagated by the nerve to the protoplasm (bioplasm) by reason of continuity of material.

The drawing in Plate VI. will serve to illustrate the only conclusion I can accept at this time with reference to this interesting question. Although I publish only this one drawing, the fact has been observed in many instances, and in many different animals. In no case have I been able to satisfy myself that the nerve ends in the manner M. Pouchet describes, and which accords with the conclusions arrived at by some other observers who have studied somewhat similar structures. I believe, however, that upon this matter my own conclusions as regards the non-continuity of the nerve fibres with the cell are in accordance with the more recent observations of Dr. Klein, though this observer describes a plexus of excessively fine nerve fibres upon or over the protoplasm (bioplasm) of the corneal connective-tissue corpuscles, as demonstrated by the use of chloride of gold, but which is not seen in my specimens prepared according to a different method of investigation.

As far as I have yet been able to extend my inquiries I feel confidence concerning the results arrived at. I am quite certain of what I have described, and equally sure I can demonstrate the facts to others. That there may be still finer nerve fibres is of course quite possible, but for the present I prefer to discuss the bearing of what I have myself been able to demonstrate conclusively, rather than to reason upon the observations of others, more especially as the facts I have adduced have in nearly all cases been confirmed by a number of observations upon different tissues in different animals, and at varying periods of life.

The drawing illustrates the appearances observed in the neighbourhood of one of the prolongations of a solitary pigment corpuscle in a beautiful specimen of the delicate fibrous membrane from the abdominal cavity of the Hyla, or green tree frog. The portion of tissue under observation displayed the various points represented with great distinctness, and though it was magnified by the $\frac{1}{50}$th of an inch object-glass, magnifying nearly 3000 linear, made by Messrs. Powell and Lealand, so thin was the specimen that little difficulty was experienced in getting clear views of every part.

The branch of the pigment cell (*c*) is seen in the central part of the drawing, and the granules of pigment suspended in the transparent fluid material flowing in the tubular cavity are represented. The nerve fibre (*a*) prolonged from the elongated nucleus (bioplasm of the nerve) is seen to divide into two branches, one of which crosses the pigmentary process, while the other pursues a course for a short distance parallel to it. If, as often happens, a fine nerve fibre runs very close to one of the processes for a short distance, and is then lost to view in consequence of passing behind the body of the cell, and is perhaps hidden by a thicker portion of fibrous tissue,

or by another pigment cell, an appearance leading to the conclusion that the nerve fibre became continuous with the cell, and terminated in it, would be produced. Excessively delicate points of this kind cannot be determined unless specimens of extreme tenuity are examined, and mounted in fluid in which their position can be changed. It is physically impossible to spread such very thin preparations perfectly flat in any limpid fluid, and they can only be manipulated with success in a viscid medium like strong syrup or glycerine. In good specimens in these media we may, however, often follow nerve fibres less than the $\frac{1}{100000}$ of an inch in diameter for a long distance. The edges of the delicate nerve fibre often appear sharp and well defined, and at certain intervals the nuclei of the fine fibres forming what I believe to be the terminal or ultimate nerve net-works, or plexuses, are seen. These bioplasts or nuclei are usually situated more or less on one side, so that the greater part of the nerve fibre is placed upon one side of the nucleus. With regard to the nerve fibrils themselves, many (I believe, all) are unquestionably compound, consisting of still finer fibrils, which are arranged according to the same plan as the nerve fibres in the larger trunks. The constitution of these very fine nerve fibres can often be well made out in my specimens (but not in gold or osmium preparations) at the point where one divides into two or more branches (see Pl. VI.). There are few questions more worthy of being thoroughly prosecuted than this, for by studying carefully the course pursued by the fibres in nerve trunks of various sizes, we cannot fail to ascertain facts of the highest importance, which will greatly influence the conclusions deduced concerning the ultimate distribution and arrangement of nerves. This will in time necessarily lead us to more trustworthy reasoning concerning the nature of nerve action, and the precise way in which some other tissues are influenced, governed, or regulated by the instrumentality of nerve tissue, than will be gained by *assuming facts* concerning nerve structure, and then determining what according to the idea advanced must be the nature of nerve action, as some who have been considered philosophical have ventured to do.

II. — *Report on Slides of Insect Scales.* Sent to the Royal Microscopical Society by the Chevalier de Cerbecq, accompanied by a letter. Examined by Henry J. Slack, Sec. R.M.S.

(*Read before the* Royal Microscopical Society, *Jan.* 3, 1872.)

The objects on slides sent by the Chevalier appear to be mounted in balsam, by which many are rendered so transparent as to require great care in illumination. His intention in forwarding them was to show that they confirmed the idea that a beaded structure was a reality, and not a mere optical illusion. On several slides the scales are arranged in star patterns, so that an illuminating ray from any direction will fall upon them at different angles.

The following examinations were made with Powell and Lealand's new $\frac{1}{8}$th, Ross' C eye-piece and $\frac{1}{10}$ths condenser, stopped to an angle of 75°, and employed with a single radial slot giving unilateral light, generally sent across a scale nearly at a right angle to its long axis.

? Deilephila Elpenor (Elephant hawk-moth) exhibits various appearances according to illumination and focussing. An aspect of coarse beads is evidently an optical illusion, and even if the objective is well corrected, a false appearance is seen if the focussing is too near the object. The most probably correct view seemed that of longitudinal ribs, with trough depressions between them, each trough crossed thickly with very numerous rows of beads in fine lines. A false aspect is presented if these horizontal beads are not well displayed.

Melanippe.—1. A scale showing better than most, gave appearances somewhat similar to the preceding, but the bead rows between the ribs inclined to be convex. This was not the case in some other scales. Some scales appeared to contain a great many more beads than others. In injured scales some bead rows were displaced without injury, but in some spots which looked crushed —no structure visible. Many scales show beading as plain as possible. The fineness of the beading varies in different scales.

2. Delicate beaded ribs, transverse rows as if in depressions; very distinct.

Morpho Helinor.—Structure much the same. Beading very delicate and fine, but beautifully sharp and clear.

Microlepidopter.—Ribs not as strong as in most others. Beading very delicate; linear on some scales, in others running in curved and angular directions to perpendicular axis. Beads very distinct, though small.

JUNONIA (GAROVIUM ?).—Beaded ribs very distinct; bead rows at right angles to them and between them distinct. A very slight error in focussing confuses the appearances. Each rib should be shown as a raised row of single beads.

MARS CHANGEANT.—Ribs numerous; beading fine, but distinct; transverse beading not uniform in angle. Careful focussing needful.

PAPILIO AGARIS.—Beading distinct both in broad and long scales; rib beading in former somewhat irregular.

LYCORRA ATERGATIS.—Ribs close together; beading distinct. Slight error in focussing confuses ribs and interspaces. Torn scales show that line of separation tends to run between beaded ribs; in some places single bead rows have been detached with little injury.

ABETINOR DE SURINAM.—Rather difficult; beads distinct when well shown, and seeming close together. Damaged scales confirm fact of beading. It is very easy to confound the ribs and interspaces with their bead rows at a lower level.

ATLAS CHINA.—No reason to doubt the beading. Ribbing best shown towards serrated tips.

ALCANDOR (CHINA).—Beading between the ribs irregular. Central ribs in one scale obscured by thick dark irregular beading. Another scale has this character all over.

III.—*On the Structure of the Stems of the Arborescent Lycopodiaceæ of the Coal-measures.* By W. Carruthers, F.R.S.

IV. On a Leaf-bearing Branch of a Species of Lepidodendron.

(*Read before the* Royal Microscopical Society, *Jan.* 3, 1872.)

Plates VII. and VIII.

The beautiful series of specimens illustrating the structure of this *Lepidodendron* have been recently obtained for the British Museum from the valuable collection of Mr. John Butterworth, of Shaw, near Oldham. Like my friend Professor Williamson, I have also to express my great obligations to Mr. Butterworth for the singularly instructive specimens which he has submitted to my examination. Living as he does on the spot where the calcareous concretions, which have supplied such valuable materials to recent workers, occur, imbued with a love for the study of these ancient vegetable forms, and having an extensive and accurate knowledge of their structure, and a quick eye for the parts in their structure which are yet only obscurely known, Mr. Butterworth makes sections with his own hands of carefully-selected materials, in a manner which renders them highly instructive, and greatly facilitates the labour of interpretation and description. The truth of these observations will be apparent by the series of drawings accompanying this paper, made by Mr. Hollick from four slides representing transverse and longitudinal sections of the stem and transverse sections of the leaves close to and at a little distance from the stem.

Of the numerous figures that have been given by Witham,

EXPLANATION OF PLATES.

Plate VII.

Fig. 1.—Transverse section of the branch of a *Lepidodendron*—natural size.

„ 2.—Longitudinal section of the same—natural size.

„ 3.—The lower portion of Fig. 1 enlarged 8 diameters. *a*. the vascular axis with the processes representing the leaf bundles not yet liberated from the axis, and surrounded by the free leaf bundles. *b*. the space occupied by delicate cellular tissue, now filled with carbonate of lime. *c*. the epidermal tissues, with openings through which have passed the leaf bundles. *d*. the bases of the leaves, showing also the single vascular bundles passing to them.

„ 4.—Portion of Fig. 2, enlarged 8 diameters. The letters indicate the same parts as are represented in Fig. 3. *e* is the leaf base, showing the line of articulation.

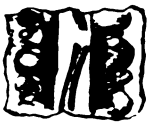
Plate VIII.

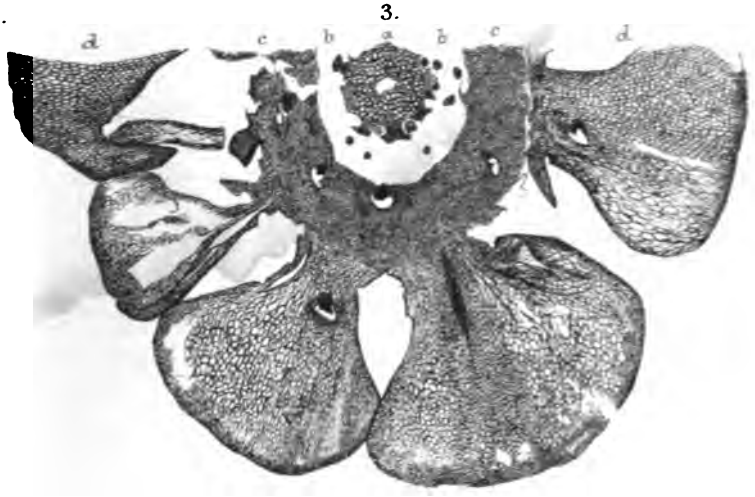
Fig. 1.—The bases of four leaves cut through near their connection with the stem.

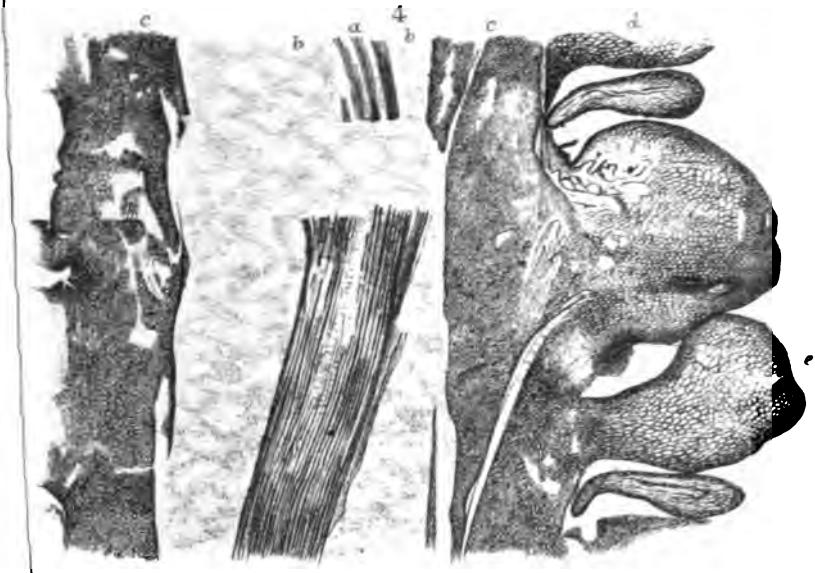
„ 2.—The bases of the leaves at a greater distance from the stem.

„ 3.—A single leaf (*a*. from Fig. 1), magnified 18 diameters, showing the single vascular bundles at the lower part.

„ 4.—A single leaf (*a*. from Fig. 2), magnified 18 diameters, showing the leaf bundle of Fig. 3 split into two distinct bundles.







A.T.Hollick ad nat del.

Branch of a Lepidodendron from the Coal Measures.

Lindley and Hutton, Brongniart, Binney, and myself, none have shown the structure of the leaf bases, and their relation to the stem, except that which I figured in my first communication to this Society;* but in that figure the single leaf was exhibited in one direction only, and not with that completeness which, by the help of Mr. Butterworth's specimens, I am now able to give.

As, however, the specimen exhibits several differences in its internal structure from the *Lepidodendron* stem which I formerly figured, and to which I have referred, I will describe its structure at length. It is most desirable to have as many types of stem structure, and as many varieties of the same type before us, so that when we come to generalize as to their affinities, we may fairly estimate the value of the different variations, and intelligently interpret the affinities of these palæozoic stems to the similar structures in existing allied plants.

The differences in the structure of this stem are so distinct from those that have been already published, that it would be quite in accordance with the practice hitherto adopted to give to this fragment a specific name. But as the species in *Lepidodendron* are distinguished by the form and markings on the leaf scars, and as these are at present unknown in this fragment, it is better to leave it in the meantime unnamed, and trust that the continued labours of Mr. Butterworth, or some other investigator, may result in identifying the named form to which this belongs. The practice of recklessly bestowing names on fragments that obviously belong to, but cannot at the time be certainly correlated with known species, which is followed by some students of vegetable palæontology, is not only adding innumerable synonymes to the already heavily-burthened pages of systematic works, but is creating a terrible and a thankless labour for every honest and exhaustive worker. It is, no doubt, a short and ready solution of every difficulty to set aside the labours of previous workers, and it is besides rather flattering to one's self to be the creator of a series of names. But what would be thought of such a practice in any other department of Natural History? Suppose, for instance, it were discovered that we had in this country another *Papilio* beside the Swallow-tail, and that one entomologist got hold of a hind wing and found that it had two tails, and so full of his important discovery he figures and describes his fragment as *P. bicaudatus*, Mihi; another finds a head with the antennæ attached, and these are obviously more club-shaped than the known species, and of course it is *P. clavatus*, Mihi; the body falls into the hands of a third, and it is thick and short and blunt, and easily distinguished from *Machaon*, so it becomes *P. truncatus*, Mihi; the fore wing turns up, and it has got blue lines and spots, and it would be

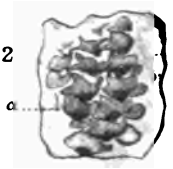
* 'Mon. Micr. Journ.,' vol. ii., Pl. XXVII., Fig. 1 *a*.

absurd not to give this beautiful new species a name, and it is *P. cæruleus*, Mihi; but the body is investigated by an entomologist with an anatomical bias, and he makes some important observations deserving to be published, and the subject must have a name, so it becomes *P. intestinalis*, Mihi; and to terminate an illustration which might be carried to any extent, the caterpillar is found in a field of carrots, a discovery so important must be published at once, and it is *P. Carotæ*, Mihi. The absurdity of such proceedings is apparent from such an illustration as this, but in fossil botany the terrible reality has to be encountered, and not only roots, stems, branches, leaves, and fruit get different names, but different states of the same stem receive different generic and specific names. In the progress of my descriptions I shall take occasion to point out the origin of these accidental states which have received generic positions, and give my reasons for eliminating many worthless names from the science. And I shall further, in the present case, avoid introducing a name which in my next communication I may have good reason for placing as a synonym to a well-known and well-described species.

The axis of this *Lepidodendron* (Pl. VII., Figs. 3 *a*, 4 *a*) is entirely composed of scalariform tissue, which in the transverse section is seen to be irregularly arranged, with the long diameter of the vessels having a radial direction. The ends of the vessels are more or less circular or polyhedral, and they are tolerably uniform in size throughout, except at the exterior, where they are very much smaller. It is important to notice that this central vascular axis is not invested at any part of its circumference by the cylinder of radiating scalariform tissue which I described in *Lepidodendron selaginoides*, Sternb.* The boundary line of this axis is irregular. The small scalariform vessels project, and form recesses in which free bundles of vessels of similar size occur. The projections are the leaf bundles to be given off somewhat higher up the stem. They form as yet only ridges, gradually becoming more prominent, until they are completely separated from the axis, and then they appear as described, as free bundles in a deep furrow. Those bundles are to be found throughout the transverse section of the stem; near the centre they are always cut right across, but as they proceed towards the circumference of the stem, they are cut in a more and more increasingly oblique direction. This is the result of the course the bundles take, being at first almost erect and parallel with the axis, but gradually taking a more outward course until they reach the base of the leaves. They spring from the outer margin of the central vascular axis, a long way below where they pass off to the leaf.

Prof. Williamson, in his recent investigations into the organiza-

* 'Mon. Micr. Journ.,' 1869, Pl. XXVII., Figs. 1 *b* and 2 *b*.



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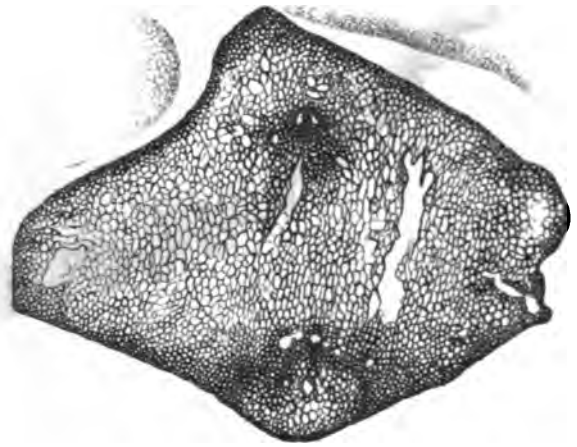

4.

A T Hollick, ad. nat. del

W West & Co. imp.

Leaves of a Lepidodendron from the Coal Measures.

tion of *Lepidodendron*, proposes to call this axis a medulla.* Our specimen clearly establishes that this cannot be the nature of the axis. It is, as we have said, entirely composed of scalariform tissue; it gives off from its outer surface the vascular bundles which supply the leaves; it has no investing scalariform or woody tissue, and it is placed in the centre of the cellular tissue of the stem. There can be no doubt that it is homologous with the vascular axis of the living *Lycopodiaceæ*, as Witham, in the first paper that treated of the internal structure of a *Lepidodendron*, with remarkable sagacity suggested.† The circumference of the axis is precisely similar to that of *Lepidodendron Harcourtii*, With., being entirely without any investing cylinder; but in that species, as figured by Witham,‡ by Lindley and Hutton,§ and by Brongniart,‖ the interior of the axis is composed of "elongated cellular tissue" (Brongn.), with acute terminations, and having a scalariform structure. There is no indication of such a change in the structure of the axis of this plant either in the transverse or longitudinal section.

The tissue surrounding the axis, Pl. VII., Figs. 3*b*, 4*b*, has completely perished in the specimen figured, and the space which it occupied is filled with semi-transparent carbonate of lime, except when here and there a leaf bundle has resisted decay. The destroyed tissue, which is well preserved in some specimens that I hope to figure in due time, was a thin-walled parenchyma.

Beyond this, Pl. VII., Figs. 3*c*, 4*c*, are the true epidermal tissues of the stem. The spherical cells have thickened walls. They are of larger size towards the interior of the stem, and the smaller cells of the exterior are found in longitudinal section to be slightly elongated, yet not to so great an extent as in *Lepidodendron selaginoides*, Sternb.¶ This epidermal layer has successfully resisted decay. Scattered through it are several leaf bundles in open spaces apparently completely detached from the tissue itself. This isolation has been produced by the decay of the delicate cellular tissue which accompanied the bundles from the inner perishable layer. Stems frequently occur, especially in arenaceous rocks, in which the hollow interior has been filled in with the amorphous substance of the rock, and this has pushed itself from the interior outwards through the empty passages occupied by the cellular tissue of the leaf bundles. The indurated epidermal tissues having been subsequently converted into coal, and removed, by slow combustion before the specimen was taken from the rock, or by accident

* 'Proc. Roy. Soc.,' vol. xix., p. 500.
† 'Trans. Nat. Hist. Soc.,' Newcastle-on-Tyne, March, 1832.
‡ *l. c.*, Pl. ii., figs. 2 and 4.
§ 'Fossil Flora,' Pl. xcviii., fig. 2.
‖ 'Observ. Sigillaria elegans,' Pl. xxx., figs. 5, 8; xxxi., fig. 4.
¶ 'Mon. Micr. Journ.,' 1869, Pl. XXVII., Fig. 2*e*.

or atmospheric agency after its discovery, the amorphous cast of the interior with its longer or shorter processes corresponding to the leaf bundles have been supposed to be stems with leaves, and have received the generic name of *Knorria*, and the numerous accidental varieties have obtained a large series of specific designations. The longitudinal section (Pl. VII., Fig. 4) shows one of these empty canals passing upwards and outwards from the vascular axis.

From the exterior of the stem spring the bases of the leaves (Pl. VII., Figs. 1 and 2). There is no cuticle to the stem, as its outer surface is entirely made up of the bases of the leaves. The cellular tissue of the leaves is a continuation of the epidermal tissue (Pl. VIII., Figs. 3, 4). The cells in the interior of the leaf are large, and in longitudinal section somewhat oblong. Towards the margin they are much smaller and more compact, forming a firm surface to the leaf. The leaves proper have fallen from the branch figured, and have left a cicatrix which can be detected in those leaves which are cut through the centre, and to some extent it may be seen in the lower leaf in the longitudinal section, Pl. VII., Fig. 4 *e*. A single bundle passed from the axis into each leaf, and it remained single, for some short distance, through the base of the leaf (Pl. VIII., Fig. 3), but before reaching the articulating surface to which the true leaf was attached, it divided into two distinct bundles (Pl. VIII., Fig. 4). It is important to notice the two appearances that the outer surface of this specimen would exhibit, depending on whether we were examining the surface of the stem or the surface of leaf bases; in the one case the leaf bundle would be single, in the other double.

The leaf bases had a direction nearly at right angles to the supporting stem. They were rhomboidal in transverse section, with the lateral angles acute. It is probable that in both *Lepidodendron* and *Sigillaria* the leaf bases were persistent, and consequently that the principal character on which Corda separated *Lepidophloios* from *Lepidodendron* does not hold good. Indeed the specimen which I have now described should be rather placed in *Lepidophloios* than *Lepidodendron*, were the two designations retained.

IV.—*On Bog Mosses.* By R. Braithwaite, M.D., F.L.S.

Part III.

Monograph of the European Species.

Plate IX.

About 55 species of Sphagnum are scattered throughout the world, nearly all of which have so much in common in their appearance and habit, that we do not readily observe the characters requisite to establish good species and again to form these into sections.

The structure of the cortical layers of the stem, and the form of the leaves found on the stem, the fruit-peduncle and the divergent branches, combined with the cell-texture of the same, afford the characters on which we must principally rely for specific distinctions, though the last named does not appear to be so invariable as has been supposed, for the spiral threads contained in the cells may partially disappear under certain conditions of growth.

In the arrangement of the species Bridel adopts two sections, Obtusifolia and Acutifolia; and this plan is followed by Wilson in the 'Bryologia Britannica.'

C. Müller, in his 'Synopsis Muscorum,' has *a.* with rounded leaves, *b.* with truncate leaves, and the latter is again divided into two, according as the peduncular leaves have or have not spiral fibres.

Sullivant, in Asa Gray's 'Botany of the Northern United States,' arranges the species by the relative positions of the two kinds of cells, seen on cross-section of a leaf, a character far too minute, and difficult to be observed, to be of practical utility. Prof. Schimper places all the species in two groups, Monoicous and Dioicous; also a most unpractical arrangement, as apart from the inconspicuous nature of the flowers, the species are so frequently found in a barren state, that such a mode of arranging them affords no help to the student. Lastly Prof. Lindberg, in his paper already quoted, which appeared in the 'Öfversigt K. Vetenskaps Akad. Förhandlingar' for 1862, has with a master's hand distributed the Sphagna in natural groups, characterized essentially by the form of the branch leaves, and leaving nothing to be desired.

After separating *S. macrophyllum* as the genus *Isocladus*, Prof. Lindberg divides the Sphagna into two sections, 1, Homophylla having the stem leaves and branch leaves alike in form, and destitute of threads. *S. sericeum* and *S. Holleanum* from Java and Sumatra belong here. 2, Heterophylla having the stem and branch leaves of different forms, and in this section four groups include the European species. In a letter recently received, my kind friend alters the sequence of these, placing *S. cymbifolium*

first, as being the most highly developed form, and this order will be followed here. The drawings have been made by the camera lucida and Messrs. Beck's large instrument, 3-inch object-glass and A eye-piece for leaves, ⅔-inch and B eye-piece for their cell-texture. In the synonymy I have only quoted the more important works, with the hope of being less tedious to the general reader; and in the descriptions, the characters more essentially distinctive of the species, are printed in italics, so as readily to catch the eye and save repetition.

Sphagnum Dill.

Stems dichotomously fastigiate, erect, renewed by an annual innovation at apex; ramuli fascicled at base, flagelliform, partly patulous, partly adpresso-reflexed; the younger clavate, erect, clustered in a dense capitulum at summit of stem. Cauline leaves five ranked, erecto-incumbent or reflexed, soft; ramuline leaves very small at base of branches, thence gradually larger, toward apex narrower and longer, acute, all nerveless, when dry extremely fragile, whitish. Flowers monoicous or dioicous; the male strobiliform on lateral ramuli, antheridia very numerous, solitary at the side of each involucral leaf, on an elongated filiform pedicel, subglobose; female flowers forming long gemmules, lateral, many in each capitulum, archegonia 1–5 in the same perigynium, with very slender, much-branched paraphyses, internal perigynial leaves very small, finally evolved to form the perichœtium and larger than the external. Fruit solitary, up to maturity hidden in the perichœtium, afterwards exserted on a receptacle, prolonged into a pseudopodium. Capsule globose sessile on a discoid turbinate vaginula with a bulb-like pedicel immersed in it; solid, with numerous stomata, chestnut-coloured or blackish; operculum minute hemispherical, mouth naked without any annulus or peristome; columella elevated, hemispheric, covered by the excavato-hemispheric sporangium; pericarpic membrane very thin, whitish, irregularly torn, adhering partly to the vaginula, and partly to the capsule; spores sulphur-coloured or ferruginous.

A. Cymbifolia.

1. *S. cymbifolium* Ehrh.

B. Subsecunda.

2. *S. tenellum* Ehrh.—3. *S. rubellum* Wils.—4. *S. neglectum* Ångst.—5. *S. subsecundum* N. von Es.

C. Rigida.

6. *S. Ångstromii* C. Hartm.—7. *S. molle* Sulvt.—8. *S. rigidum* N. H. and S.

D. Cuspidata.

9. *S. squarrosum* Pers.—10. *S. teres* Ångst.—11. *S. acutifolium* Ehrh.—12. *S. strictum* Lindb.—13. *S. fimbriatum* Wils. — 14. *S. Lindbergii* Schpr. — 15. *S. Wulfianum* Girgensohn. — 16. *S. intermedium* Hoff.—17. *S. cuspidatum* Ehrh.

Of these *S. Ångstromii* and *S. Wulfianum* have not yet been detected in Britain.

Group A. Cymbifolia.—Plants robust, laxly tufted, sometimes intermixed with other species. Branches turgid, those of the coma obtuse; branch leaves imbricated, very broad, ovate, rounded and cucullate at apex, boat-shaped, concave, scarcely margined. Dioicous.

1 Sph. cymbifolium Ehrhart.

Hannöverisches Magazin (1780) p. 235.

PLATE IX.

Syn.—*Sphagnum molle deflexum, squamis cymbiformibus*, Dillen. Hist. Musc. p. 240, Tab. XXXII, fig. 1 (1741).—*Sph. palustre a.* Lin. Sp. Pl. II, p. 1106 (1753). Lindberg Revis. Crit. Ic. Fl. Dan. p. 8 (1871).—*Sph. cymbifolium* Ehrh. l. c., Hedw. Fund. Musc. II, p. 85, Tab. I, fig. 1 (1782). N. Hsch. & St. Bryol. Germ. I, p. 6, Tab. I (1823). Brid. Bryol. univ. I, p. 2 (1826). C. Müll. Syn. Musc. I, p. 91 (1849). Wils. Bryol. Brit. p. 17, Tab. IV (1855). Schpr. Torfm. p. 69, Tab. XIX (1858). Syn. Musc. Eur. p. 684 (1860). Lindb. Torfmos. No. 14 (1862).—*Sph. latifolium* Hed. Sp. Musc. p. 27 (1801). Turner Musc. Hibern. p. 5 (1804). Schwägr. Supp. I, P. I, p. 12 (1811). Eng. Bot. T. 1405.—*Sph. obtusifolium* Ehrh. Crypt. exsic. p. 241. Hoff. Deutsch. Fl. II, p. 21 (1795). Web. & Mohr, Bot. Taschen. p. 72 (1807). Hook. & Tayl. Musc. Brit. p. 3, Tab. IV (1818).

Dioicous. Plants robust, 6–12 inches long, often bipartite, laxly or densely tufted: pale or dirty olivaceous green, sometimes variegated with purple or brownish yellow, pale beneath. Stem solid, woody, tawny brown, sometimes green, covered with a very spongy *cortical web forming 3–4 strata, the cells of which are fibrose and porose.* Ramuli 4–5, of which 2–3 are divergent, arcuate, turgid, acute at apex, the others pendulous and appressed to stem, their *cortical cells spiriferous*, and part of them perforated at apex. Cauline leaves lingulate-spathulate, slightly frayed at the rounded apex; very soft, longitudinally sulcate, very laxly areolate, usually without fibres or pores.

Ramuline leaves densely imbricated, broadly ovate, deeply concave with the margins incurved above, *extreme apex cucullate and squamulose at the back by prolongation of the inferior margin of the large pores;* hyaline cells dilated, rhombic, with annular and spiral fibres, pores large, not numerous; chlorophyl cells compressed, entirely enclosed by the hyaline. Male plant like the

female but more slender, the spikelets of flowers thick clavate, green, ochraceous or reddish, the bracts broadly ovate, like the branch leaves in structure. Fruits either clustered on the coma or scattered, peduncular leaves laxly imbricated, broadly oval, obtuse, with their upper hyaline cells fibrose; capsule solid, but little exserted or on a moderately elongated peduncle; spores rufo-ferruginous.

Var. β *squarrosulum* Bry. Germ. Plants slender in loose dark green tufts, divergent branches loose, their leaves more pointed and spreading, somewhat squarrose.

Var. γ *compactum* Schultz Supp. Fl. Starg.

Var. β *congestum* Schp.—*Sph. compactum* Brid. Bry. univ. I, p. 16, pro parte.

Stem mostly divided, densely tufted, pale purple or rufous and green; branches densely crowded, sub-erect, julaceous, obtuse; upper cells of stem leaves having spiral threads.

Hab.—Marshy places in woods, watery heaths and wet hollows in moorlands. Generally distributed. Fr. June. This species is at once distinguished by the cucullate squamulose apex of the branch leaves, and the presence of very fine fibres in the cortical cells. The intermediate leaves are very minute, but may be most readily seen by cutting out a piece of the stem, with a single branch-fascicle attached, and observing them *in situ*. The drawings are made from moderately lax specimens of the typical form, collected by myself on Ben Lawers, β from Continental specimens collected by Herr Schliephacke, γ collected by J. G. Baker, Esq., on Honister Crag.

EXPLANATION OF PLATE IX.

Sphagnum cymbifolium.

a.—Female plant. *a* ♂.—Upper part of male plant.
β.—Var. squarrosulum. γ.—Var. compactum.
1.—Part of stem with a single branch-fascicle.
2.—Catkin of male flowers. 2 *b*.—Bract from same.
3.—Fruit with its peduncle. 4.—Peduncular leaf.
5.—Stem leaf. 5 *a a*.—Areolation of apex of same. 5 *a b*.—Ditto of base.
6.—Leaf from middle of a divergent branch. 6 ×.—Transverse section. 6 *p*.—Point of same. 6 *a*.—Areolation about half-way up.
7.—Intermediate leaves from the base of a divergent branch.
8.—Leaf from apex of same.
For section of stem and cortical cells, see Pl. XC. in last volume.

Sph. cymbifolium

V.—*The Advancing Powers of Microscopic Definition.*

By Dr. Royston-Pigott, M.A. Cantab., M.R.C.P., F.C.P.S., F.R.A.S., M.R.I., F.R.M.S.

As it is now above two years since I had the honour of laying before the readers of the Transactions of the Royal Microscopical Society some of the results to which I had been led by several years' patient inquiry, I may now be permitted to state that further investigations have confirmed the views then first broached. During this interval I have had the honour of receiving several communications from distinguished microscopists confirmatory of their truth, and among them I wish to mention that of Colonel Dr. Woodward, who has most kindly forwarded to me three distinct sets of his peerless photographs. Dr. Woodward has favoured me also with the War Department Memorandum on Test Podura, from which I take leave to make a few extracts, as placing the matter in a clearer light than any words of mine can do.

"War Department, Surgeon-General's Office,
Army Museum, *February* 22, 1871.

"For a long time the scales of certain insects have been favourite test-objects for high-power definition. Among these especial importance has been attached to the so-called Podura scale, which has been extensively used by English and American microscopists, as well as by makers of objectives, who have employed it to try their glasses during the process of construction. The market was supplied with slides from various sources, which were offered for sale by all vendors of objects for the microscope, under the general designation of 'Podura scales.' The markings on the scales, as seen by a good 1–5th or by higher powers, were generally described as resembling exclamation marks, without the lower dot, and with a clear line of light running down the mark. It was thought that those objectives were most perfectly corrected, which showed these exclamation marks well defined, with the points sharp, the butts handsomely rounded, the contours black, the central line of light brilliant and broadest towards the butt, and the whole scale as free as possible from chromatic effect." . . . .

"Under these circumstances considerable interest was excited among microscopists by the paper of Dr. G. W. Royston-Pigott, which appeared in the 'Monthly Microscopical Journal' for December, 1869 (communicated May 21, 1869). Dr. Pigott asserted that the exclamation marks were an optical deception, produced by two sets of beads crossing each other at a small angle, each of the exclamation marks corresponding to three or four beads." . . . .

"The photographs (of the *Lepidocyrtus curvicollis*) are taken from a slide presented by Mr. Joseph Beck, partly because the scales on

this slide were the most plainly marked of the series at my disposal, partly because, as Mr. Beck collected the scales himself from a carefully-identified insect, there could be no question as to the species from which they were derived. A brief account of the results of these studies must now be presented."

"1. The *Degeeria domestica* (*Seira domestica* of Sir J. Lubbock). . . . . Their markings were very bold, and the object was in every way a beautiful one. . . . . With carefully-centred illumination the markings resembled the notes of exclamation on the Lepidocyrtus scale. . . . . When suitable obliquity was given to the light, an appearance readily started into view, which recalled at once the description of Lepidocyrtus scale given by Dr. Pigott. In focussing upon the upper surface of the scale, varicose ribs, *which might almost be described as rows of beads, were seen*, with similar ones belonging to the lower surface appearing less distinctly in the intercostal spaces. By depressing the objective still further the lower varicose or beaded ribs came into view, the upper ones appearing less distinctly in the intercostal spaces. These varicose ribs were alternately reddish and greenish, their colour varying with changes in the illumination. When the upper ones were reddish, the lower were greenish, and *vice versâ*. The words varicose ribs would seem to describe the appearance more accurately than 'rows of beads.' Certainly no such distinct isolated bosses were seen, as may be observed, in so many diatoms. It is worthy of note, moreover, that on most of the scales, when the objective, after having been carefully focussed, first upon the upper ribs and then on the lower, was still further depressed, the exclamation marks started into view. . . . . When monochromatic sunlight was used to illuminate the object, the form of the varicose ribbing appeared unaltered, if anything, rather more sharply defined than before; simply the colour phenomena disappeared, the object appearing like a black engraving on a ground of the tint employed. . . . . The photographs were taken with the immersion 1–16th of Powell and Lealand.

"2. The *Lepidocyrtus curvicollis*. With the 1–8th and 1–16th Powell and Lealand, and oblique light thrown lengthways along the scale, appearances were observed which corresponded essentially with what was seen on the Degeeria. The varicose ribs, however, were finer, and as it were more difficult to obtain appearances, which would suggest the epithet beaded. Nevertheless the structure in question was observed in a tolerably satisfactory manner with all the slides of Lepidocyrtus belonging to the Museum. A well-marked scale, on the slide presented by Mr. Joseph Beck, 1–160th of an inch long, and 1–700th broad at its widest part, was selected for the purpose of preparing the photographs which accompany this memorandum. One of these shows the exclamation marks as seen with central illumination, the other shows the varicose or beaded aspect as displayed by oblique illumination."

"The general conclusions arrived at during the investigation may be stated as follows:—

"The scales of *Degeeria* (*Seira*) *domestica* and *Lepidocyrtus curvi-*

collis are very similar in structure, the markings on the Lepidocyrtus being the finest. These scales probably consist of a double membrane, and the markings are probably corrugations in these membranes. The corrugations on the opposite sides of the scales differ in boldness, those on the side next the insect being most prominent. If this side is towards the objective the exclamation marks are seen as the objective approaches the scale, before the varicose ribbings of the two membranes come distinctly into focus. In the reverse position of the scale the varicose ribbings are first seen, and the exclamation marks do not become distinct till after the scale is focussed through. The corrugations appear to have the form of narrow, longitudinal somewhat wavy ribs, which are constricted at short intervals, giving them a varicose aspect (Dr. Pigott's 'beads'). The ribs on the opposite sides cross each other at a small angle, and in this circumstance, and the different degree of prominence belonging to the markings of the opposite sides, the true explanation of the exclamation-mark appearance must probably be sought. These exclamation marks are probably illusive or spurious appearances, notwithstanding the great distinctness they attain under favourable conditions of illumination."

The photographs, the Colonel informs us, represented a magnifying power of 3200 diameters; central light showing the exclamation markings, and oblique illumination the varicose or beaded appearances.

Our talented and distinguished Secretary, Mr. Slack, who is well known for the strength of his convictions, in the 'Popular Science Review' for January, 1872, recurs to the same subject in an elaborate paper, apparently with the honest intention of setting this matter at rest. "The scales of insects," says this gentleman, "are an ornamental covering, probably of some use in protecting the membrane from which they spring, and on which we find them arranged like tiles on a roof; but the flying power does not seem impaired when numbers of them are rubbed off. By their beautiful aspects, they make, according to Mr. Wallace's observations, males and females mutually attractive, and they are frequently the means of disguises which enable their possessors to escape notice of their enemies." He goes on to remark that

"Among the most difficult to be shown clearly, are certain markings on the scales of insects popularly termed Podura; and since Dr. Pigott affirmed that in the famous test-scale now named *Lepidocyrtus curvicollis*, with sufficiently corrected glasses a distinctly beaded structure was to be seen in them, fresh discussion as to their real nature has gone on without ceasing, and strong feelings, as well as reasonings, have been shown by many who had perfectly satisfied themselves with the appearance of the well-known note of exclamation marks, so well shown and so beautifully figured by the late Richard Beck."

He also proceeds to observe that

"An intermediate membrane (in these scales) has been described by some observers, but this appears only the result of a deposit which in most scales takes a more or less beaded form, and may combine into a distinct layer of some kind. Dr. Pigott's 'beads' are by no means inconsistent with the existence of corrugations; and the exclamation marks are probably *true aspects* with a particular focussing and illumination, though few observers, who have taken much care in the investigation, have for many years supposed them to afford an accurate and complete idea of structure. The extreme delicacy of the Podura or Lepidocyrtus scale gives rise to so much difficulty, both of observation and interpretation, that it is advisable to be guided by analogy drawn from easier scales in its interpretation. This plan was pursued by the writer (Mr. Slack), who traced what seemed to be real beads in ordinary and easy butterfly scales, through more difficult ones, up to those of *Lepidocyrtus curvicollis*. Mr. McIntire took up the question with great skill, and with an absence of prejudice somewhat remarkable in a discussion which has excited an unusual amount of strong feeling; and whatever ultimate conclusion may be reached, his observations and beautiful sketches will have a permanent value.* . . . . In scales of *Polyxenus lagurus* he found what was 'very uncommon'; according to his observations, 'a deposit between the membrane' and the scale was a very solid structure. Most of his endeavours to detect beaded deposits led him to think such appearances were only 'ghosts'; and it is well known that false appearances of beading are easily produced under certain conditions. Mr. McIntire's account of his observations and experiments scarcely warrants his conclusions, for he admits 'pigment granules' in scales such as *Anaithusia Horsfeldii*, figured long since by Mr. De la Rue, and in some others. Lieut.-Colonel Dr. J. J. Woodward, of the U. S. Army, employed his well-known skill in photographing Podura scales; and in a paper read before the Royal Microscopical Society 'On the Coarser Degeeria Scale,' he says, 'I had no difficulty in making out appearances, which, so far as I can gather from Dr. Pigott's own descriptions and the published discussion of his views, are substantially the same as those seen and shown by him . . . . and even on the more minutely marked and difficult Lepidocyrtus scale I have been able to develop appearances which seem to be substantially similar.'† Dr. Woodward did not, however (writes Mr. Slack), pronounce any decided opinion as to the real structure, but since this paper he has kindly forwarded to the Royal Microscopical Society and to the writer, photographs of *Degeeria domestica*, beautifully exhibiting a beaded appearance. In a communication to the Royal Microscopical Society, read in May, 1871, Dr. Woodward speaks of Mr. Joseph Beck having shown and left with him a fine Podura slide, showing the note of exclamation marks with remarkable clearness, 'but immediately afterwards, with the same optical combination and magnifying power, without any change in the cover

* See 'Monthly Microscopical Journal,' January 1, 1871, &c.

† April No. of the 'Monthly Microscopical Journal,' 1871.

correction, by simply rendering the illuminating pencil oblique, and slightly withdrawing the objective from its first position, he obtained a negative which displayed the bead-like or varicose appearance of the ribbing more satisfactorily than he had previously been able to do.' A photograph of this appearance may be seen at the Royal Microscopical Society's room."*

The article in question next informs its readers that "Dr. Maddox took up this much-controverted question, and after taking great pains to remove oily matters by chemical solutions, he finally made out a ribbed structure, to which he thought the beaded aspects were due, as fine ribs crossing each other would give that effect. It does not seem, however, that the existence of such structures as Dr. Maddox figures negatives the existence of deposits in a more or less beaded form; nor do the investigations of Mr. Wenham, which prove the reality of surface irregularities more or less corresponding with the exclamation marks." †

An interesting letter from The Chevalier Huyttens de Cerbecq, in the January number, gives evidence of the extending range of our Transactions. It begins with so piquant a sentence, and appears to touch so closely the much-vexed question so long at issue, that I may be permitted to make an observation or two upon it. The Chevalier writes:—

"*I think it most important for everyone that he should be able to see through the microscope all that is to be seen, and nothing else* . . . . having studied for some months the scales of many butterflies and moths of European, Chinese, and Brazilian origin, I have been struck by the elegance of the beaded appearance those objects presented. I may thus almost entirely confirm Mr. S. J. McIntire's views: *those scales have almost all of them a beaded appearance, perfectly defined:* but where I differ totally from this most distinguished Fellow of the Royal Microscopical Society is, when he asserts in his paper read the 9th November, 1870, 'that those beads have no real existence *as beads*, but are due to the interference of the rays of light by corrugated membranes; that they are, in fact, *ghost-beads*.' Reasoning upon this hypothesis, it is not at all wonderful that Mr. McIntire should have attributed his failures, when 'he wished but could not call into existence *those almost palpable beads*,' as he calls them himself, so distinctly are they visible, 'to a predisposition of his mind, and to the want of that necessary adroitness in manipulating which everyone knows very well is not always at one's own command.' . . . . I think it was due to an imperfect correction that Mr. McIntire was not always able to see the beaded appearance of the scales. . . . . In confirmation of the present letter, I beg the Royal Society to accept a dozen of my slides of butterfly scales, some of them *selected*." ‡

* "Recent Microscopy," 'Pop. Sci. Rev.,' Jan., 1872.

† 'Pop. Sci. Rev.,' Jan., 1872, pages 11–14.

‡ By the courtesy of Mr. Slack I have been (Jan. 11) favoured with a loan of these interesting slides. The *Rhetenor* (*Surinam*?) presents, *when* "*the colour test*"

At the time of penning these lines these specimens have not been as yet seen by the writer.

The acumen of the Chevalier in microscopic research is well evidenced by his telling remark that he has proved the real nature of the *Surirella gemma* (so beautifully photographed by Colonel Woodward), "that most beautiful puzzle," for he says "I have seen the dots which this interesting diatom wears. This appearance must be the true one, because, having tried that same diatom with a double reflector obliquely disposed at right angles to each other, I have obtained the same appearances." It is worth recounting that the Chevalier used immersion objectives of Ross $\frac{1}{12}$ and Nos. 9 and 11 of Hartnack, and that Mr. McIntire employed a Hartnack also, which he exhibited to me at my residence about two years ago. The improvements, however, which have been made by Messrs. Powell and Lealand in their objectives, have given a new impulse to minute investigation, and I can speak highly of a new one-fiftieth of an inch immersion objective, expressly constructed for me—the first they have made upon their newly-discovered principles of construction. And this exquisite glass has been employed by me to re-test some of the more remarkable results previously obtained.

Thus, in viewing the fringes of delicate objects, I have observed a better defined outline, the most difficult of all kinds of pure and unalloyed definition. The serrated edges and quill-like projections of the ribs of scales have been more sharply, clearly, and more distinctly cut in a light and dark tracery.

I have also observed the general beading of a variety of scales hitherto totally "impalpable" with ordinary glasses employed in the usual manner. Just as twenty-five years ago ordinary glasses only gave a lined appearance to the then difficult tests, such as the *Navicula hippocampus*, and just as an ordinary $\frac{1}{8}$th now only gives the delicate longitudinal ribs on the scales detached from the gnat's wing, which indeed generally presents a pale structureless smooth

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is obtained, the most beautiful appearance of double-striated beading I have yet been able to observe: judging from a comparison with the beading of the Rhomboides, I should estimate them at 100,000 (one hundred thousand) per inch. They can be readily counted in close contact at the serrated end of the scale. The lines on Nobert's 19th band appear to be about $\frac{1}{100000}$th in diameter. The Rhetenor beading can be distinguished with an old but excellent quarter, or rather fifth of Andrew Ross, 1851 date.

The "Atlas, China," is a magnificent object at 4000 diameters with Powell and Lealand's new $\frac{1}{25}$th immersion.

The *Deilephila Elpenor* is a beaded but accurately-striated scale, so regular as to appear absolutely perfect, and presenting the most delicate molecular structure imaginable.

Many of the scales sent by the CHEVALIER HUYTTENS DE CERBECQ are formed of longitudinal striated beading, crossed with short double rows of transverse beading, containing about four or five beads in each transverse bar, but much smaller than the longitudinal of which the Deilephila is a most interesting type.

appearance (structureless as regards beading), the 1-50th immersion of Messrs. Powell and Lealand displays a rich profusion of beading.

I have also succeeded in displaying the same results with the Lealand $\frac{1}{4}$th and Wray $\frac{1}{3}$th by means of sufficient amplification and accurate spherical and achromatic corrections.

The faltering steps by which microscopists have during the last twenty years groped their way to their present magnificent results must ever be instructive, as developing the grades by which a dim, difficult, almost desperate definition has given place to a brilliant, certain and satisfactory evolution of previously unknown forms.

The question as it now stands appears to warrant the conclusion that all membranous structures of the scales of winged and also unwinged insects are APPROXIMATELY *striated, the striæ in the two layers being either parallel, inclined to each other, wavy or radiating either in one set or both.* I use the term *approximately* as denoting the first approximation attainable to ordinary definition. As the definition advances, these striæ show in different planes, become ribs; and most probably from either diffraction or polarization or both, being in different planes, exhibit complementary colours *red* and *greenish blue*, according to the compound character of the illuminating ray, or if monochromatic light be employed, then shaded more or less. The next step in a more advanced definition is the resolution of these ribs and intercostal spaces into beads, molecules, or agglomerated particles. When these lie in different planes, and the corrections are brought to the utmost perfection at present attainable, they assume respectively different colours, usually ruby or pink, red and sea green, or sapphire blue. The assemblage of these minute little ruby and sapphire like bodies, densely crowded within the ribs (if not actually forming them), is one of the prettiest autographic pictures (of what the microscope may yet be expected easily to perform with proper corrections) with which I am acquainted. Indeed, I may perhaps be excused for venturing the opinion that had the marvellous glasses of to-day been employed a few years ago, many of the microscopical papers (innocently contributed) would never have been penned; at this moment they seem but a monument of transitional science.*

This paper would be extremely incomplete were I to omit to

* For the information of the Fellows who may be disposed to order Messrs. Powell and Lealand's 1-50th immersion objective (price 30 guineas), I may state that it is incumbent to use a covering glass of about 3-1000ths of an inch thick: but that I have found the very clear talc formerly employed by Pritchard (several of whose slides I possess, as also his wonderful doublets of $\frac{1}{10}$th of an inch focal length) answers remarkably well with the new immersion objective.

It is interesting to observe that these doublets, though only possessing an aperture of a very few degrees, develop the exclamation marks in some extremely old scales of *Lepidocyrtus curvicollis*. *These appearances are therefore* not dependent upon *large* aperture, which appears to me a very significant and striking fact.

notice more in detail the performance of Messrs. Powell and Lealand's new 1–16th objective, which they kindly placed at my disposal for a fortnight. This lens is similar though perhaps slightly superior to the celebrated immersion 1–16th signalized by Dr. Woodward as the only one which resolved Nobert's 19th band so as to be counted, and photographed the *Amphipleura pellucida*.

As I had announced in May, 1869,* a double row of spherules in diatoms (when only one-half had been *suspected*), I particularly wished to confirm this appearance. It is well known that formerly black dots used to be seen *above* the true beading (well figured by Beck). The new 1–16th showed exactly double the number of these dots. In individual spherules of the Formosum, no less than three different colours could be descried upon each boss or bead, and with oblique achromatic light a black shadow in addition. The same appearances resulted from a skilful use of their new $\frac{1}{8}$th immersion. I here beg leave to insert the notes taken May 25, 1871, and dictated by an observer with good powers of vision, and confirmed by myself.

"*Particulars.*—Condenser $1\frac{1}{2}$-in. Ross, obliquity of its axis 30°. No stops or diaphragm. Powell and Lealand's best 1–16th immersion.

"(1) *White beads with red tops*, and green narrow parallel stripes in one direction only between the beads.

"(2) A rather deeper focus, *white beads and red beads*; fainter stripes between: the dots form diamond patterns.

"(3) Focus deeper still. Prominent white beads; black shadow on the left and on bottom of the bead (lower side). There is a reddish background, the right-hand side of the bead is green.

"(4) Obliquity of axis of condenser reduced to 10°. Green and white beads, alternate rows, most distinct without any background whatever, the whole space being entirely covered with closely-packed beading, so that the white set of beads appear in the blank spaces figured in the American photographs."

These are apparently exactly twice the general quantity of beads usually seen, the blank spaces teeming with them. When the obliquity was reduced to zero, or the axis of the $1\frac{1}{2}$ condenser (unstopped) coincident with that of the instrument, the *new* beads came out beautifully distinct under the most careful corrections, as green, black, and white, as follows, representing them by G, B, and W.

G	B	G	B	G	B
	W	W	W	W	W	
G	B	G	B	G	B

"Green, Black, Green, Black, forming rows with white between."

* Paper received May 21, 1869.

Jan. 8, 1872. With Powell and Lealand's new immersion $\frac{1}{8}$th I succeeded in showing them with the same direct condenser, in a crowded form, with lines running between at right angles, forming a plaid-like pattern (square, and no longer oblique), exhibiting the double set and no intervals.

In my paper of December, 1869 (received May 21, 1869), I mentioned the double row of beading, but with the $\frac{1}{16}$th Powell and Lealand of 1862 I could not succeed in developing such decided appearances as now described. But the beaded form was so familiar with even their 1862 $\frac{1}{8}$th that there appeared nothing extraordinary in the matter till our late President announced in July, 1869, the renowned hemispheres of the Formosum, as shown by "Reade's Prism" (two months after my paper was submitted to the Council). I have reason to believe that our late excellent and distinguished President was in ignorance of the fact that the writer had the *priority*. But I even then insisted upon the *double set* now beautifully displayed by the new glasses.

It may be remarked that a plane mirror reflects five or six false images, and even a prism of reflexion which depends upon the truth of its plane surface is a poor substitute for direct unreflected light, which I always employ in the most delicate investigations.

In face of these observations with the best glasses, I presume, in existence (Powell and Lealand's own 1–16th, which I think they are unwilling to dispose of, and their new 1–50th immersion made expressly for the writer), I may make bold to predict that a double set of beading will also be discovered upon the Angulatum, Rhomboides, and other difficult diatoms.

Besides diatoms, however, there are beaded scales of such extreme delicacy as to try severely the observer's patience, as well as the quality of the very best glasses now extant. The finest that I have hitherto been able to resolve are the exceedingly minute markings upon the ribs of the gnat's wing scale, mounted in Canada balsam. Powell and Lealand's $\frac{1}{8}$th new immersion can hardly be said to accomplish this feat, although it readily resolves all the ribbed scales sent over by Chevalier de Cerbecq. Probably, before microscopists will succeed in this resolution, considerable practice will be required upon coarser, yet similarly mounted scales. This remark leads me to make a few observations upon

The Resolution of Ribbed Scales.

The advancing penetration of a microscope of the very highest class now attainable, when all necessary attention is paid to the correction of the aberration of the *eye*, as well as the eye-pieces and objectives of the observer, is in no way better exemplified than in

the breaking up of the most delicately *lined scales* into their component molecules.

It is noteworthy that these were the very first objects used to test improving definition. These lines could be seen by minute spherical lenses, and Mr. Williams, the Assistant Secretary of the Royal Astronomical Society, lately informed me that formerly he was able with lenses of his own manufacture, and which he kindly lent to me, of about 1–100th of an inch full length, to distinguish not only the *lines* of such scales, but even *beads*, at which he says he got thoroughly laughed at in those microscopically benighted days. The process by which definition has advanced seems identically the same now as formerly. Diatoms and scales were at first merely lined objects. In scales, next came a ladder-like appearance between the ribs, just like that figured by Mr. Hogg on the scale of the gnat's wing. An improving definition showed more numerous transverse lines: as the glasses are better made, the intervals between the lines appear molecular: the lines become ribs, and lastly the ribs themselves give up their component beads, formed generally in two different planes. All these scales appear to be made upon one type, *a double* agglomeration of molecular beading, more or less regular in size, and more or less symmetrically arranged. Innumerable examples could be given of this. The finest scales of *Menelaus* (*morpho*) transmit a bluish light, and the ribs yield two sets of beads.

Again, the tufted elongated scales of *Hipparchia Janerii* give up a very beautiful collection of reddish and bluish beads, as close together apparently as grains in a sand heap, the bluish being uppermost.

(*To be continued.*)

VI.—*Microscope Object-glasses and their Power.*

By Edwin Bicknell.

In the 'Monthly Microscopical Journal' for Nov. 1871, I made some "remarks" on Dr. Woodward's "Note on the Resolution of *Amphipleura pellucida* by a Tolles' $\frac{1}{8}$th." In those remarks I said that I considered it nothing more nor less than deception in Powell and Lealand in putting out objectives of that power as a $\frac{1}{16}$th, and that Dr. Woodward was no less guilty of the same in knowingly sending out work done by that objective as the work of a $\frac{1}{16}$th. Mr. Wenham has taken me to task for making that statement, and thinks it hardly fair, and discourteous to the gentleman named. I should not have known the power of the so-called $\frac{1}{16}$th, if Dr. Woodward had not given the data; and while he was *very* par-

ticular to give the exact power of the Tolles' $\frac{1}{8}$th, he remained silent in regard to the Powell and Lealand $\frac{1}{16}$th.

I applied one of the ordinary rules of arithmetic to Dr. Woodward's figures, and therein, I consider, lies my unfairness. To say that Messrs. Powell and Lealand did not know the power of the objective when it was made, or that Dr. Woodward did not know its power when he used it, would, I think, be unfair.

However, I thank Mr. Wenham for coming to my rescue. He says: "A scientific microscopist gives the diameters with his illustrations, states the aperture, and the *nominal* power of the object-glass. This quite meets the case." I agree with Mr. Wenham perfectly. Dr. Woodward has given the diameters, and I have applied his own rule; but in this case (the so-called $\frac{1}{16}$th) there was a large discrepancy, amounting to nearly 50 per cent., between the *nominal* and the actual power given. It was very easy to reach the conclusion. I have placed the odium where I think it belongs.

Mr. Wenham further says: "In such a difficult and complex arrangement as a high-power object-glass, it is almost impossible for all the makers to work to the same magnifying standard." I would like to inquire where all the much-talked-of mathematical formulæ are? Does he mean to intimate that the best opticians work by "rule of thumb"? or that it is guesswork? I have been supposing all the time since I became interested in the use of the microscope, and seeing such terms as "index of refraction," "dispersion," "light flint," "heavy flint," and "Faraday flint," "crown glass," &c., that there *were* some mathematical formulæ involved somewhere in the construction of object-glasses; possibly this statement by Mr. Wenham may account for the discrepancy between the *nominal* and the *actual* power of some object-glasses.

I should advise the opticians, if what Mr. Wenham says is true, to wait until the object-glass is done, and then ascertain its power and christen it, and then they would be within a reasonable distance of the true standard. I am well aware that the position of the combinations as determined by the cover adjustment for different objects, will change the magnifying power to a certain extent. I do not see why that range should exceed 15 per cent. of the magnifying power of the objective (unless the object-glass is made like the Tolles' $\frac{1}{8}$th for the U. S. Army Medical Museum, to work both wet and dry with the same front). I have just tried the following object-glasses, *viz.* Tolles' $\frac{1}{2}$, 70°; $\frac{4}{10}$ths, 145°: $\frac{1}{16}$th, 170°: Wales' $\frac{1}{8}$th, 100°: Zentmayer, $\frac{4}{10}$th, 75°: *all of these* are *just on the mark* in magnifying power, and in no case does the power change over 12 per cent. of the actual power of the object-glass, and the three lowest powers will adjust for an ordinary slide bottom up.

Now if we measure the power of an object-glass at its adjust-

ment for uncovered objects, and find that it agrees closely with its nominal power, it will show that the maker means what he says, whether it happens to be $\frac{1}{8}$th or $\frac{1}{15}$th.

Mr. Wenham further says: From an early date $\frac{1}{8}$ths, $\frac{1}{9}$ths, or $\frac{1}{10}$ths, and some now approach $\frac{1}{12}$ths in power (also something about steam-engines). Granted, but does all that prove anything except the truth of my position? Mr. Wenham has in this endorsed my statement a second time, and as far as it being the intention of the opticians to make $\frac{1}{12}$ths at the price of $\frac{1}{8}$ths, and so on, it is simply absurd. In my examinations of objectives of the different makers, I find that Tolles and Zentmayer keep very close to the standard, and I believe English opticians can do the same if they choose.

I wish to ask Mr. Wenham if he does not believe all good opticians *can* put their objectives right on the mark; that is, if they will; and when that is done let *quality* decide the question of superiority?

Museum of Comparative Zoology,
Cambridge, Mass., *Dec.* 26, 1871.

VII.—*Remarks on a Tolles' Immersion $\frac{1}{15}$th.*

By Edwin Bicknell.

I notice in the 'Monthly Microscopical Journal,' vol. vi., page 290, a Note on Tolles' Immersion $\frac{1}{8}$th communicated by Mr. Slack, from a letter written by Dr. Woodward to Mr. Slack; the concluding paragraph of which is as follows:—"I wish I could speak as favourably of Mr. Tolles' higher powers. They are very good indeed, but I have *yet to see one of them* which will rival the so-called $\frac{1}{18}$th immersion of Powell and Lealand." I have at present by me a Tolles' $\frac{1}{15}$th which was formerly a dry $\frac{1}{12}$th, and was "converted" into immersion in August, 1868: its power when the combinations are closed is $\frac{1}{15}$th, ang. ap. 170°; when the combinations are opened for uncovered objects its power is under a $\frac{1}{14}$th and its ang. ap. 155°; although somewhat under, I prefer to call it a $\frac{1}{15}$th.

I have this morning, December 25, 1871, by means of sunlight rendered monochromatic by the copper solution, and an achromatic condenser of 145°, stopped off all but the extreme oblique pencil, *resolved, counted, and measured* the three last and most difficult diatoms on Möller's "Probe Platte," *viz. Navicula crassinervis, Nitschia curvula*, and *Amphipleura pellucida;* these are all in balsam. I have a portion of the slide of *Amphipleura pellucida*, which (I believe) Dr. Woodward first photographed (which was

accidentally broken in Boston); this fragment of the cover is so mounted that part of the diatoms are in balsam and part of them are dry. These I have no difficulty in resolving, counting, and measuring, either dry or in balsam. I give here a Table of my measurements, by which it will be seen that the *Am. pellucida* on the Woodward slide is finer than that on Möller's "Probe Platte," and I find it a little more difficult in balsam.

Navicula crassinervis	78,000	lines to	English inch.
Nitschia curvula	82,000	„	„
Am. pellucida	87,000	„	„
„ Woodward	92,000	„	„

I have seen no account of *Am. pellucida* having been resolved and counted in balsam before. With lamplight and the above condenser I can resolve on Möller's "Probe Platte" up to and inclusive *Nitschia curvula*, the *Am. pellucida* having thus far refused to show its lines.

The objective as I usually use it with the B eye-piece gives about 1500 diameters: when I employ it in counting I use a B *positive* eye-piece and an achromatic concave lens in the draw-tube (amplifier), which just doubles the power, giving me 3000 diameters. It bears this "eyepiecing" well, and by using a Tolles' D solid eye-piece I can still see these fine lines at a power of 6000 diameters; such objects as *P. angulatum* are well shown with this power.

I do not consider this objective one of Mr. Tolles' best resolving objectives. He has made other objectives of both higher and lower power that will excel this one in this respect. This objective is quite as good by central light, has good working distance, adjusts for covers up to $\frac{1}{100}$th of an inch, in fact, according to Dr. Carpenter's comparison, more of a "roadster" than "race-horse."

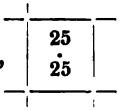
I make this communication in order that English, Continental, and American observers can compare the above results with their own work.

Museum of Comparative Zoology,
Cambridge, Mass., *Dec.* 25, 1871.

VIII.—*Maltwood's Finder Supplemented.* By W. K. Bridgman.

Maltwood's Finder is unquestionably of considerable service to the microscopist for his own individual use, but it requires the addition of several adjuncts to render it generally useful when required in the way of interchange. Thus, if a marked slide be sent to a friend at a distance, the latter has had, hitherto, no means of ascertaining how

far the indications of his own instrument shall coincide with those of the original observer by whom the marking was effected, consequently it would often happen that "there was nothing in the field." It is, hence, indispensable that he should have some means of determining, not only whether there be any discrepancy, but that he should be able to ascertain the exact amount and direction in which they may differ. This can be effected in a very simple manner. Take, for instance, a common gummed label and draw two lines from one side to the other at right angles so that they cross in the centre, and then at the point of intersection make a minute puncture with a fine needle. Next, place the instrument in position, *with the light central*, and putting on the "Maltwood" bring the central square into view, and let the centre of the square be as near in the centre of the field as possible, so that if a mark were to be made

then it would appear thus,  Now, take a common glass

slip and place it on the stage in the place of the finder, and having wetted the label place it on the glass and move it about until the puncture comes into the centre of the field. If this be accurately done the two ought perfectly to coincide, and therefore when this correspondence is tested under different instruments the variation of distance and direction from where the puncture really appears and where it *ought to be*, will show the different spots at which the same object will be found in each instrument. A test-slide *with the marker's name* accompanying slides sent for inspection will always afford a "key" for their successful examination; or where parties are in frequent communication, or interchanging objects, one might be kept in store by each correspondent.

The accuracy with which this central spot can be determined will, of course, regulate the degree of perfection which may be attained, hence it is desirable to have some mechanical means by which its exactness shall be measured. A very simple pointer may be made in a few minutes by almost any ordinary observer. From the B eye-piece unscrew the upper portion so as to expose the stop in the focus of the eye-lens. With a pair of compasses mark a circle on a thin piece of stiff card which, when cut out, will just *fit firmly* into the tube of the eye-piece, then mark another circle *a little larger* than the opening of the stop, and draw a line through the centre quite across. Next cut out the central portion and fasten down the ring on to a piece of white paper by two pins just above the line, and complete the line across from one to the other on the paper on to which the ring is pinned. A point midway of this line will now be the exact centre of the ring, and it only remains to fasten

some translucent substance which shall mark this spot and not be in the way of observation.

A thin transparent bristle out of a new tooth-brush I have found to answer the purpose very satisfactorily. Selecting one of the finest and straightest, cut a fine notch across the ring in the direction of left to right, and placing the hair in the notch with its inner end a little short of the centre, fasten it down at the other extremity with a small piece of gummed paper, and when dry place it in the eye-piece on the stop and replace the eye-lens as at first. Its accuracy may now be tested by rotating the eye-piece in its place, when the end will describe a very minute circle, in the centre of which is the real centre of the field.

My own pointer is movable. A small milled head is so cut that it turns through only about one-sixth of a circle, and hence at one extremity it brings the hair across the field, and at the other it carries it under the stop to the side so as to be quite out of view when not in use.

As regards the plan of notation, this will probably have been anticipated from the foregoing remarks; for if a dot be sufficient to indicate the centre of the field, it will answer equally well to indicate the relative position of any object in that field, and a dot in any part of a square will serve to note down the precise whereabouts in that square of any object of which it may be desired to preserve a record. Thus, any particular specimen being found, substitute the Maltwood and write down the two numbers contained *in that square over which the end of the pointer rests*, and make a dot in the same relative position to the figures, as $\frac{29}{41}\cdot$, or if it fall on the line, $\frac{29}{41}|$ mark the line as well either vertically or horizontally, but in all other cases the lines may be altogether ignored. The want of some *simple* and *easy*, yet *certain* method of recording, has always been a desideratum, but I find this plan leaves little more to be desired.

IX.—*On a New Micro-telescope.* By Prof. R. H. Ward.

This is designed especially for travelling and field use, but applicable to some of the daily work of the microscopist. It consists of a stand and accessories as follows: An ordinary tank microscope having the body focussed by a rack and clamped at any desired height upon a stand like that of the bull's-eye condensers. Probably few naturalists have any suspicion of the real usefulness of this little piece of apparatus, not only in the study of objects living in an aquarium or preserved in alcohol, glycerine, &c., but for the hasty inspection of herbarium specimens permanently fastened upon

sheets of paper too large for the stage of an ordinary microscope. and for the preliminary examination of objects in jars, boxes, dissecting troughs, &c. The writer always keeps upon his working table such an instrument carrying a double nose-piece (the crooked form) and usually one-inch and three-inch objectives, and uses it continually and with great satisfaction as a substitute for a simple microscope. The brass foot-plate at the bottom of the upright pillar should be made somewhat larger and heavier than usual. A stage of convenient size and simple construction, sliding upon the upright pillar and capable of being clamped securely in any position. This stage carries a diaphragm and mirror below and stage forceps above, and enables the instrument to be used as a vertical compound microscope for ordinary work when, as in travelling or on a field-day, no more commodious stand may be available. This combination may also be used as a dissecting microscope, though for that purpose it is greatly preferable to use the tank microscope as a magnifier only, and to place the object, if opaque, on the table, or if transparent, on the stage of any good dissecting microscope that may be within reach. A draw-tube sliding over the compound body from below, and capable of being fastened by a bayonet catch to the brasswork through which the compound body is moved by the rack. The objective in the compound body now acts as an erector, and another is to be screwed, by means of a large adapter, into the lower end of the draw-tube, to act as the objective. The rack-movement now only varies the power, and may be thus used as a fine adjustment, while the coarse adjustment must be gained by moving the whole instrument. This combination is extremely useful for dissecting, its greatest misfortune being that it is a monocular arrangement. A telescopic object-glass of one-inch linear aperture and four-inch solar focus, to be screwed, instead of the objective, into the bottom of the draw-tube. To this object-glass the compound body with its eye-piece and objective acts as an erecting eye-piece, and is focussed by means of its rack-movement. This combination gives a telescope of good working qualities, and of power entirely disproportioned to its size. A brass pillar about two inches long, having a steel transverse bar for a handle, and at its lower end a gimlet-screw to be fastened into a tree, post, or board. Into its upper end may be screwed the upright pillar of the tank microscope. The gimlet-screw may be made of steel, which is somewhat durable, or a common iron screw may be used, which easily wears out, but can be replaced at a nominal expense. This fixture adapts the instrument to field use as a microscope or telescope.

This instrument should be furnished with a compressor and two objectives,—a one-inch and a two-inch, two-thirds inch, one-half inch, or four-tenths inch, according to the wants of the owner.

Either of these objectives may be used as an erector, though the higher ones will seldom be preferred for this purpose. The four or five inch objective usually furnished with tank microscopes may well be dispensed with, as its effect is easily gained by means of the erecting arrangement. Microscopists who have laid aside the ordinary "erector" of the shops as an entirely unsatisfactory affair, need not, on that account, expect a similar failure in the use of a good one-inch or two-inch objective as an erector.

The cost of this instrument ought not to be large. It should not exceed sixty or seventy dollars to those already supplied with objectives and compressors, or one hundred dollars complete.—*American Naturalist.*

NEW BOOKS, WITH SHORT NOTICES.

The Micrographic Dictionary: a Guide to the Examination and Investigation of the Structure and Nature of Microscopic Objects. 3rd Edition. By J. W. Griffith, M.D., assisted by the Rev. M. J. Berkeley, M.A., F.L.S., and Rupert Jones, F.G.S., Professor of Geology, Royal Military and Staff Colleges, Sandhurst, &c. London: Van Voorst, 1871. Parts I. and II.—We must congratulate Mr. Van Voorst upon his effort to bring out a new edition of the 'Micrographic Dictionary.' It is a work which for years has stood without a rival, and in matter, illustration, and arrangement, it has left little to be desired. Of course, in making these remarks, we are referring to the book at the time of its last edition. Since that time the progress made has been very great indeed, especially as regards the microscope and its necessary apparatus. The work done also both by foreign and English histologists, in the time that has elapsed, has been very great; but that we have hardly to deal with in our notice of the present two first parts of the new issue, as they are really nearly entirely, though not exclusively, occupied with the account of the microscope, of its additional apparatus, of the methods of testing them to see that they are in perfect condition, and of the mode of examining the different objects which the microscopist may have to study.

Now this part of the work is of the utmost importance; for assuredly the Dictionary will be looked up to as an authority, and therefore its information should be full and complete. But we are sorry to have to express our opinion, at the very outset, that this is not at all the case. To this, of course, the editors can very fairly say, "We have given all the information that we consider necessary in the introduction; hereafter, under the special heading, each subject will be dealt with as fully as it merits." And this may be very true, and may possibly explain the deficiency of the information in the first two numbers of the Dictionary. We must say of it that, so far as it goes, it is excellently done. The writing is marvellously clear, and the subjects succeed each other in such order as to be perfectly intelligible. In fact, we know of no work which contains such admirable advice to the student in regard to the many difficulties of microscopic investigation. But when we look to see how far the edition has been influenced by modern work, we confess that the result is not at all satisfactory. For example, we find hardly any reference to the subject of photography in connection with the microscope. The same may be said of that beautiful instrument, the micro-spectroscope, which is dismissed in a couple of lines. And then, worst of all, the question of Diatomaceæ and their structure, and of Podura scales and the scales of Lepidoptera, have been treated of almost as one might who was writing the former edition of the book. These we consider very serious and important omissions from an Introductory chapter to a treatise on the microscope and its work; and though we may hope to see them laid before us in the future numbers, still we must regard it as

an error of omission that they are not given in the first place as an introduction to the student.

We think Mr. Van Voorst would have done well to have had another editor in addition to the three already selected, and that he would have found him easily in the Royal Microscopical Society we have not the slightest doubt—some one who, being constantly at the Society, was well acquainted with the various discoveries which have taken place of late years on the subject of Diatomaceæ, *et hoc genus omne*. And we hope that, if his engagements are not definitively concluded, he will not leave our suggestion without some serious consideration.

As to the contents of the book there is not much for us to speak about; but such as there is, is so far well done. We have gone over this part rather carefully, and we find that the authors have not left unnoticed what has been done of late years, some of their references being to books and papers published as late as 1870. We think the new edition, on the whole, a very fair one; and if the publisher does not absolutely scoff at our advice, we doubt not the future parts will be everything that the working microscopist can desire.

PROGRESS OF MICROSCOPICAL SCIENCE.

The Development of Amblystoma lurida has been recently worked out by Dr. P. R. Roy, an American naturalist. He leaves the development within the egg unnoticed, as he says there is no difference between it and the fish, which has been carefully studied already. The tadpole of this salamander emerges from the egg in twenty-five days. April 25, length one-half inch, colour olive, eye-spots visible, two short holders, gills rudimentary. May 5, tenth day, eyes developed, irids golden, colour greyish olive, with three faint transverse bands of a darker hue, gills plumed, no holders, fore legs starting; is now active, and feeds voraciously on aquatic insects. May 25, thirty days from the egg, fore feet tridactylous, consisting of thumb, forefinger, which is greatly elongated, and middle finger a little longer than the thumb. If there should be an arrest of development at this stage, the track would be bird-like. Next, the fourth finger makes its appearance, and, on the hind feet, the fifth comes still later. What is especially interesting is that when the legs or feet have been amputated, which frequently occurs, the operation being performed by those miniature fresh-water sharks, the larvæ of dragon-flies and water-beetles, the development of the toes is precisely in the same order, first the three toes, then the fourth, and on the hind feet the fifth. The gills are now beautifully plumed, and when closed reach to the centre of the entire animal; hind legs starting. June 20, fifty-six days, hind feet developed. As the lungs increase the gills wither and are gradually absorbed, so that by the middle of August

the gills have all disappeared. The time consumed in this wonderful process is a little over one hundred days.

The Affinities of Crinoids.—Professor Agassiz gives the following account of Herr Metschnikoff's recent researches on these animals. He quotes from the 'Bull. Acad. St. Petersburg,' p. 508, February, 1871. The researches are of considerable importance, as they throw new light on the affinities of the Crinoids. Thoroughly familiar, says Prof. Agassiz, with the Pluteus of Holothurians, Echini, Starfishes and Ophiurans, Herr Metschnikoff commenced the investigation of their earlier stages with the determination of tracing the presence of the peculiar water-system of the larvæ of the other orders of Echinoderms, what had been previously written by Busch, Allman and Thomson, on the early stages of Comatula, giving no data whatever bearing upon the subject. To his surprise he found no such water-system, nor could he trace anything in any way homologous to it; he also discovered that what constitutes the water-system of adult Crinoids, which has always been homologized with the water-system of other Echinoderms, is developed in a totally different manner. In the free-swimming Comatula larva the bag-like digestive sac is the only organ developed, it becomes the digestive cavity of the adult after the larva attaches itself to the ground. He noticed the tentacles as diverticula of the digestive sac in the interior of the larva; these subsequently force their way through to the exterior, at the time when the digestive bag has become further differentiated, and is provided with a mouth opening in the centre of the oval disk, and an anus opening not far from it on the side of the calyx. There is formed at this stage a large cavity which divides into two parts; the upper part, uniting the hollow tentacles at their base, forms the so-called circular canal, while below it, and connecting with it, we have a large cavity forming the perivisceral cavity, a mode of development of the circular ring and of the perivisceral cavity totally unlike that observed in Ophiurans, Starfishes, Echini, and Holothurians. Metschnikoff compares the mode of development of the upper and lower cavity to analogous processes in the embryonic growth of Alcyonella and other Bryozoa; he traces a striking similarity in the structure and position of the digestive organs and tentacles with similar organs of Bryozoa. However that may be, he has shown conclusively that the larva of Comatula has apparently nothing in common with other Echinoderm larvæ; but, says Prof. Agassiz, we must wait for his figures on this intricate subject before we can decide if the position he assigns to Crinoids is true to nature.

Monocotyledons the Type of Seeds.—A paper on this subject having been read by Mr. Thomas Meehan before the American Association at its last meeting, Dr. T. C. Hilgard made the following remarks upon it. He observed that the whole question came back to the laws of phyllotaxy. The very fact of these "genetic" numbers, as he had called them, required the second element to be derived from the first one; as all radial organs must be derived from their predecessors. The fact itself was apparent in the far-too-much neglected phenomena

of cryptogamous *developments* (or "embryology" of authors). The moss-spore proper (apart from the *Chlorospermeæ* as true moss-*spawns*), develops into a true land (or aquatic) Conferva. The latter bears a bud at the ends of its thread-like "prothallium." Each of its cells is generated out of a preceding one. A terminal cell enlarges into a conical leaf. *Out of that leaf* springs the second, at its base. It is in fact only on the supposition of radial organs generating their successors at the side of the *rift*—at the centre—alternating from either border (as in the case of the pod-leaves, producing fertile ovules), that the whole of phyllotaxic phenomena, and of organic numbers in general, becomes explicable. The production of new elements, however, takes place in a very embryonic condition. 'Cotyledons already formed do not *divide*. Lobes of fissures, folds, &c., of cotyledons are no divisions, but are due to unequal enlargement. New elements are not formed by division, but by sprouting.

Foraminifera of the Chalk of Gravesend and Meudon.—A very important paper on this subject has been contributed to the 'Geological Magazine' for November last, by Dr. W. K. Parker, F.R.S., our President, and Professor Rupert Jones, F.G.S. It should be read by those who are interested in the subject. The authors conclude by expressing the fact that the specimens figured in the 'Mikrogeologie' are for the most part very minute, such as lie among the finer débris of washed chalk; whilst those treated of by D'Orbigny were larger individuals picked out by means of hand-lenses from the coarser dust of the disintegrated material. The great difference of size, however, among individual Foraminifera carries but little weight in the determination of species; for the conditions, not only of growth, but of feeding ground, depth of water, and climate affect them so greatly, that a form which may be gigantic in one *habitat*, will be arrested or dwarfed in another, retaining all the essential characteristics of shape and structure which are required for its specific identification.

Peroxide of Hydrogen in Pus-globules.—In a paper read before the Medical Society of Victoria, and published in the 'Australian Medical Journal' for August last, Dr. J. Day calls attention to the value of peroxide of hydrogen in destroying the infectious nature of pus-globules. He has tried it in cases of small-pox with advantage. He shows its action on pus-cells to be extremely active.

Nerves of the External Ear of the Mouse.—Professor Schöbl gives an account in Schultze's 'Archiv'* of the above. He states that he knows no cutaneous structure throughout the entire mammalian series that is so richly supplied with nerves—not even the wing of the bat. And he goes on to say that he has discovered peculiar tactile bodies in it, and a remarkable pale terminal plexus of sensory nerves. If the cartilage of the ear be horizontally divided so that the ear is split into two lamellæ, no less than four nerve plexuses may be seen in each. Lying on the cartilage are the larger trunks, which usually divide dichotomously, and intercommunicate in seven different fashions.

* Band vii., Heft iii.

The second layer, like the first, is composed of medullated fasciculi, of smaller size, freely intercommunicating with each other, and lying immediately beneath the capillaries. The third layer is composed of still finer medullated fasciculi, and is on a level with the capillaries. Small fasciculi of this layer, composed of from two to four fibres, run to hair follicles, and, having encircled each with one or several turns, terminate in a little nervous coil, knot, or glomerulus at its base. The nerve knots are almost perfectly spherical, with a diameter of about 0·015 of a millimètre; and they occasionally include a few ganglion cells in their interior. Schöbl estimates the number of nerve knots at about 12,000 for each ear. Finally, from the last-named nerve plexus a fourth plexus arises, which is composed of pale fibres, and which lies immediately beneath the Malpighian layer. At their points of junction are well-marked nodal enlargements. The plates are exquisitely done, but we fear that their artistic merits are higher than their value as fair observations.

Experiments with Vibrating Cilia.—A very curious set of experiments on this subject is described by Professor Jeffries Wyman, M.D. They appeared in the 'American Naturalist' as long ago as last October, but we have not had space to name them. They are certainly very curious and interesting, and are so well described that our readers will have no difficulty in carrying them out. The first-described experiments are those made in water. For these the gills of Unios and Anodontas are well suited. Their cilia are quite active, and vibrate in such directions, that on the inner gill the motion is *from* the free edge, and on the outer *to* it, facts which the experimenter should keep in mind. If an *inner* gill is cut away from its attachment and laid on the bottom of a flat dish, its cilia acting as legs, it will soon begin to move with its *free* edge forwards, and will in the course of time travel the entire length of the dish. Prof. Wyman has seen a whole gill move ten inches in four hours. Under similar circumstances the *outer* gill will move with its base or *cut* edge forwards. This difference depends, as will be readily seen, upon the fact that the cilia of the two gills vibrate in opposite directions. The result of ten experiments gave the rate of motion of a piece of gill measuring 12 mm. by 14 mm., 6 mm. a minute. If two outer gills are laid with their free edges towards each other they will at once begin to approach, and it frequently happens after meeting that one crawls directly over the other. Another and more striking experiment which shows the reaction of cilia on each other may be made as follows. Fasten a gill to a piece of cork under water, and place upon it a portion of a second gill about a half-inch square. If this piece is so placed that the cilia vibrate in the same direction with those of the gill below, it will remain stationary, or nearly so, since the cilia offer no resistance to each other. If now the upper piece is reversed so that the cilia vibrate in opposite directions, the upper piece will move with double the speed and through twice the distance in a given time that it would with its own cilia alone, for while the lower cilia move the upper piece through a certain space, the cilia of the upper piece also move this in addition through an equal space. A third form of

this experiment consists in placing the upper piece so that its cilia vibrate at right angles to those of the lower. In this case, while the lower cilia tend to move the upper piece from side to side, those of the upper tend to move this lengthwise of the lower. The direction which the upper piece takes is a resultant one, *viz.* intermediate between the two. Then follow the experiments in air. Though the tissues of the gills of Unios and Anodontas are quite soft and incapable of resisting other than very light weights, they will nevertheless carry small disks of paper supporting a bristle, on the top of which is a small pellet of cotton or a flag of tissue paper. In order to show the flag more distinctly, a board painted black should be nailed to the edge of the one on which the gill rests, to make a background. With this precaution the experiment may be seen over a large room. To mark the distance traversed, a pointer of white paper should be set up on the board supporting the gill and at the beginning of the experiment, the end of the pointer brought in contact with the end of the flag on the gill. When left to itself, the disk on the gill with its flag at once begins to move to the opposite side, and the flag is seen to recede from the pointer. The distance traversed may be increased to several inches, by placing two or more gills side by side, the free edge of the first slightly overlapping the cut edge of the second, &c. The mucous membrane from the roof of the mouth of frogs is much more solid than the gills of Unios, and the cilia vibrate with much greater force. Different ciliated membranes exert very different degrees of force, but he has found none better suited for experiments than that just mentioned; especially, when taken from the mouth of the bull frog, which gives a large surface. It has the advantage, too, of keeping up its activity for twenty-four hours or more, after being detached from its natural connections, if only kept cool and moist. For moistening it water answers sufficiently well, but the serum of the blood of the frog is still better. The attention of the writer was first called to the possibility of moving weights much larger than was supposed possible by noticing the ease with which a piece of skin which was accidentally placed upon the ciliated membrane was swept off. By loading the piece of skin with weights the mass moved was found to be unexpectedly large. The author has devised a curious and interesting machine for demonstrating to a large class the amount of work which can be done by these ciliated surfaces. It is described in the article to which we have referred.

The Embryology of Chrysopa and its Bearings on Classification.—This is a most important and valuable paper, which was read before the American Association by Dr. A. S. Packard, jun. At one of the former meetings he dealt with the embryology of Diplax. Now he has taken up Chrysopa. He says he did not observe the formation of the blastoderm, but the blastodermic skin ("amnion") of Chrysopa is of the same structure as in Calopteryx. At the posterior end of the egg the round nucleated cells are crowded together in the same way as in Calopteryx. The primitive band is of the same general form, and floats in the yolk as in Calopteryx, but more as in Aspidiotus, though it rests more on the outside of the yolk than in those

genera, and the end of the abdomen rests on the outside of the yolk, rather than rolled in within the yolk; but that the germ is an endoblast (so far as that condition has any special significance) is shown by the fact that the ventral side of the primitive band points inwards towards the centre of the yolk, as in the Libellulidæ, the Hemiptera, and some Coleoptera (Telephorus and Donacia) in contradistinction to the Phryganeidæ and the Poduræ (Isotoma), in which the germ or primitive band floats entirely on the outside of the yolk. After the procephalic lobes and rudiments of the appendages of the head and thorax have begun to develop, a second moult (visceral layer) of the blastoderm is made, which envelops the head and underside of the body, much as in the Libellulidæ and Hemiptera. At this time the embryo is much like that of the last-named insects. The germ does not revolve in the egg, as in the Libellulidæ, but the head remains throughout embryonic life next the micropyle. At the next stage observed, the appendages of the limbs had appeared, the embryo being situated on the outside of the yolk, the end of the abdomen curved around on the opposite side of the yolk. At this time the inner or "visceral layer," forming a second moult of the blastoderm, envelops the germ, much as in the Libellulidæ, and Hemiptera, and Coleoptera (Donacia). It is evident that this *faltenblatt* of Weismann (or visceral layer of Brandt) is shed at a later stage than the "amnion" proper. This stage corresponds with that of Calopteryx figured by Brandt. At this time the germ of Diplax and Calopteryx (Libellulidæ) floats within the yolk, but this difference he would regard as having no special importance, as in the Hemiptera the germ at the same stage of development rests on the outside of the yolk in Corixa, while in the Pediculina, according to Melnikow's researches, the germ floats within the yolk, and in the Curculionidæ (Attelabus) the germ rests on the outside of the yolk (ectoblast), while that of Telephorus is a decided endoblast, *i. e.* floats in the interior of the yolk. After this period, the embryo of Chrysopa exactly corresponds to that of all the Libellulidæ whose development is known (Agrion, Calopteryx, Perithemis, and Diplax). The embryogeny of Chrysopa is identical, then, with that of the Libellulidæ. What becomes, therefore, of the distinction between the "Pseudoneuroptera" and "true" Neuroptera, insisted on by some of the leading entomologists, since Erichson's day? "Never believing that the differences were great enough to separate the Linnæan Neuroptera into two independent orders or suborders (whichever we may choose to call them), I now ask if embryology does not give independent testimony as to the close alliance at least of the Libellulidæ and Hemerobidæ, even if we go no farther?"

Relation of Brachiopoda to Polyzoa.—In his recently-published memoir on the 'Early Stages of *Terebratulina septentrionalis*,' Professor Morse thus discusses the relations which exist between the above two groups. With propriety may also be suggested a certain parallelism between the leading groups of the Polyzoa and the Brachiopods. We have forms like Lepralia, attached by one region of their shell, this shell being calcareous and exhibiting minute punctures, which have

been compared to similar markings in certain Brachiopods. So among the latter group do we find forms attached, as in Thecidium, and some species of Productus; and generally the articulate Brachiopods might be compared to such forms as Lepralia, while on the other hand, such genera as Pedicellina, with its long, pliant, and muscular stalk, or Loxosoma, with a stalk highly retractile, may be compared to Lingula. The limits or intentions of this paper will not allow any considerations regarding the relations of the Brachiopods with the other groups of the animal kingdom. "I have elsewhere expressed my belief that they are true articulates, having nearer affinities with the Vermes; and in view of the above relations of the Brachiopods with the Polyzoa, it is interesting to remark that Leuckart has for a long time placed the Polyzoa with the Vermes, and in a new edition of the 'Outlines of Comparative Anatomy' Professor Carl Gegenbaur removes the Polyzoa from the Mollusca, and associates them with the Vermes."

NOTES AND MEMORANDA.

The 'Archives de Zoologie expérimentale et générale.'—The first number of this periodical has, we believe, appeared. It is under the editorship of M. H. Lacaze-Duthiers, and the subscription list is 35 francs a year. It is to deal with general zoology, supported by the precise laws of morphology, deduced from the most minute histological inquiries, demonstrated by a long and continued study of evolution, and submitted to the control of experience. In a word, it is to deal with *Experimental Zoology*. It will be a quarterly publication, and its Nos. will appear in January, April, July, and October. Each number will contain from eight to ten sheets of matter, and five or six plates, done either on copper or lithographed, and occasionally coloured. It will have a special part for the review of foreign works. A better editor than M. Lacaze-Duthiers could not have been chosen for the serial.

Field Club Prizes.—The Early Closing Association announces, through Mr. Henry Walker, their Secretary, that they intend giving prizes, among other subjects in microscopy, to London naturalists, &c. We regret to see that the Royal Microscopical Society is excluded from the list of competing bodies. In answer to a communication from us, Mr. Walker replied that he would see whether the Royal Microscopical Society should not be included along with the various other microscopical societies, but as we have not heard from Mr. Walker for some time, we fear his efforts were unavailing. The Countess of Ducie is the lady who has given the funds for the establishment of the prizes, which are as follow:—5*l.* 5*s.* for the best List of the Ponds and other aquatic resorts, within fifteen miles of London, and the Microzoa found in them, in the twelve months

between ——— 1871, and ——— 1872, giving the locality of pond, the date of the visit, and the state of the weather at the time. 3*l.* 3*s.* for the second best ditto; 2*l.* 2*s.* for the third ditto. Total, 10*l.* 10*s.* The papers on Microscopy must not in any instance exceed in length two columns of 'The Times' newspaper Parliamentary Debates. Professional collectors and dealers are excluded from the competition. The prizes are intended exclusively for those with whom Natural History pursuits are solely the recreation of their leisure after business hours.

PROCEEDINGS OF SOCIETIES.*

Royal Microscopical Society.

King's College, *January* 3, 1872.

W. Kitchen Parker, Esq., F.R.S., President, in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations to the Society was read, and a vote of thanks given to the respective donors.

The Secretary announced that among the donations there were two volumes from the Lisbon Academy, the one containing a description of terrestrial and fluviatile mollusks of the island of Madeira, with papers of an anatomical nature, some descriptions of crustacea, &c.; the contents of the other volume being purely medical.

It was moved by Mr. Suffolk, and seconded by Mr. Shadbolt, "That Messrs. James Hilton and Henry Perigal be requested to act as auditors in the preparation of the accounts of the Society to be presented at the anniversary meeting."

The Secretary read the proposed list of officers for the ensuing year.

A paper "On the Development of Fungi within the Thorax of Living Birds," by Dr. James Murie, was read.

A vote of thanks was passed to Dr. Murie.

Mr. Slack said he wished to call the attention of the Fellows to one of the presents mentioned at the last meeting. He referred to the box of slides of various lepidopterous scales sent by the Chevalier H. de Cerbecq, which he had selected, as showing that the beaded appearances on the scales resulted from positive forms of a globular character, and not from any optical illusion. He (Mr. Slack) had carefully examined the whole of these slides, and he thought it might stimulate others to the same course if he stated what

* Secretaries of Societies will greatly oblige us by writing their report legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. 'M. M. J.'

he had seen. He therefore handed in a paper containing a report of his observations.

The thanks of the meeting were given to Mr. Slack.

Wm. Carruthers, Esq., F.R.S., then read a paper "On the Structure of the Stems of the Arborescent Lycopodiaceæ of the Coal-measures."

A vote of thanks was then accorded to Mr. Carruthers.

Professor Thiselton Dyer (addressing the meeting as a visitor) said he thought that the statements made by Mr. Carruthers would commend themselves to the Society without any support from him. In regard to one point, however, on which Mr. Carruthers had touched, he might be permitted to say a few words. Mr. Carruthers had the advantage of a wide acquaintance with recent plants; this was, however, far from being the case with all those who had written on fossil plants. Indeed, speaking generally, it was unfortunate for the advance of biology that two great a distinction was drawn between what was extinct and what was recent, and he thought it would be a very great advance if the divisions practically existing between palæontology and the study of recent organisms were entirely overthrown. He should like to see accomplished what had not yet been done, namely, the arrangement in our museums of recent and fossil specimens, whether of plants or animals, side by side, so that they could be studied simultaneously. Moreover the division to which he alluded did not exist merely in our museums, it was to be found in the literature of science also. Hence it happened not unfrequently that we meet with vegetable palæontologists still using terms and expressions in their descriptions of structure which were altogether obsolete in the eyes of botanists. For example, Prof. Williamson, at the last meeting of the British Association, proposed to approximate the classification of the vascular cryptogams to the division of flowering plants into exogens and endogens. But this division was now almost universally admitted to be untenable. In his endeavour to correlate the extinct vascular cryptogams with endogenous and exogenous flowering plants, Prof. Williamson was making use of distinctions which had no place in modern botany. Our great English naturalist, John Ray, laid the foundations of a natural classification of flowering plants by dividing them into Dicotyledons and Monocotyledons. De Candolle thought these two groups might be characterized more conveniently by the mode of growth of their stem; he substituted, therefore, for Ray's names, those of exogens and endogens. It had, however, been shown that De Candolle's views involved an entire misconception of the mode of growth. By the researches more especially of Mohl, it had been proved that Monocotyledons were really not endogenous at all; they might, indeed, be more properly described as acrogenous. Then, again, exogenous growth was by no means confined to Dicotyledonous plants. Mr. Berkeley mentioned something very like it in a lichen (*Ipnea*). It was well known to occur in *Lessonia*, the great sea-weed of the Southern oceans, and according to Ruprecht also, in the allied *Laminaria digitata* of our own shores. In *Dracæna*, an undoubted

Monocotyledon, there were regular concentric zones of circumferential growth. In fact exogenous growth was found in vegetable organisms of very different affinities, and was not wholly characteristic of any one. It appeared to be a provision which was correlate with the general form of the whole mass of vegetative organs, and was forced upon the plant, in fact, as a necessary condition of its mechanical stability. For classificatory purposes the terms exogen and endogen were now almost universally abandoned, and Ray's designations, which were found quite valid, were used instead.

As to *Lepidodendron* and allied extinct plants, he had also no doubt about the existence of an exogenous growth in them. Nevertheless they were undoubtedly cryptogamic. The significance of their exogenous growth was not that they had anything whatever to do with Dicotyledons, but that they were large growing plants with a big branching crown, and hence required a stem strengthened gradually by lateral increase. It seemed a waste of time to trace, in a vegetative adaptation of this kind anything like genetic affinity.

Dr. Braithwaite wished to express the great pleasure he had experienced in hearing the admirable paper which had been read by Mr. Carruthers. He hoped it marked a new era in the work of the Society, and that it might be taken as the type of a series of papers confined to single subjects. We had the microscope, which was the implement or tool of work, but there was little systematic use of it. We wanted details of the growths both in vegetable and animal kingdom; and in consequence there were many portions of structure not at all worked out. If a few workers were to concentrate their intellects, and bring their energies to bear upon some of the minutiæ of vegetable or animal growths, we should have a great many more valuable papers before the Society.

The President said, alluding to the remarks of Professor Dyer, that when Mr. Rupert Jones and himself were working together on the *Foraminifera*, they never drew any broad line of distinction between the recent and fossil forms. In his (the President's) collection, which contained five or six thousand slides of Foraminifera, the recent and fossil forms were all placed side by side. By so doing he found by far the greater number of forms were still extant. One type especially, the *Nodosarian* (an ancient but still common form), ran into such an extraordinary series of forms, that he had shown to Dr. Carpenter and Professor Huxley that *eighteen* genera had been made of that form, and that characters of three genera could co-exist in one single specimen. In regard to putting together a great number of fossils, which have received distinct names, if anyone had seen Dr. Carpenter's Introduction to the Foraminifera he will have found a form called *Dactylopora*. He (the President) had found a clue to *Dactylopora* in some caterpillar-shaped shells from India on the common clam. In examining the French tertiaries he found, besides the caterpillar-shaped forms, others that were perfect rings, and that in these specimens there were two or more rings conjoined. Mr. Rupert Jones and himself brought out a series of papers in the 'Annals'; and when Dr. Carpenter took up

Dactylopora, he (the President) put into his hands a very large collection of shells collected by Sir Charles Lyell, and he (Dr. C.) found still more wonderful forms of this type, supposed to be coral. The whole thing showed how the recent is connected with the fossil, when genus after genus, and species after species are put together into one category, and shown to be essentially one specific type. Dr. Carpenter, by his great skill in *Dactylopora* alone, had produced one of the most beautiful monographs in existence, an exact counterpart of the same thoughtful work in which Mr. Carruthers was engaged.

Dr. Royston-Pigott, referring to the paper of Dr. Anthony, published in the current number of the Journal, said, if an object be carefully illuminated by a central light, not overflooding it, so that all *milkiness* may be avoided, and if the observer took the trouble to insert different apertures of the diaphragms behind the object-glass, some very curious effects would be seen, and the remarkable phenomena obtained by this arrangement were worthy of notice. Dr. Anthony's paper had put him in mind of what he himself had seen in the battledore scale. He (Dr. Pigott) had noticed that if you have a very good $\frac{1}{8}$th, no new features would be displayed, and only the usual rows of beads would be seen; but when the aperture is gradually reduced, it becomes a very remarkable object; for you will not only see the large beads, but thousands of others, which stand up with remarkable precision, so much so that you can see them shaded black, and the whole scale usually only marked with parallel ribs studded all over with beautiful beaded effects. He wished to call the attention of the Society to the fact that, by reducing aperture, you may get some very singular revelations. If there were any photographers present that evening he would appeal to them to confirm the principle laid down by Sir David Brewster, who has said that you cannot get a true picture if you use a very large camera upon a small object. Dr. Pigott then described the distortions which would occur in a portrait where too large a camera was used, and which, according to Sir David's principle, were caused by the mixing up together of all the images caused by different areas of the camera lenses; so that the result of such a proceeding would be that a compound image would be obtained, but not a likeness. To show the portrait correctly, you must diminish the glasses, as nearly as you can, to the aperture of the eyes; and you then photograph the object nearly by parallel rays.

Dr. Pigott then illustrated the principle for which he contended by a description of the effects that would be produced by a number of cameras, all photographing one and the same head, at the same instant. The right ear would be shown by one, the left by another, and so forth. In the same way, if the object-glass be of very large aperture a great many views of the same elevated object under the microscope are all different, yet all these views are mixed up into one compound image. The eye of the observer was receiving rays from every side of a minute object at once, as well as from the top. He was looking at it from innumerable points of view at one and the same instant. But when the aperture was reduced, immediately the

surface of the battledore scale was seen studded all over with shaded granules or beads, which stood up in remarkable relief; the diminished aperture increasing the focal depth of vision.

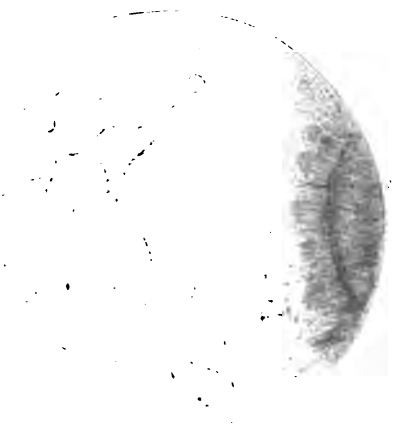
Note.—Dr. Pigott desires to add that he by no means wishes to disparage the value of large angular aperture. Microscopes as well as telescopes, under certain conditions, were however often improved in definition by reducing aperture.

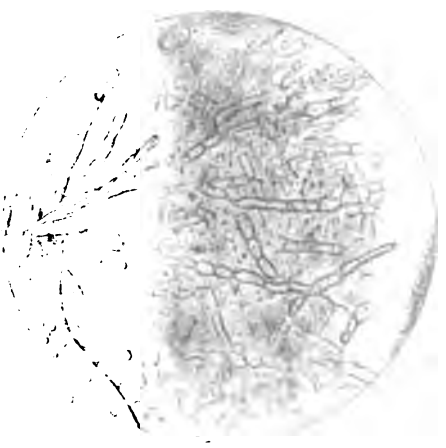
The meeting of the 7th February will be the anniversary meeting of the Society.

Donations to the Library, from Dec. 6th, 1871, to Jan. 3rd, 1872:—

	From
Land and Water. Weekly	*The Editor.*
Society of Arts Journal. Weekly	*Society.*
Nature. Weekly	*Editor.*
Athenæum. Weekly	*Ditto.*
Journal of the Linnean Society, No. 54	*Society.*
Memorias da Academia Real das Sciencias de Lisboa. Tomo 4. Parte 1 and 2	*Academy.*
Annual Report of the Brighton and Sussex Natural History Society, 1870–71	*Society.*
Journal of the London Institution, No. 9	*Institution.*

WALTER W. REEVES,
Assist.-Secretary.





THE

MONTHLY MICROSCOPICAL JOURNAL.

MARCH 1, 1872.

I.—THE PRESIDENT'S ADDRESS.

(*Delivered before the* ROYAL MICROSCOPICAL SOCIETY, *Feb.* 7, 1872.)

GENTLEMEN,

I come before you this evening to deliver the Annual Presidential Address with a mixed feeling. I have a timidity in attempting to represent microscopic lore in what may be termed the new phase, namely, the use of high magnifying powers and great complexity of apparatus. On the other hand, I feel confident in stating that in the use of what one may call antiquated microscopic powers, there is far more yet to be done than many of the Society imagine. High magnifying power is the rage; yet few are the researches but have been foreshadowed by workers in the early part of the present century. I lay much stress upon this point; and were I to refer to an example of one of the highest names connected with the microscope in this country, I should mention one of your early Presidents, JOHN QUEKETT. I take this man's work as an example of what can be done with low microscopic powers; although it is true that in substantiating detail, or in giving others clearness of conception, as to the intimate structure of tissues, he often illustrated his sketches of texture by highly-magnified objects. The greater number of his discoveries, of which not only the Microscopical Society, but England, may be proud, were made with low powers. This is known not only to myself personally, but to all who knew him intimately.

The microscope, ordinarily, may be said to be applied to the elucidation of fine organisms and minute tissues. It is plain, however, to everyone, that all organs, built up of textural parts, are more easily comprehended by viewing them in their broad phases, than by incessant reference to their component parts. The ordinary embryo, say, of fowl, frog, or fish,—from the size of a mustard-seed to a pea,—can be viewed sufficiently with a hand-lens or a very low power, to elucidate the organic relations of the changing protoplasmic mass. As growth proceeds, it is easy to perceive that each organ evolved necessarily enlarges; and the magnifying power employed might be said to be in inverse ratio to the size of the

object. At length a time comes when it can be no longer viewed with an ordinary object-glass; and to trace its evolution and development, we needs must have recourse to sections. It is mainly with the result of observations made on such *bisections*, as well as of *dissections*, that I wish to occupy your time to night.

The pleasure you have afforded me in appointing me to the honourable post of President has been dashed with a painful sense of unfitness, so little have I lightened the labours of the Council; nor can I be a proper annalist to my fellow-workers. The breathless ardour with which I have followed my own hunt has left me little leisure to give you an account of the country over which I have so hotly ridden. Thomson, speaking of himself, in his 'Castle of Indolence,' says,

"He loathèd much to write, ne carèd to repeat."

I must, however, excuse myself by saying that mine is *secondary*, and not primary, indolence; it is the idleness of labour. An anecdote may illustrate my shortcomings with regard to this unchecked research that leaves neither leisure nor patience for intellectual stock-taking. My kindly task-master, now in Egypt—I refer to Professor Huxley—gave me this scant praise at the conclusion of my long piece of work, the "Shoulder-girdle." "Parker, you're an idle fellow." "Why?" asked I, in astonishment. "Why? because you have made no *abstract*, and have left me to labour through that heavy mass of work, for any and every idea I want to extract from it."

Some recent conversations with "Friends in Council" have made me somewhat more easy about to-night's duty, for they have informed me that I need not speak of things outside my measure, that is, of other men's labours; but that I may give some account of my own.

My own work, Gentlemen, is somewhat ambitious; it relates to the highest types of animal life, and to the crowning part of their organization, namely, the "kingly-crowned head." Not relating, indeed, to all the wondrous structures that are centred there; but to the box holding the precious brain,—the basket carrying the organs of expression and of speech,—and the instruments used in those all-important functions, breathing and swallowing.

Thirty years ago my favourite author in these matters was Sir Charles Bell, a good type of the PALEY or *Fitness* school. Such good reverential men loved to trace the handiwork of an infinitely clever *Contriver*, who brought organisms into being in a moment—monads or whales—the plan of each having been perfected beforehand by a mode of thought peculiarly human. Looking from another stand-point, and yet reverent as they, I have no quarrel, even now, with this school. They but saw and understood things

in an *ex parte* manner, pleased with a fitness, delighted with a plan.

There are deeper meanings than these in the vertebrate organization; and as early as 1790 the great poet and morphologist, Goethe, began to get some glimmerings of a gorgeous morphological science unimagined by earlier observers and limited thinkers. In 1807 Lorenz Oken took up, worked out in his peculiar way, and went somewhat mad upon this most fascinating subject. He had several disciples, and the greatest of these was our own countryman, Richard Owen.

I could almost have wished that the "transcendentalism" which grew out of these oblique glimpses, partial and fitful, in the dawn of a new era of biological science, had never reached our shores, and that we had passed direct from the labours of the great "gradationalist," Baron Cuvier, to those of the German Embryologists, such as Baër, Reichert, and Rathke. These men did indeed lay a foundation, deep and wide, by their incomparable labours, for a true morphological knowledge of the Vertebrata.

Socrates was accused of corrupting the rising youth of Athens by questioning the truth of received axioms, and disturbing their unfinished minds with doubts. We may accuse a modern and native philosopher, our English Oken, of misleading students—I speak feelingly for myself—in the opposite direction, namely, by dispelling wholesome doubts by interpreting all the hard sentences of Nature for them, and by holding before their charmed vision an *Archetypal Idea*, instead of setting them to work at the various types, with scalpel, and lens, and microscope.

The inductions of this anatomist were made from particulars far too few on which to build so stupendous a theory; and meantime, the whole life-history of no single type had been worked out.

The facts all fitted, and fitted beautifully; a reason was given for everything; and two laws governed the creation of all the modifications of the Exemplar, namely, homology and teleology; when homology failed, then teleology came to the rescue.

In speaking of this justly-discarded method of morphological research, I am reminded of the words of Bacon in his "Advancement of Science," where he speaks of the "Invention of Sciences."

"The induction which the logicians speak of, and which seemeth familiar with Plato (whereby the principles of sciences may be pretended to be invented, and so the middle propositions by derivation from the principles), their form of induction, I say, is utterly vicious and incompetent: wherein their error is the fouler because it is the duty of Art to perfect and exalt Nature; but they, contrariwise, have wronged, abused, and traduced Nature. For he that shall attentively observe how the mind doth gather this excellent dew of knowledge,

like unto that which the poet speaketh of, *Aërei mellis cœlestia dona*, distilling and contriving it out of particulars natural and artificial, as the flowers of the field and garden, shall find that the mind of herself by nature doth manage and act an induction much better than they describe it. For to conclude upon an enumeration of particulars, without instance contradictory, is no conclusion, but a conjecture; for who can assure, in many subjects, upon those particulars which appear of a side, that there are not other on the contrary side which appear not?

"As if Samuel should have rested upon those sons of Jesse which were brought before him, and failed of David, which was in the field."

Hastening through this part, I must give the *last words* of transcendentalism, as uttered by an anatomist still living beyond the Great Water.

In the number of that useful periodical, 'Nature,' which appeared on the 28th of December last, the editor has solemnly and in good faith given the abstract of a paper read at the Indianopolis Meeting of the American Association for the Advancement of Science, by Dr. T. C. Hilgard. The title of this production is the "Numeric Relations of the Vertebrate System." I can only give you, as specimens, the head and tail of this monstrous production of a human mind—a production which doubtless cost its parent the severest throes, after which he must have "felt himself spent, and fumbled for his brains."

"There are five (not four only) complete neural rib arches to the cranium of all vertebrate animals, to wit—(1) The condylar or sensitive belt, with the condyle plates for side ribs, and the lower arch of the transversely bipartite occiput for its vault piece; (2) the petrosal or acoustic, containing the auditory nerves in its side beams (easily detected by removing the ear drum of felines, &c.), and overarched by the interior belt of the occipital squama; (3) the parietal belt, originally containing the true gustative of fixed tastes (sour, sweet, salt, and bitter), the glosso-pharyngeal, in an incision, from which it is, however, soon crowded out by the internal carotid artery, and the overlapping 'acoustic rib blade.' The next (4) is the optic or frontal, visibly succeeded, in fishes, by (5) the ethmoidal or olfactory vertebra. The rest of the cranium is formed by its 'extremities' or prehensile appendages."

That is the *head* of the theory: now for the *tail*.

"The vertebral blocks, as well as the ribs, are the product of the primitive axial series of (invertebral) discs, which, when completely arrayed, each bear five branches—viz. two pair of haemal arches, two pair of neural arches, and a fascicle of parallel cleets, so to speak, which being cemented together, both in the front and rear, by the superficial ossification of the discs at either end, are fused into the

block pieces, as found, e. g. in the young hog, the cementing slab covering the big neural rib head likewise, and not only the pentagonal prismatic block. The first disciform ossification we find in the corals, forming cribrose ethmoidal discs, such as the closely set 'sigillate impressions' of the *Astræa*, and afterwards left behind as the coccyx, e. g. of Byathophyllum" (*sic*).

I hope, like Richard Baxter's friends, we shall have no more "*Last* Words"; but that this dying song of a false philosophy, which has been wafted to us across the Atlantic, will soon cease its echoes; and that our attention may be no longer diverted from honest work.

And now I must acknowledge, with some shame, that the noble band of workers who have laid a sure foundation of vertebrate morphology are, with one or two exceptions, foreigners; mostly Germans. I have mentioned three of these, Baër, Reichert, and Rathke; but I must also tell of Müller, Hallmann, Köstlin, Remak, Vogt, and Agassiz, and more recently Kölliker, Max Schultze, and Gegenbaur. Our great Scotch anatomist, Goodsir, was unfortunately entangled in the meshes of transcendentalism, and his most important paper on the Morphology of the Skull, which appeared in the 'Edinburgh New Philosophical Journal' for 1857, although rich in ideas, is yet a hopeless mixture of observation and speculation. Just at this time, when for us there seemed no hope of native workers who should emulate the Germans, Huxley suddenly arose.

Some of you, I dare say, have seen a small modest-looking paper in the 'Proceedings of the Royal Society,' entitled "The Croonian Lecture on the Theory of the Vertebrate Skull." It was delivered before the Royal Society on the 17th of June, 1858, by Professor T. H. Huxley.

That memoir has been before me continually, and I have read therein day and night; it is a lamp bright with pure, intellectual light; or it may be compared with the mustard-seed of the sacred parable, which, though inconspicuous, is destined to grow into a fair and large tree. I will now give you the author's account of the importance of Development—morphology—as compared with a mere collation of the structure of adult forms—the Gradational Method.

"The study of the gradations of structure presented by a series of living beings may have the utmost value in suggesting homologies, but the study of development alone can finally demonstrate them.

"Before the year 1837, the philosophers who were occupied with the Theory of the Skull confined themselves almost wholly to the first-mentioned mode of investigation, which may be termed the 'method of gradations.'

"If they made use of the second method at all they went no

farther than the tracing of the process of ossification, which is but a small, and by no means the most important, part of the whole series of developmental phenomena presented by either the skull or the vertebral column. But between the years 1836 and 1839 the appearance of three or four remarkable essays, by Reichert, Hallmann, and Rathke inaugurated a new epoch in the history of the Theory of the Skull.

"Hallmann's work on the Temporal Bone is especially remarkable for the mass of facts which it contains, and for that clearness of insight into the architecture of the skull which enabled him to determine the homologies of some of the most important bones of its upper arch throughout the vertebral series.

"Rathke showed the singular nature of the primordial cranial axis, and Reichert pointed out in what way alone the character of its lower arches could be determined. For the first time the student of the morphology of the skull was provided with a criterion of the truth or falsity of his speculations, and that criterion was shown to be Development."—P. 5.

From the same treasure-house I extract the following never-to-be-forgotten axioms:—

"1. The notochord of the vertebrate embryo ends in that region of the *basis cranii* which ultimately lies behind the centre of the basisphenoid bone.

"2. The *basis cranii* is never segmented.

"3. The *lamina perpendicularis* of the ethmoid has the same morphological value as the presphenoid.

"4. The petrosal has the same morphological value as the mastoid; if one is not an integral part of the skull, neither is the other.

"5. The nasal bones are not neurapophyses.

"6. The branchial arches have the same morphological value as the hyoid, and the latter as the mandibular arc.

"7. The mandibular arc is primitively attached behind the point of exit from the skull, of the third division of the fifth nerve.

"8. The premaxilla is originally totally distinct from the palato-maxillary arcade.

"9. The pectoral arch is originally totally distinct from the skull."—P. 53.

Having endeavoured to give honour to whom honour is due, I will now, with permission, speak of my own researches.

On the 28th of June, 1860, I read before the Zoological Society, an abstract of my paper on the Osteology of *Balæniceps Rex*. I was at that time working out the subject *gradationally*. But whilst this was in hand, my friend Dr. John Harley showed me what Professor Huxley had been doing, and how completely he had broken up the idols of the transcendentalists; Professor Rupert Jones at the same time lent me the Croonian Lecture.

The commenced work was set aside; the microscope was brought

out; sitting hens were laid under contribution; and the first chick's head yielded me the "basi-temporals" in a distinct condition. This initial piece of work was highly praised by Huxley. Since then no step has been taken, no observation made, without the microscope; every organ, every tract of cartilage, every fibrous band, and every ossicle, has been subjected to microscopical examination, assisted by proper chemical reagents and solutions.

My friend Huxley himself showed me how to use caustic potash, and glycerine; and another valued fellow-worker, Mr. Henry Power, taught me the value of chromic acid. At the kind suggestion of Dr. Sharpey and Professor Huxley, the Royal Society voted me a valuable microscope. Thus have I been helped and befriended on every side, and to the pleasures of science have been superadded the pleasures of friendship.

Let it not be said, Gentlemen, that scientific men are a quarrelsome tribe, my own experience contradicts such an aspersion; for during all these years there has been to me "neither adversary, nor evil occurrent." My old master, Professor Owen, laughs gently at my weakness in going over to the enemy; he wonders at the folly of the man who can take a Huxley for his "guide"; yet, "although he hath laughed on me, I believe it not."

I hope that my mention of the *Balæniceps* paper will not lead any Fellow of this Society to refer to it; it was written in my younger days, and its composition is so peculiar, that my most intimate friends "hide their minds" when it is spoken of in my presence. Its style is said to be the evolution of a new species of writing, and is supposed to be unconformable with any type known to scholars generally. Professor Huxley thought it sufficiently grotesque to deserve a new specific name of "Parkerian." Eleven years have elapsed since then, and my literary infants still retain a peculiar and somewhat *ursine* form; nevertheless, I have laboured hard to lick them into elegance. Two years after the first paper, it occurred to me to take up the great gallinaceous group of birds developmentally, to be illustrated by collateral embryological work. I had been incited to this by finding that one group of these birds, the Tinamous, were essentially ostriches. That was really a discovery in this country; but Sundevall, the Swede, had already said of *Tinamus*, *Rhynchotis*, and *Crypturus*, "Struthiones parvos referunt!" That which delighted me most, however, was to find that sea-gulls were plovers in their infancy, and became specialized into their own type afterwards. This was a germinant idea, and many of you know what fruit it has borne through the rich manuring given to it by Professor Huxley. The result may be seen in his paper, in the 'Proceedings of the Zoological Society' for 1867, on the Classification of Birds.

Working on, in the way I have described—largely with the micro-

scope,—tracing the structure of the skull in the ostrich tribe,—doing a little side-business on the matter of the shoulder-girdle and breast-bone,—I was at last brought to a stand by my guide, who made me solemnly promise that I would close-in upon the chick, and look at nothing else until that type was worked out. I obeyed; and being already, from the influence of the Foraminifera, an evolutionist, my eyes *would* see evidences for that doctrine of creation. I shall not enter into details; but I will reproduce the conclusions I came to in the paper on the Fowl's Skull, in the 'Philosophical Transactions,' for 1869, pages 803, 804. These views, thus leading to evolutional doctrine, are given in the concluding remarks of the recapitulation, and through the influence of my guide are, I believe, clearly expressed. I wrought much with Professor Huxley in the matter, who is not only clear-headed himself, but is the cause that clear-headedness is in other men. My remarks are as follows:—

"Having thus, with as much expedition and brevity as I have been competent to, run through the ten arbitrary but not unnatural stages in the morphological history of the fowl's head, I would conclude by hinting at the importance of the various isomorphisms displayed by the skull and face of this one type in its stages of growth.

"I have described it upwards, but my long and really anxious labour has been in the opposite direction; the stages were traced from that of the old bird downwards to that of the chick of the fourth day of incubation.

"Whilst at work I seemed to myself to have been endeavouring to decipher a palimpsest, and one not erased and written upon again just once, but five or six times over.

"Having erased, as it were, the characters of the culminating type—those of the gaudy Indian bird—I seemed to be amongst the sombre grouse; and then, towards incubation, the characters of the sand-grouse and hemipod stood out before me. Rubbing these away, in my downward work the form of the tinamou looked me in the face; then the aberrant ostrich seemed to be described in large archaic characters; a little while and these faded into what could just be read off as pertaining to the sea-turtle; whilst, underlying the whole, the fish, in its simplest myxinoid form, could be traced in morphological hieroglyphics."

Since arriving at these conclusions I have gone still farther downwards in search of morphological facts; *down* as to type in the frog and salmon: and *down* in the stages of growth, working out in these fishy types the very earliest recognizable stages of the skull, face, and sense-capsules.

As to the mode in which these organisms have been worked out, I may perhaps say that I have been obliged to train myself, not only to be able to appreciate the whole histology of the matter, but what is still more difficult, to learn the meaning of *masses* of tissues, which form the rudiments of the various parts; whilst, as yet, the

tissues are of an *indifferent* kind. The result of this morphological research appears to me to be the gathering together of a mass of evidence in favour of the theory of evolution. I have already spoken of the stages of the fowl's skull, and what they point to; and I am at present engaged in working out the facial arches and nasal capsules of carinate birds, generally, so that the paper on the Fowl, with this supplementary work, may serve as a memoir on the *Bird's* Skull, and not that on the *Fowl*, merely. What do I find?—that the whole group of existing birds, excluding those half-ostriches the Tinamous, as well as the genuine ostriches, form a mere Order, as neat and compact as the Lacertilia among the reptiles. Once a small distance *above* the ostriches, and the life-tree forks and re-forks, and very few steps upwards have to be made before we arrive at the most accomplished types. From that monster bird, the *Dinornis*, to that feathered bee, the humming-bird, there are but a few and easily-traced stages; and from the ostrich, that plumed camel, up to the most perfect birds of the group—

I.

"The ousel-cock, so black of hue,
With orange-tawny bill,
The throstle, with his note so true,
The wren with little quill,

II.

"The finch, the sparrow, and the lark,
The plain-song cuckoo gray,"—

* * * *

The distance, I say, between that giant and these exquisite types (by way of the tinamou, the hemipod, and the songless Passerines of the New World) is very small indeed. Finally, for I fear you are growing weary, the frog, after having passed its simple and primordial stage, passes into a state between the lamprey and the chimæra, and then skipping past the osseous fishes and tailed Amphibia, foreshadows the mammal in its wondrous metamorphosis.

As to the salmon, the *simple stage* once passed, it gets, even before hatching, on to the level of the sharks and rays; in a week it is a Ganoid, and in a few weeks attains a sub-teleostean type—a condition somewhat below the highest kinds of fish, such as the Percoids.

It is said that proper Teleostean (osseous) fishes are not known below the Chalk; yet the Plagiostomes (rays and sharks) are found with the Ganoids in the Upper Silurian deposits—the Plagiostomes, however, being the lower types. All these things "atone together"; and, as far as we know at present, the life-history of each individual of a high type is a repetition of the evolutional progress in the ascent and modification of the vertebrate forms from the *Beginning*.

II.—*Mycetoma: the Fungus-foot Disease of India.*

By Jabez Hogg, Surgeon to the Royal Westminster Ophthalmic Hospital, Hon. Sec. R.M.S., &c.

(*Taken as read before the* Royal Microscopical Society, *Feb.* 7, 1872.)

Plate X.

The specimen of Fungus-foot disease I wish to bring to the notice of the Society was lately received from Dr. Blanc, of H.M. Indian Army, Surgeon in charge of the Rajkote Hospital. The points of interest in connection with it are, first, the disease, which was found to be confined to the sole of the foot, occurred in an unusually young person, a native of 18 years of age, who had previously enjoyed good health; secondly, soon after coming into hospital Mycetoma was diagnozed by Dr. Blanc, who on submitting a very small piece, which came away in a poultice, to microscopical examination, observed well-defined filaments of the fungus; and lastly, the affected part of the sole of the foot being immediately excised, the wound rapidly healed, and in a short time after the patient was able to leave the hospital cured.

The specimen, preserved in strong spirits of wine, presented a hardened, shrunken appearance when it came into my possession. As this was the first time I had had an opportunity of examining the disease in its earliest stage, I was little inclined to cut it about, and destroy its appearance. A small cut had, however, been made across the slightly prominent and discoloured centre of the mass, as represented in Fig. 1. This opening I carefully enlarged, removing small portions for microscopical examination; but finding nothing except quantities of fat-corpuscles and connective tissue, I dissected away a good deal of the surrounding structures, and came down upon two or three blackish-looking minute spots. With a low power, an inch and a half, and condensed light, I made out a group of globular bodies, exactly like balls of soot, or one of the *Smuts* of the Ustilaginous species; represented at Fig. 2, *a*.

EXPLANATION OF PLATE X.

Fig. 1.—The section from foot: one-half the original size; showing diseased spot.

„ 2.—*a*, Nest of soot-ball bodies, magnified 50 ×. *b*, A portion of one of same after boiling in liquor potassæ, pressed out on a slide; exhibiting molecular and resinous matter; magnified 150 ×.

„ 3.—Another portion, showing at *c* fungoid threads; magnified 350 ×. *d*, Amœboid bodies and fat-corpuscles, magnified 650 ×. *f*, Fat-globules and crystalline or resinous matter.

„ 4.—Another portion, showing well-defined dissepimented cells, imbedded in homogeneous colouring matter; magnified 650 ×. *g*, A detached spindle-shaped thread, surrounded by fat-corpuscles; also magnified 650 ×.

On removing some of these small concretions, which proved to be too intractable for further microscopical examination, I placed them in a test-tube, covered them with liquor potassæ, and subjected them for a few minutes to a boiling heat. A small amount only of the colouring matter was dissolved out, but soon fragments were found to be soft enough to break up on a glass slide. A drop of glycerine solution was then added, and a thin glass cover placed over all. With a power of 350 diameters, I first observed numerous detached fragments of an orange-coloured resinous substance, a number of fat-globules and discoid bodies, with granular matter. On carefully focussing and illuminating the specimen by direct light, articulated filaments were seen imbedded, and slightly projecting beyond the edge of the coloured mass; represented at Fig. 3, *c*. When more magnified these were converted into free loops, not unlike papillæ. The fungus threads were for the most part exceedingly minute; there was a compressed, or fossilized appearance about them, if I may so express it.

On increasing the magnifying power to 650 diameters, these threads were resolved into long, jointed, dissepimented cells; some branching out (represented in Fig. 4) and attaining to a considerable length, while others terminate in an enlarged ovoid head, probably a spore receptacle, containing one or more spores. In others, again, a minute oil-globule apparently occupied the centre; but it is not easy to determine this point, from the large quantity of colouring matter present. A peculiar budding out was noticed in some of the globose cells (represented in Fig. 3, *e*), and a few bodies separated away from the coloured mass, were of a paler colour, and partaking of an amœboid form (Fig. 3, *d*).* These later somewhat reminded me of Haeckel's Leptocytode; from the cytode of which this histologist says: a homogeneous membrane is differentiated from the granular contents; prolongations thrust out, and ultimately becoming a free-moving body, a *Protamœba primativa*. The walls of the threads appear in some instances thick, while those which were separated from the homogeneous matter were exceedingly thin and transparent. Notwithstanding the boiling in liquor potassæ, large quantities of fat-granules continued to float about, and the carbonaceous colouring matter was not nearly removed. The growth in some particulars, save that of colour, appears to partake more of the nature of a confervoid plant in its simple articulated threads and cell multiplication, than of a "truffle-like fungus." But as "one swallow does not make a summer," neither does one examination enable me to write or speak with much authority on fungi. I should prefer, in all examinations of

* Dr. Carter has I believe described, although I have not been able to refer to his paper, "Amœboid bodies," in connection with an advance stage of the fungus-foot disease.

the lower forms of life, to see a hundred examples of the same organism present before asserting anything decidedly about it. I must, however, add, so far as I am able to draw a conclusion from the specimens of the fungus-foot I have examined, that, from the relatively small proportion the fungoid filaments bear to the diseased mass, the fungus can only be regarded as a secondary product; one which may aggravate disease, but can hardly be said to originate it.

III.—*The Advancing Powers of Microscopic Definition.*

By Dr. Royston-Pigott, M.A. Cantab., F.C.P.S., F.R.A.S., M.R.I., F.R.M.S., formerly Fellow of St. Peter's Coll., Cambridge.

Part II.

(*Continued from page* 68.)

It seems unnecessary to describe further the complicated beaded system of insect scales distinguishable by the most perfect glasses now made, notwithstanding the very beautiful specimens sent over by The Chevalier de Cerbecq, which I strongly recommend to the attention of our Fellows. I therefore pass on to notice more particularly "advancing powers of definition" illustrated by Mr. Slack in his interesting paper on the Pinnulariæ, which gives a most valuable confirmation of my views as to the inadequacy of the old-fashioned glasses to deal with the higher order of definition. As I had experienced considerable difficulty in developing the beading of Pinnulariæ with an 1862 ⅛th of Powell and Lealand four or five years ago, I was pleased to receive a statement from Mr. Slack, that with his new immersion ⅛th of Powell and Lealand he had completely succeeded in resolving the *costæ* into component beading: at this time I did not possess the new glass. And I very much question whether old-fashioned glasses will resolve them now. Speaking of this beading, Mr. Slack sagaciously observes (even when using the new ⅛th) in his paper:—

"When the best has been done with any objective, it becomes evident that a slight increase of the difficulty, from greater minuteness of structure, would render it invisible and make the surface look plane."

It is noteworthy that to most glasses the interspaces between the beads of the Formosum and of the *Podura curvicollis Testscale do look quite plane* and free from structure.

"An examination of the Pinnulariæ on Möller's type slide, led to the belief that the costæ of such diatoms as *P. viridis, nobilis*, &c., had been misunderstood. That instead of broad, insolvable ribs, a truer view exhibited fine lines of beads springing from the median band."

Those who wish to succeed, with inferior glasses, to develop Podura and Lepisma beading composing the ribs (corrugations or rumplings seem terms rather indistinct to give a good idea of truly formed ribbings), would probably be assisted by practising the resolution of the ribs shown by the *Pinnulariæ*, and such diatoms as *Surirella gemma*. Mr. Slack also says:—

"P. nobilis.—Very difficult except in lucky parts and valves. . . . Median band beaded in complicated pattern.

"P. PEREGRINA.—Median band and costæ resolvable into rows of beads.

"P. LATA.—Median band consists of rows of fine beads at right angles to costæ.

"P. DIVERGENS.—With D eye-piece the $\frac{1}{8}$th resolves the median band into rows of beads which curve round the expanded ends of the two furrows."

Colonel Woodward kindly informs me that he has not yet been able to see the median bands of either Formosum, Angulatum, or Rhomboides resolved into beads; but after this feat by Mr. Slack, I trust he will be encouraged to produce photographs of the beaded median line of diatoms, notwithstanding they were somewhat confidently denounced as spurious after a statement had been made by the writer in the following words:*—

"In my hands an immersion 1-16th has displayed the beading of which the median ribs of the

Formosum (*a*) (diameter, three to one)
Angulatum (*b*) (diameter, three to two)
Rhomboides (*c*) " "

appear to be composed. Very curiously the diameter of these beads bore a different proportion to the general beading in each case, as above indicated—a confirmatory fact for the truth of their existence, and negativing the supposition of their being spurious, and guaranteeing their integrity."

The Advancing Definition of Lined Objects.

As it may be laid down as an axiom that in all optical glasses there is a residuary spherical aberration of either the first, second, or third order, the very finest glass now made bears to the very finest and most delicate test-object somewhat of the same relation that ordinary glasses bear to the rougher tests.

The ordinary glass showed lines; the extraordinary, beads. So where a splendid glass now only shows lines as in the *Amphipleura pellucida*, a glass of transcendent quality will by-and-by resolve this again into its components.

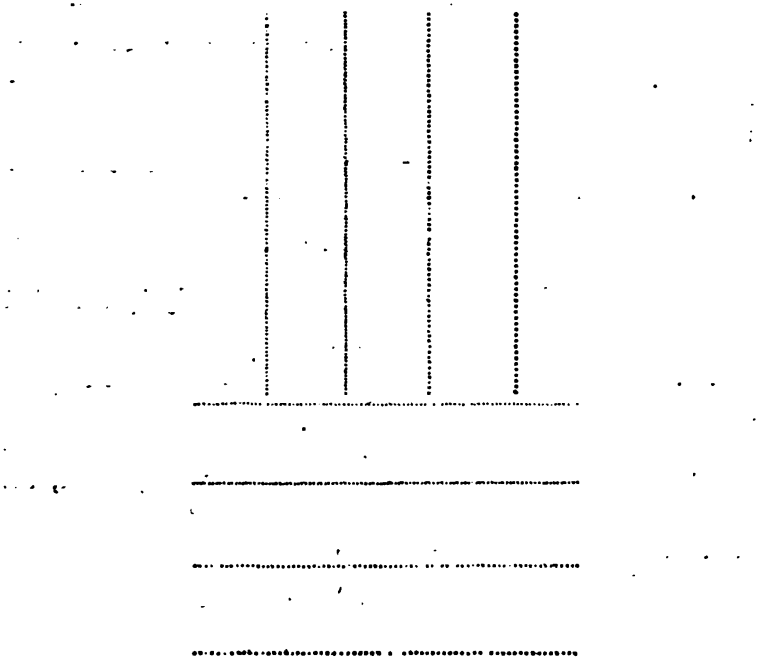
A Tolles' $\frac{1}{15}$th immersion, with monochromatic light, has shown the *Amph. pellucida* with 87,000 lines to the inch, counted by Mr. Bicknell, of Cambridge, U.S., though mounted in Canada balsam. Messrs. Powell and Lealand show a much finer scale than this, without monochromatic light, with their $\frac{1}{18}$th.

A very great source of difficulty is the spurious set of lines which haunt every fine line shown in the best microscopes, even when the corrections are pushed to the finest point of adjustment.

* November number of this Journal for 1870. First described in 'Pop. Science Review,' April, 1870, Plate.

Colonel Dr. Woodward very justly remarks that the causes of these false appearances have puzzled the best opticians of the age. The character of these lines I have observed to vary with different instruments, and in inspecting NOBERT'S lines, each objective seems to have its own peculiar set of spurious ones.

The great difficulty of resolution presented to the human eye by lines formed of dots is well illustrated by the following diagram:—



If the observer shuts one eye and holds the paper at ten inches, he will find that one of these lines is better resolved at a certain angle of position by turning the paper gradually so as to incline the line more and more to the vertical. This simple experiment illustrates the difficulty of resolving *lines* (really composed of minute dots), into their components. *In Nobert's lines*, of course no component dots exist, but the spurious lines are developed in profusion. The photo-picturing of these lines, however, seems to present less difficulty than diatoms, which with the same degree of fineness probably possess a very complicated structure. The finest *Amph. pellucida* photographed is given, I believe, at

92,000 lines per inch.
NOBERT'S XIX BAND .. 112,000 „

Mr. Bicknell says, "With lamplight and the condenser I can resolve on Möller's Probe Platte (Type plate) up to and inclusive *Nitschia curvula*; the *Am. pellucida* having thus far refused to show its lines."* The advancing power of the microscope is demonstrated by the readiness with which Powell and Lealand show these lines with their 1–16th and lamplight. Mr. Bicknell measures his specimen at 87,000 lines only per inch. Powell and Lealand at (I think) about 100,000. But specimens vary very much in difficulty and fineness.

The *Surirella gemma*, ordinarily a line object, gives up beautiful beads. In the photographs presented to me by Colonel Woodward I counted with a pocket lens *thirty beads*, and carefully inserted the points of a pair of fine compasses into the centre of the *first* and *thirty-first* bead so as to give the exact measurement of thirty beads. This I found to be on Photograph xvii

$$1 \text{ inch and } \frac{125}{1000} \text{ or } 1''{\cdot}125.$$

The power marked on the card was 3100 (probably by a clerical error), which gives

83,000 beads per inch, nearly.

This appears rather too great. Fortunately the Colonel has sent me two photographs of the same diatom. The larger is not quite $2\frac{1}{4}$ times the size† of the smaller. If therefore the magnifying power employed be as stated upon the smaller one, 1034, the second photograph is magnified not 3100 times, but

2200 times,

which gives nearly

59,000 beads per inch.

IV.—*The American Spongilla, a craspedote, flagellate Infusorian.*

By H. James-Clark, A.B., B.S., Prof. Nat. Hist., Kentucky University, Lexington, Ky.

Plate XI.

The argument of Haeckel, and others, that the Sponges are essentially compound Polypi, is virtually based upon the assumption that the minor (afferent) and major (efferent) ostioles of the former correspond to the mouths of the latter; and that the profusely branching afferent and efferent canals of the Sponges are strictly

* Page 71, No. xxxviii., this Journal.
† Nearly as 347 to 162, by actual measurements.

Fig. 4

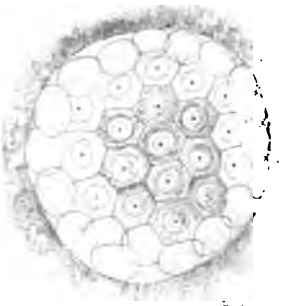


Fig. 3

Fig. 5

Fig. 1

comparable with similar canals in the polypidom of Halcyonarians: and, by implication, that the cilia-bearing cells of the interior living-wall of the Zoophyte find their homologues in the ciliated, cell-like bodies of the interior chambers of the Porifera. If, now, it should turn out that these last are not altogether mere cell-components of a tissue, but are each, severally, an independent body, although closely connected with others in a common bond, then the attempted *parallelism* between the two groups must utterly fail of confirmation. The tendency of Carter's later investigations, and our own too, is to show that this is no vain supposition.

For ourselves, we hold that each *ciliated body* of the Sponge is a *cephalic* member (a *cephalid* in this case) of a polycephalic individual.* We believe, as far as we can understand his undecided, rather hesitating position, Carter's latest decision is, that the Sponge is a community of Amœbous individuals,† and not a

EXPLANATION OF PLATE XI.

Spongilla arachnoidea Jas.-Cl.

The following letters apply to identical parts in all of the Figures:—*a*, Investing membrane: outer division.—a^1, Sectional profile of the cytoblastema of *a*.—*b*, Cells in the thickness of *a*.—b^1, Cells (like those at *b*) about the spicules (*e*).—b^2, Cells of the investing membrane, with their nucleus; a surface view.—b^3, Temporary junction (by contact only) of the outer (*a*) and inner (*c*) divisions of the investing membrane.—*c*, Investing membrane; epithelioid inner division, in sectional profile.—c^1, Interspaces between monad chambers.—*d*, Junction of the divisions of the investing membrane along the spicules.—*e*, Larger spicules.—e^1, Smaller spicules.—*f*, Circulatory apartment.—*g*, Monadigerous mass.—*h*, Monad chambers and monad groups.—*i*, Aperture of *h*.—*j*, Monads, or the body proper in Figs. 3 and 3*a*.—*k*, Cylindrical collar of *j*.—*l*, *Flagellum*.—*n*, Nucleus.—*o s*, Minor ostioles.—*v*, Contractile vesicles.

Fig. 1.—Magnified 320 diameters. Part of a very young *Spongilla*, of an oblate spheroidal form, and about $\frac{1}{25}$th of an inch in diameter. On the right is presented a face view of the investing membrane and the underlying monadigerous mass. On the left the focus is so adapted as to be fixed on a face view of the monad mass, and at the same time on a sectional profile of the investing membrane at a^1, b^3, *c*, and *d*.

Fig. 2.—Magnified 780 diameters. Interior of a monad chamber, seen through the aperture; the monads appear in end view, and crowded together side by side like a pavement work.

Fig. 3.—Magnified 1600 diameters. A single monad, as seen in profile in the monad chamber. Only two contractile vesicles were present in this specimen. The cylindrical collar (*k*) is extended to its utmost.

Fig. 3*a*.—Magnified 1600 diameters. Foreshortened, front view of a monad; the body (*j*) in the distance; the hollow cylinder (*k*) projecting toward the observer like a dark hoop, and the *flagellum* (*l*) in the centre appearing as a black spot.

Fig. 4.—Magnified 780 diameters. Sectional view of a monad chamber, bringing the aperture (*i*) into profile, as well as the monads which lie at the same level; thus showing their convergence about the central open space.

* See article on "Polarity and Polycephalism," 'Silliman's Amer. Journal,' January, 1870

† See Carter "On Fecundation in the two Volvoces;" "On Eudorina, Spongilla, &c.," 'Annals and Magazine Nat. Hist.,' January, 1859; also for July, 1871, "On New Sponges, &c."

polycephalic unit. Yet, whichever view prevails, the tendency is the same, and the Polyp theory is negatived most unquestionably. The incompatibility of the interior organisms of the two groups, above mentioned, is so great that it would seem as idle to elaborate a proof of it, as to attempt the demonstration of an axiom. The question is really circumscribed, according to the method of Haeckel, to arguing that, since a system of branching canals in the Sponge reminds one very strongly of the intricate network of passage-ways in the basal parts of certain Polyps, therefore the two are homologous, and bear an identical relation to the rest of the organism Carter* has answered this far-fetched homology with considerable detail in a recent paper; and we do not therefore feel called upon to add more to it.

The principal aim of this article is to furnish new material in proof of the polycephalism of the Spongiæ, and particularly in regard to their relation with the *Protozoa Flagellata*. We are highly pleased to find that Carter has lately† confirmed our earliest observations‡ as to the organization of the collar-bearing monads of *Leucoselenia*, by an investigation of *Grantia compressa*. He has also accepted our interpretation of the horn-like processes of the sponge-cell of *Spongilla alba*; that they are the outlines of a membranous collar in profile.

We have now to bring forward a fourth example of a *craspedote*, flagellate monad-cephalid in a Sponge. It seems to be a *Spongilla*, but specifically, at least in its monads, it differs from the English forms. For convenience sake, we will call it *Spongilla arachnoidea*, from its resemblance to an irregular spider-web. It lives in fresh-water streams and ponds, usually about the bottom of the stems of water-plants, or wherever there is considerable shade; apparently avoiding the light, as we seldom, if ever, found it in open water. In size it varies from a few inches to half a line in diameter; of no definite shape; and has a uniform fuscous or yellowish-brown colour; and is wrapped about by a filmy, transparent, colourless envelope ("*investing membrane*" Carter). The brown colour is inherent to the interior mass, in which the groups of monads are imbedded; in fact, the latter are themselves as strongly coloured by brown granular contents. The "investing membrane" is also slightly tinged with amber colour by the large and small spicules which are imbedded in it. Excepting in very small specimens, foreign matter is often so thickly spread over the surface as to obscure the view and seriously interfere with a correct interpretation of the relation of parts. We have been most fortunate

* "On New Sponges, &c.," 'Ann. Mag. Nat. Hist.,' July, 1871.

† *Ut sup.*, July, 1871.

‡ 'Memoirs Boston Soc. Nat. Hist.,' vol. i., 1867, "On the Spongiæ Ciliatæ as Infusoria Flagellata."

in our endeavours with the minuter individuals, which occasionally, we found, would allow a view through and through their entire bulk, and of course left full opportunity for a satisfactory study of the details of special parts, without our resorting to the dissecting needles. Anyone who knows, by experience, the intense contractility of the living sponge, can appreciate the advantage of not being obliged to destroy and sever parts of an organism from their natural relations. Premising thus, that everything has been studied in "place," even to the details of the monads, we shall endeavour to describe this sponge as if it were to be the type for future comparison.

General Plan.—The whole individual sponge is endowed with a double envelope (Fig. 1, *a*, a^1, *c*, *d*), the outer and inner parts of which are directly continuous into each other at many points. The outer division (*a*, a^1) lies at a considerable distance from the monadigerous mass (*g*), and is, as it were, suspended on the points of the larger, far-projecting spicules (*e*); just as a tent canvas is supported on the ends of poles. The inner division (*c*) closely embraces the monadigerous mass like an epidermis, and even plunges between the hollow groups of monads, forming to them a basis of support. The outer and inner divisions are continuous with each other at many points, as stated just now, but only where the larger spicules project. There the envelope (*d*) runs along the spicules, completely embracing them, as if in a sheath, from their tips to their bases, where they rest on the brown mass of monads. In brief, we might say that the sponge is covered with a miniature colonnade, whose ceiling is the outer division of the envelope, the pillars are the bundles of spicules, and the floor is tapestried by the inner division, which about the pillars hangs from the ceiling in lofty folds. The continuity of the outer division of the envelope is broken by numerous round or oval openings, of various and frequently changing sizes, sometimes very large, which allow a free ingress of the water to the space just beneath. These are the *afferent ostioles* (*os*), through and into which a constant current of floating particles may be seen moving with considerable vivacity. Here and there, scattered at wide distances, finger-like, hollow processes from the outer division arise singly and at various angles. Each is terminated by a large aperture, the *efferent ostiole*, from which a current of water and floating matter emerges with more or less spasmodic irregularity. The smaller individuals, from half a line to half an inch in diameter, possess only one such ostiole; and those an inch in diameter seldom have more than two or three like conduits; but they are very large, sometimes a quarter of an inch in length when fully extended, and of the proportions and taper of the human forefinger.

Plunging the focus of the objective to the floor of the colonnade,

the inner division (*c*) there is found to be pierced by much more numerous openings (*i*), but far smaller in diameter, and quite methodically arranged, each one corresponding to and overlying a hollow group of monads (*h*). The outer division is further embellished with irregularly-scattered minute spicules (c^1), which lie imbedded in the cytoblastema, parallel with the surface of the envelope, and occasionally crossing each other at various angles. To complete this general sketch, we will state more definitely the relation of the constituents of the monadigerous mass. There are essentially but two elements here; namely, the inner division (*c*) of the investing membrane, and the groups of monads (*h*) which are imbedded into it, below its surface. In a fully-expanded individual these groups seldom lie so closely as to touch each other. They vary considerably in size and are usually globular or spheroidal, and form a single *stratum*, with rather narrow interspaces (c^1) between them.

It seems proper here, at least for the sake of precision, that the *cytoblastematous basis*, in which the monad groups are imbedded, should be considered apart from the epithelium-like, inner (*c*) investing membrane which overlies it, although the two are essentially one; the epithelioid membrane, by prolonging itself between (at c^1) and beneath the groups, forming for them a continuous foundation. In this light, then, we shall speak of the monadigerous mass as consisting of three elements, namely, the inner investing membrane proper, the group of monads, and the *cytoblastematous basis*. This *basis* seems to constitute a large part of the bulk of the body, since it occupies all of the interior space beneath the monad groups. In specimens which grow over flat surfaces in depressed patches, or around stems of plants, it forms a relatively thin layer; but where the body stands out an irregularly-rounded mass, sometimes an inch in diameter, the cytoblastematous basis fills up the interior, in enormous proportion to the bulk of the monad layer.

Organography.

The Investing Membrane.—The *investing membrane* (Fig. 1, *a*, a^1, *c*, *d*) consists essentially of two histological elements, namely, a very diffuse *cytoblastema* (a^1) and irregularly-disposed cells (*b*, b^1, b^2) scattered through it. The intercellular *cytoblastema* forms a very thin layer (a^1) between the cells (*b*); but where the latter are imbedded in it, its outer and inner faces are as wide apart as the considerable depth of the cells demands; and thus it happens that the membrane (both the outer and the inner divisions) presents in profile (a^1, *c*, *d*) such an irregular thickness. The *cytoblastema* (a^1) is colourless, hyaline, and apparently homogeneous under a low power; but when magnified to about four hundred

diameters it displays a very finely granular aspect. It occupies wide intervals between the cells, certainly more than one-half, and fully three-fifths of the whole area of the membrane. Its apparent extent, in a general view, is even more than that, owing to the extreme transparency of the cells, and their consequent inconspicuousness. That the cytoblastema, notwithstanding its low undeveloped state, is the true contractile element in this membrane, there can scarcely be a doubt, when we consider both its widespread preponderance and its relative continuity, as contrasted with the scattered, disconnected condition of the cells (b^2) which are imbedded in it. Sometimes it is barely possible to discover even the trace of a cell on the border of an afferent ostiole (*os*), and in that case we must infer, inevitably, that it is cytoblastema which opens and closes the aperture. We find it, too, embracing the extreme tips of the larger spiculæ, where the cells utterly fail to appear.

The *cell element* (*b*) of this membrane is also in a lowly condition; only partially developed. There is no *cell wall*. What may appear to be a wall is really the thin stratum of cytoblastema (a^1) overlying the distal and proximal faces of the cell. This is our conclusion after the most critical scrutiny, with a carefully-corrected objective. Were it not, indeed, for the usually constant presence of a distinct nucleus (*n*) in each cell, we would be strongly inclined to look upon it as merely a dense collection of coarser granules than are generally diffused through the cytoblastemic layer. The irregular and jagged outline, and the caudate projections of the cells (b^2), also tend to tempt one to the latter view. The cell element in this case, then, corresponds only to what is usually considered the cell contents, and a nucleus. The contents are composed of coarse and fine grey granules, which at times are quite conspicuous, but most frequently are so transparent and slightly refractive as to appear, collectively, unless specially focussed upon, as a faint blotch in the investing membrane. This renders it all the more difficult to trace the outline of the cell, and particularly where it throws out irregular, caudate prolongations, to blend with those of other cells. We have been able to detect but one layer of cells in this membrane* when it is well stretched out. The depth of the cells, as may be seen in a sectional profile view (*b*), is about equal to their breadth, and their length is from one-half more than to twice their breadth; but frequently they are as broad as long. They stand in no particular relation to the ostioles; and, as stated above, sometimes scarcely touch their border. The *nucleus* (*n*) may be readily detected by its peculiar, strong refraction, and its considerable superiority in size over the granules. Its bright refractiveness in this connection reminded us of a contractile vesicle,

* Carter figures two or three cells overlying each other in *Spongilla alba*, 'Ann. Mag. Nat. Hist.,' July, 1857, Pl. 1, fig. 7.

but, although suspecting it of such a function, we could detect no change other than might be produced by the varying length and breadth of the cell, and the shifting of the relative position of the coarse granules. In the *inner division* (*c*) of the investing membrane the cells are usually smaller than those in the outer division, but differ in no respect otherwise, neither in form nor arrangement. They lie flat on their sides in the cytoblastemous layer; but, except in profile, they are most difficult to discover on account of the underlying brown mass of monad groups and granular interstitial substance.

Although we have been unable to discover any distinct cell elements in the cytoblastematous mass immediately around and beneath the monad groups, neither have we found it possible to distinguish it from the cytoblastema lying on the surface: and since the continuity between the two is unbroken, we must, perforce, consider them as one. The underlying portion of the cytoblastematous mass, however, is characterized by irregularly-scattered, moderately coarse, brown granules (*c*^{1}). These serve very well as a dark frame or setting to the monad chambers (*h*), and by contrast bring them out more strongly.

The Monad-cepholids.—We now proceed to describe the most essential feature of this animal, the *monads*. They are the characterizing, the dominating element, in reference to which the whole organism is contrived and constructed. They are not cells; they are the *heads* of a polycephalic individual, and consequently correspond functionally to the tentaculated heads of Polypi, and not to their interior epithelial cells. We must first describe what we call the monad chamber.

The *monad chambers* (Fig. 1, *h*; Fig. 2; Fig. 4) are deep spherical hollows which form the receptacles of the groups of monads (*j*). They are mere cavities, and have no lining wall.* They may be easily recognized, in young specimens, as clear, more or less circular, areas scattered in pretty close proximity to each other over the "cytoblastemic mass." Each chamber has a single, small, circular aperture (*i*) which perforates the inner (*c*) investing membrane, and allows egress into the circulatory apartment (*f*). The aperture (*i*) varies in size at times, and may even be completely closed. We have never seen it open wider than one-third the diameter of the chamber, and very rarely more than one-fifth as wide. That it is a true perforation, and not a clear spot, may be demonstrated by bringing a chamber into profile, so that its aperture (Fig. 4, *i*) lies

* The hollow groups of monads were originally described by Carter ('Ann. Mag. Nat. Hist.,' July, 1857) as lining an hypothecated vesicle, which he named the "ampullaceous sac." He has since ('Ann. Mag.,' Jan., 1859) revoked that view and adopted another. We believe him to be, excepting the inferred "ampullaceous sac," in the main, right in his first interpretation; but as our species are different we cannot speak definitely.

on the extreme border, and then an actual break in the continuity of the investing membrane becomes evident.

Entering this aperture we do not meet with any obstacle for a little distance around it; there is a clear open space (Fig. 4); but pressing onward beyond that, either to the right or the left or directly forward, the cavity appears filled by a collection of vibrating bodies. They seem to be arranged radiatingly from and about the centre. Close inspection, however, modifies this view, and it *turns out* that they are based upon the periphery of the chamber, and converge towards its centre, where is a small unoccupied space. We presently recognize these converging bodies to be *craspedote, flagellate* monads (*j*), so closely packed together, side by side, as to form a continuous *stratum* (Figs. 2 and 4) over the whole concave face of the chamber, excepting immediately about the aperture. Every feature of the monad is strongly marked; even the cylindrical collar is so heavy and conspicuous that its outlines may be seen with as low a power as two hundred diameters. We have studied these bodies with a $\frac{1}{8}$th-inch objective, and found it not at all difficult to focus down upon the details of their organization, without pressing upon or even touching the specimen.

These monads are in every general essential identical with those which we originally found in *Leucoselenia*, and like those also, recently described by Carter,* in *Grantia compressa*. They are attached to the concave face of the chamber by their posterior end (Fig. 4, *j*); and the anterior extremity, with its flagellum (Fig. 3, *l*) and collar (*k*), projects freely into the open space, and toward the centre of the apartment. When fully expanded, the length of the body and collar together is about one-third, or a little more, of the diameter of the chamber; so that nearly one-third of the latter is unoccupied at the centre, except by the tips of the flagella converging from every direction. As the monads lie touching each other on every side (Fig. 2), they mutually flatten their bodies, sometimes so much as to give them a strong polygonal outline; or, when the whole mass is expanded, they scarcely impress each other, and therefore retain a rounded contour. By plunging the focus so as to look into the aperture of a chamber, down upon the monads at the bottom (Fig. 2) of it, an end view of each cephalid is obtained. From this point the foreshortened cylindrical collar looks like a strong, dark circle (Fig. 3 *a*, *k*), which retains its conspicuousness as we plunge down farther, even to the base, where it is attached to the body (*j*). The outline of the latter is considerably without the "dark circle," the two being concentric to each other. At the same time we see in the centre of the dark circle a black spot (*l*) which may also be focussed up and down upon, and hence it is inferred to be a continuous line foreshortened. Other views (Fig. 3, *l*) con-

* 'Ann. Mag. Nat. Hist.,' July, 1871.

firm this, and show that it is a single *flagellum*. The monads are so transparent, and the organization so distinct, that the collar and flagellum may be seen clearly from an opposite point of view, looking directly through the body of the cephalid. This, too, is the best position from which to study the *contractile vesicles*.

A sectional profile view of a group (Fig. 4), to be obtained by plunging the focus half-way through a chamber, serves best to disclose the manner in which the posterior ends (*j*) of the monads are affixed to the concave face of their receptacle; and we also here obtain a strictly profile aspect of a monad. Figure 3 is such a view, representing a single cephalid, under a much higher power than in Fig. 2 or 4. An excellent and least obstructed side view, but not strictly a profile, is to be had by focussing upon the monads immediately about the aperture of the chamber. Here we look directly into the door-way, or through the bordering, transparent epithelioid membrane which it penetrates.

The body proper (Fig. 3, *j*) of a cephalid is a little shorter than it is broad; on the whole, spheroidal in shape. Its posterior end is broadly rounded, and so is its anterior extremity. In front rises a cylindrical, membranous "collar" (*k*), which tapers slightly and projects forward to a distance equal to considerably more than twice the length of the body. Its diameter is not more than two-thirds, or even less than that of the body. Although colourless and homogeneous, it is remarkably conspicuous on account of the thickness of the membrane of which it is composed. Near its open extremity it is more transparent and less obvious than toward its basal attachment.

The *flagellum* (*l*) arises from the centre of the anterior end of the body, in the midst of the area which is surrounded by the membranous cylinder (*k*), and without tapering extends a little farther than the open end of the latter. It vibrates usually throughout its length, but is most active near its tip. We have never seen it assume a rigid, arcuate position, as in some other species of monads. It is particularly remarkable for its want of transparency, and looks like a black thread more than any vibrating cilium that we have ever met with. Its action, at times, is rather that of a strong wriggle than a vibration.

The *contractile vesicles* (*v*).—The body of the monad is distinctly marked by a coarse, scattered, brown granulation, with two or three rather large, clear spots, at a considerable distance from each other, but always close to the periphery. These clear areas are the *contractile vesicles* (*v*). They do not occupy any particular place in the body, although they usually are not in front. The systole and diastole are extremely slow, but very distinct, if sufficient patience is summoned to watch them fixedly, and without interruption. The last third of the systole is abrupt, and then only

does the vesicle appear to contract suddenly; whereas by watching it through a complete circuit of diastole and systole, one learns that its function is, on the whole, performed very slowly. This very abrupt movement, quite happily, may serve to rebut any such objection as that the otherwise tardy action is merely the result of protoplasmic contraction of the body, as in certain Palmellate Zoospores. Their immovable position, as regards the body contents, is another item of rebutting evidence.

The *Spiculæ* (Fig. 1, *e*, *e*^{1}) are very slender, slightly curved, needle-shaped bodies, gradually tapering to a sharp point at each end. They have a bright amber colour, and a rather dark, strongly refractive outline. From tip to tip they are slightly roughened by irregularly-scattered, low, but acute prominences or knobs. There are two kinds of spicules, large and small, but they differ in no other respect. The larger (*e*) are from four to six times longer and thicker than the smaller ones. They occur in bundles of two, three, or four; and act as props to hold up the outer investing membrane, as described in the early part of this article. They seldom arise perpendicularly from the monadigerous mass, but more or less obliquely; and, in forming bundles, stand across each other like stacked arms. We seldom found spicules penetrating the monadigerous mass far beyond the epithelioid, inner investing membrane. They evidently belong, universally, to the investing membrane, and assist it in forming a framework in which the inner mass is suspended. The *smaller spicules* (*e*^{1}) are strictly confined to the outer division (*a*) of the investing membrane, and lie there on their sides, completely immersed in its thickness. They are scattered irregularly and sparsely about, and frequently cross each other at varying angles. We observe no nearer approach to a methodical arrangement among either the large or the small spicules; yet their very irregularity, being after a kind, and constant in that kind, may be recognized in some sense as methodical.

General Considerations.—Seeing the secluded position of the monad-cephalids, deeply ensconced in little chambers below the general surface of the circulatory apartment, it is not directly evident that their *flagella* have any agency in keeping up the inflow and outflow of currents through the afferent and efferent ostioles. Nowhere else are vibrating or non-vibrating cilia or cilia-like bodies to be met with than in the monad chambers. And since the efferent ostioles are irregularly interspersed among the much more numerous afferent ostioles, we cannot conceive how the flagella in any way could influence currents to move in a particular direction, from the smaller apertures toward the larger ones. They no doubt keep up a direct flow of matter into the sunken chambers, but the current comes from the inner depths of the circulatory apartment, and far away from the ostioles. In this

way only a turbulence of floating matter is sustained, but the general, great current is due to a far different cause. We conceive that the contraction and expansion of the body-mass in general, modified by the alternate opening and closing of the afferent and efferent ostioles, is the true motive power in this phenomenon. We have observed, often, that the outer division of the investing membrane is not kept at a uniform distance from the central, monadigerous mass; at one place it will be found to be close to its inner division, so that the circulatory apartment is very shallow there, while at another point the two divisions of the membrane are widely separated, and the circulatory apartment is very deep; and between the shallow and the deep apartments a curtain is drawn, more or less completely, extending from one pillar-like bundle of spicules to another. Each of these temporarily enclosed portions of the general apartment, it is plain now (although our actual observation on this point is very defective), may contract or expand without disturbing the contents of any other. Such an apartment with its afferent ostioles closed, may be contracting and forcing a current out at its efferent ostiole, while a neighbouring apartment may have its efferent ostiole closed, and expanding, draw in current through its open afferent ostioles.

We regret that we have not the means, in this locality, for completing these researches. Our specimens were gathered, and studied on the spot where they lived, in the western part of Massachusetts, several hundred miles away from our present residence. Unfortunately we put off the attempt to feed the sponge with coloured matter until we had completed other methods of investigation, and then we were prevented by circumstances from carrying out our designs.

In regard to the afferent and efferent canals, seen by Carter* in the monadigerous mass ("*parenchyma*" Carter), we have not met with any trace of them in the species described in this article. It is possible that they may exist in the oldest and largest individuals, but as we worked only on very small and transparent specimens, our direct observations, in this respect, strictly apply to the latter. It is more likely that ours is a different genus from the *Spongilla* of Carter, in favour of which we cite the curious fact that each aperture, in the inner division (not mentioned by Carter) of the investing membrane, exactly overlies and is inseparable from the entrance to a monad chamber ("*ampullaceous sac*"; *partim*, Carter); so that whatever enters these chambers must go out by the same way that it came in; not out into a system of branching canals, burrowed in the monadigerous mass, but into the great circulatory apartment.—*Silliman's American Journal*, Dec., 1870.

* 'Ann. Mag. Nat. Hist.,' 1857, *ut sup.*

V.—*The Refractive Powers of Peculiar Objectives.*

By Robert B. Tolles (U.S.).

In my brief note of December last (1871), communicated to your Journal, I described the action of an immersion objective of 100°, constituted of a dry objective of that angle at "uncovered" and of additionally two hemispherical lenses in front, and having the objects in liquid or in cement between. I now propose to borrow in part Mr. Wenham's diagram,* for further illustration. This diagram, Mr. Wenham says, "shows the marginal rays of mean refraction projected in the exact position that they occupy through the $\frac{1}{8}$th described." The extreme aperture is 130°.

Fig. 1 annexed, *except the dotted lines*, is a copy of Mr. Wenham's as to the front lens and projected rays, with the addition of a double-hemispherical front system, combining the functions of both the immersion front and the balsam-mounted object-slide. It will be noticed that the rays of the $\frac{1}{8}$th objective, as traced by Mr. Wenham, meet at the centre of this interposed *sphere*, because their paths are coincident with its radii. For the same reason this now immersion $\frac{1}{8}$th must be of the same angle as the dry $\frac{1}{8}$th described by him, *viz.* (about) 130°. Or to state the case in the words of Mr. Wenham, expository of a surely coincident case, the object being in balsam, "then of course it" (the object) "can be illuminated from all angles, and seen by the full aperture of an object-glass."†

So far there is evident agreement. But the illustration can teach more than this. With balsam between the hemispheres, as before, the object will be traversed and illuminated by a pencil of nearly maximum angle.

The whole pencil may or may not be passed through the objective used *according to its construction*. But if air be between the hemispheres and the plane surfaces dusted with minute objects, does anyone imagine large angular pencil to reach the object in this case? Either from above or below? Certainly not anyone. The objective being thus used, *dry*, Mr. Wenham's defiance‡ might be very safely tendered.

When air is between the hemispheres, the immersion $\frac{1}{8}$th is limited in its angle to the capacity of the dry $\frac{1}{8}$th in., *when that is used for balsam-mounted objects*. That is to say, the first interior reflecting surface (air outside) that the rays meet in any case limits the angle to 82° (−), according to the law of total reflexion at interior surfaces. But with any fluid of higher refraction than air between the hemispheres, of course the interior angle of the pencil

* Found at p. 19, No. xxv., 'Mon Mic. Jour.'

† See 'Mon. Mic. Jour.,' No. xxxvi., p. 292. ‡ Ibid., No. xxvii., p. 118.

transmitted will be increased, and for this case Mr. Wenham's challenge cannot stand.

For the sake of the simplest, clearest exposition I have in describing as related made my immersion objective for illustration of the systems, borrowing Mr. Wenham's dry $\frac{1}{8}$th diagram in part as before stated. It only now remains to point out the variation necessary to construct the immersion objective of three systems and of the proximate, interior angles.

For this purpose I will adhere to the same $\frac{1}{8}$th in. diagram, and indicate the modification in front lens needed to accomplish the result.

In the Fig. 2, the outside dotted line indicates the convex surface of the dry front as drawn by Mr. Wenham; the continuous line of a shorter radius within that is to indicate the curvature necessary to produce a $\frac{1}{8}$th-inch immersion of angle about 10° interior pencil. What is intended is to increase the refraction at the convex surface of the front by sharper convexity, or higher refractive material or *both*, to the extent necessary to make up for the diminution at the plane surface which takes place according to the refractive power of the medium flowed in. "The mere fusion by a refractive medium of the thinner or immersion lens and cover into one," as Mr. Wenham says; but here with the difference that the front lens remains of the same thickness as the dry, while the convex surface has (according to requirement of the case) increased refraction.

Below the new front of this now immersion $\frac{1}{8}$th is represented the thin cover and thin slide (increased about four times with, understood to be, balsam-mounted objects between. The ray *a*, Fig. 2, may be continued—tracing the ray from above in this case —to the object under the cover, because, although in the case of water there is some positive refraction at the plane surface of the front, it is exactly, or very nearly, balanced by negative refraction of the first surface of the covering glass, *viz.* tracing the ray from above, downward towards the object, as Mr. Wenham does in his treatment of the subject in his diagram. Moreover, if the flowed in medium were of the same refractive index as the glass, the line would be straight. Well, in continuing this ray of $52\frac{1}{2}$° incidence to the lower surface of the slide, the law of total reflexion (interior) comes into effect. Of course only 41° incidence, and 82° angular pencil, will have passage through the lower surface of the slide. This total reflexion boundary forms an absolute limit of angle for objects in balsam. I might refer my critic to my first communication* for the proper method to adopt to gain a larger pencil, but otherwise and easier let reference be made to Fig. 3 annexed, intended to represent an ordinary object-slide with covering glass, and

* 'Mon. Mic. Jour.,' No. xxxi., p. 36.

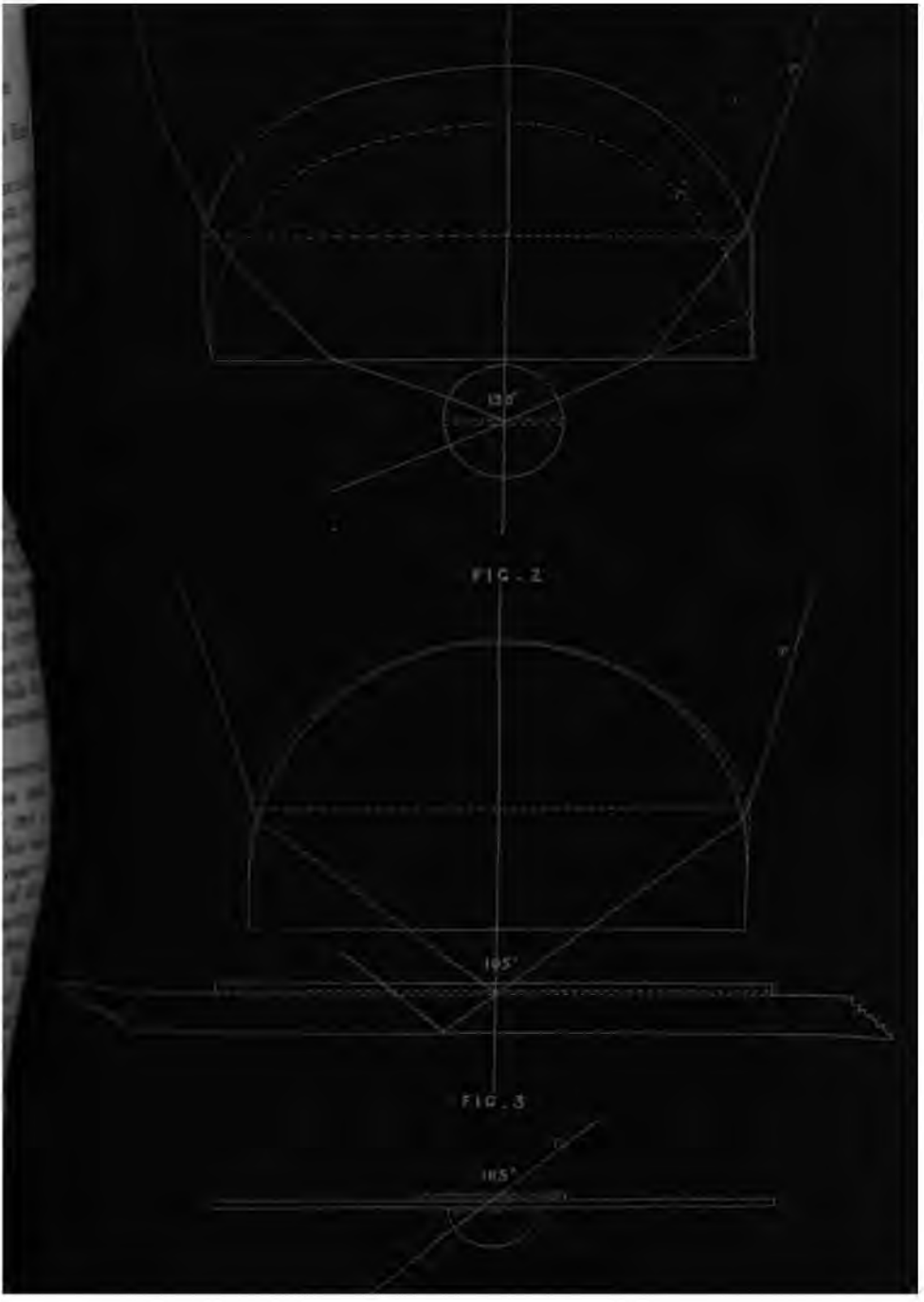


FIG. 2

FIG. 3

object in balsam. The ray *r*, identical with that of Fig. 2, having passed through the liquid above the cover, and the covering glass itself, will, or a ray correspondent to it, proceed to the convex surface of the cemented plano-convex lens below the slide, and emerge there as traced in the figure, or substantially so. An arrangement for procurement of 130° is indicated in Fig. 1, where the dotted lines show a sharper curvature as also thinner lens. When this lens is moved towards the system next back of it until the surface meets the ray *r*, coincidently with the dotted line *r'*, then that ray can have refraction correspondently with the line drawn by Mr. Wenham for 130° in his dry $\frac{1}{8}$th diagram. This can be strictly so when the medium between the plane surface of the front and covering glass is of the same refractive index as the glass. In this case, too, no limitation by total reflexion. But if water is used, there would be limitation, and within 130°. To gain fully that aperture, a medium of somewhat higher refractive index than water would be needed. But only the refractive ability of the connecting medium is the real practical limit of angle of transmitted pencil in the immersion objective below maximum angular aperture.

The limitation takes place at the upper surface of the covering glass, not *necessarily* anywhere else, with balsam-mounted objects. If air is the medium in the interspace above cover, only 82° approximately can be obtained, with whatever appliances.

As stated in my first communication, an immersion objective, if used below the slide as condenser, or any form of immersion condenser of *sufficient angle*, has the same action (of course) as the plano-convex lens, to increase the angle of the interior pencil, according to the immersing medium used.

Mr. Wenham, in his last note,* asks some questions, as follow:—"Does he" (the writer of this) "really expect us to believe that he can obtain all his aperture by the back combinations alone?" and in response I have described a means of procuring 130° with *three* "back combinations" in use instead of two.

Again, "or that any of his objectives, when duly adjusted for an immersed object, and thus showing a large aperture when measured in air, will retain the same angle when the front is immersed in water?"! *Certainly not* (*vide* my own experiment described in the first dozen lines I ever wrote on the subject, 'Mon. Mic. Jour.,' No. xxxi., p. 37, where an objective of 170°, measured in air, is credited with only 100° when immersed in water). By the way, Mr. Wenham in his own experiment† reaches the same result, *viz.* "a $\frac{1}{2}$th of 170°" in air, *in water* showed an "apparent aperture" of 100°. The objective being built for water-immersion use (doubtless) that was the proper medium for the

* 'Mon. Mic. Jour.,' No. xxxvi., p. 292. † Ibid., No. xxxii., p. 86.

pencil measured to traverse. No, not the same in water as in air necessarily; 100° of angle is appreciably more than 82°, and that difference is the question at issue.

Lastly, Mr. Wenham says, "His position is an impossible one. In a corrected high-power object-glass, when immersed, the focus does not fall in the centre of the hemisphere, but is considerably beyond it."

Very well; that does not affect my position. He will not *deny* that it *may* fall in the centre of the hemisphere and give view of the object.

In the case that I offered in my first communication, also as just related that one given by him in the next following number of this Journal, an objective of 170° in air, gave only 100° in water. But that is enough for my "position," and 100° allows of projection "considerably beyond the centre of the hemisphere" for its focal point. The position seems a very possible one, and Mr. Wenham seems to furnish proof of its validity.

Boston, *January* 19*th*, 1872.

VI.—*A Few Additional Remarks on "The Examination of Nobert's Nineteenth Band."*

By F. A. P. Barnard, Columbia College, New York.

The pages of a scientific magazine are too valuable to justify the intrusion upon them of matters of merely personal interest. But when a person is placed before the readers of such a magazine in a light in which he does not appear to advantage, he has no alternative but to ask for a brief hearing.

Early in the year 1868 I made a long-continued and laborious examination of Nobert's Nineteen Band Test-plate. I saw lines in the nineteenth band which I supposed to be the real ones. I made counts of these lines with a filar micrometer; and from these counts deduced by calculation the number of divisions like those counted which would correspond to an English inch.

It was only with the extremest difficulty that I could obtain what seemed to be the resolution. To secure the necessary conditions of illumination and focussing cost me often much time; and often I failed, after very protracted trial, to secure them at all. Moreover, when they were obtained, the image was so feebly illuminated as to require an extremely acute vision (which I think I possess) to see it at all; and the labour of observation was so severely trying to the eyes, that I should not be willing to go through such a course again.

Because of these circumstances, the results were not satisfactory to me. I therefore did not publish them, nor even speak of them beyond a limited circle of my acquaintances. I even went so far as to ignore them altogether when speaking on the subject in public; not regarding them as sufficiently important to justify me in qualifying the expression of opinion quoted by Col. Woodward in the January number of this Journal, that the lines of the nineteenth band had "never been fairly resolved."

My results and my method of reaching them were communicated to Mr. Stodder in response to a request made directly to me by him. He published them, and they became subject of criticism by Col. Woodward. Had Col. Woodward confined his criticism to the results, I should have had nothing to say; but inasmuch as he attacked the principle on which I proceeded, I felt bound to vindicate the principle as one which is unquestionably sound. I could not vindicate the principle without referring to the observations for illustration. If in doing this I have seemed to "reconsider" my opinion formerly expressed as to the value of those observations, it is simply because he has made it a logical necessity for me to do so. I certainly saw *something* and counted something. No subsequent expression of opinion of mine can alter that fact. And in order that the readers of the Journal might be in possession of the whole case, I should wish to place before them all the original minutes of the observations, if it were in my power. They have been mislaid, but I think not lost; and I hope to publish them hereafter. In searching for them I have encountered a series of observations made in the following year, when the dry-working front of the objective had been replaced by an immersion front by Tolles. The following examples, copied literally from the notes, will illustrate the mode of proceeding. In the first four bands the entire breadth of the band is measured: in the nineteenth, the measurement extends to only twenty divisions. The second number of each equation is the equivalent number of the micrometer.

1st Band.	$\frac{6}{1000}$ line = 1161 div.	6 lines = 1,161,000 div.
		1 line = 193,500 div.
2nd Band.	$\frac{9}{1500}$ line = 1159 div.	9 lines = 1,738,500 div.
		1 line = 193,167 div.
3rd Band.	$\frac{12}{2000}$ line = 1158 div.	12 lines = 2,316,000 div.
		1 line = 193,000 div.
4th Band.	$\frac{14}{2500}$ line = 1076 div.	14 lines = 2,690,000 div.
		1 line = 192,143 div.
19th Band.	$\frac{20}{10000}$ line = 391 div.	2 lines = 391,000 div.
		1 line = 195,500 div.

Objective, $\frac{1}{15}$th immersion.

If the rulings and the counts are both correct, one division of the first band should be equal to ten of the nineteenth; one of the second to $6\frac{2}{3}$ of the nineteenth; one of the third to five of the nineteenth; and one of the fourth to four of the nineteenth.

I. $\frac{1161}{6} = 193{\cdot}50.$ And $\frac{391}{20} \times 10 = 195{\cdot}50.$

II. $\frac{1159}{9} = 128{\cdot}78.$ And $\frac{391}{20} \times 6\frac{2}{3} = 130{\cdot}33.$

III. $\frac{1158}{12} = 96{\cdot}50.$ And $\frac{391}{20} \times 5 = 97{\cdot}75.$

IV. $\frac{1076}{14} = 76{\cdot}86.$ And $\frac{391}{20} \times 4 = 78{\cdot}20.$

The coincidences are not exact; but they are too close to be accounted for on the supposition that the lines in the nineteenth band are spurious.

The following are specimens of measurements made at the same time to determine the value of a French line in divisions of the filar micrometer, by measuring the divisions of a stage micrometer, in which the British inch is divided into 1000 parts; a covering glass having first been placed over the stage micrometer, as nearly as possible equal in thickness to the Nobert plate. Both lines of the filar micrometer were employed in this measurement, having been first adjusted so as to throw the total index error upon one. Each thread was then moved over one one-thousandth of an inch, with the following results:—

Index error, left screw = 0, right screw = + 42.

I. Left Screw.	Right Screw.
$\frac{1}{1000}'' = 2162$ div.	$\frac{1}{1000}'' = 2211 - 42 = 2169$ div.

$\frac{1}{1000}'' = 2162 + 2169 = 4321$ div.

1 inch = 2,160,500 div. 1 line (British) = 180,041·7 div.

$180{,}041{\cdot}7 \times \frac{81}{76} = 190{,}676$ div. = 1 line French

Another portion of the stage micrometer being brought into the field, a second measurement was taken, thus:—

II. Left Screw.	Right Screw.
$\frac{1}{1000}'' = 2167$ div.	$\frac{1}{1000}'' = 2248 - 42 = 2206$ div.

$\frac{2}{1000}'' = 2167 + 2206 = 4373$ div.

1 inch = 2,186,500 div. 1 line (British) = 182,208·3 div.

$182{,}208{\cdot}3 \times \frac{81}{76} = 193{,}011$ div. = 1 line French.

The stage micrometer is probably not greatly to be relied upon, and the covering glass may slightly differ in thickness from that of the Nobert plate. It is also coarsely ruled, but considering these circumstances, these last determinations accord fairly with those deduced from the Nobert plate previously.

In conclusion, I wish to say that I claim no merit for the resolution of the fine lines of Nobert's nineteenth band, and never have claimed any; what I claim, and all that I claimed in my former communication is, that the method employed by me for testing the character of the lines observed is, in a scientific point of view, entirely trustworthy.

PROGRESS OF MICROSCOPICAL SCIENCE.

Bilharzia hæmatobia (*Cobbold*) *in a Manchester Patient.*—Dr. Henry Simpson gave the Manchester Philosophical Society an account last year of a case of his in which this rare worm had appeared. Through inflammatory changes leading to loss of substance, the ova become free in the bladder, and are washed away in the urinary secretion in immense numbers, along with blood disks and pus-corpuscles. They are generally ovate in form, but vary somewhat in outline. At one end the shell is produced into a short spike, something like that on Von Moltke's helmet. Occasionally this is placed laterally. They vary in length from $\frac{1}{170}$th to $\frac{1}{300}$th of an inch, and in breadth from $\frac{1}{400}$th to $\frac{1}{500}$th of an inch. The shell is without any distinct operculum. The contents of the egg are seen in all stages of development, from scarcely distinguishable granules, enclosed in a vitelline membrane, to the perfectly-formed ciliated embryo, exhibiting active movements of the body and rapid play of the cilia while still enclosed in the shell. Sometimes the head of the embryo lies towards the spiked end, sometimes to the plain end of the shell. Free, living embryos are often met with in the urine, and it is curious to watch the mode in which they escape from the shell. The general shape of the embryo is elliptical; they are abundantly supplied with cilia, especially at the anterior extremity, and show distinct traces of a water vascular system. The development of this entozoon in all probability follows the same general plan as that of the other Trematode worms, or Flukes, which pass through several phases or alternations of generation; one or two intermediate hosts, generally mollusca or aquatic larvæ, being necessary before the adult fluke becomes parasitic in the body of the vertebrate animal destined to be its host.

Mode in which Cliona burrows.—Mr. Edward Parfitt publishes a paper on this subject, and the boring of mollusks. He differs from Dr. Hancock, who says, in regard to *Cliona*, "that no other excavation, whether of worm or mollusk, presents a surface anything like that of the burrows of sponges; and were no other proof at hand, this puncturing would be sufficient to establish the fact that they possess the power of enlarging their habitations." Now, says the author, if Mr. Hancock's assertion be right, these silicious tools ought to leave their marks on the walls of the holes accordingly. We should have either lines corresponding with the direction of the burrows or transversely, or both, according to the contracting of the investing membrane; but so far as he has investigated this subject, he has not been able to detect any scratches of this kind. In fresh-bored cavities into solid shell, the walls are lined with the thin, pale, yellowish process of the sponge, composed of the investing membrane and the inner sarcode, on which was scattered a few sponge cells. In these membranes are stuck, almost at random, a few spicules, the principal use of which is to hang on the investing membrane, and it is on these that the sarcode

proceeds or creeps along; but so far as he can discover, they do not assist the sponge in excavating in any way whatever, only that they are the means by which the sarcode advances. Now it seems to him that wherever the sarcode, or rather the investing membrane, touches the shell, the latter shows an eroded surface; the minute points and pits can easily be seen with a quarter-inch objective. If one of those fresh-bored holes be examined horizontally in good light, and even with an inch objective, it will be seen that these holes are not smooth, although they appear so to the unassisted eye, and even with a good pocket lens; but when carefully examined, a number of points and angular processes are seen standing out into the hole in various directions, showing at once that the hole had not been cut with anything moved with a rotatory motion. He carefully examined these holes all the way down to their apex, and he finds the same appearance all the way; similar projections of shell are seen all the way down. The apices of these holes are not round, but are more or less angular. Many of them have a triangular impression, and all of them show the minutely-roughened surface, the minute points, and pits of an eroded surface. Now he has carefully examined these sponges for some erosive agents, the same as did Mr. Hancock; but he has, with that gentleman, not been able to detect any; yet in his own mind he feels certain that it is by the action of some agent of this kind that these holes are eroded both by the annelids and sponges. Our not finding an acid in these animals does not prove that they do not use, or separate, or secrete, an acid or an erosive agent; and he sees no reason whatever for doubting, neither does he see any difficulty in believing, in their decomposing the sea water, or separating carbonic acid from it, and using it as soon as separated. By this means it is easily conceived how these animals work, and it is also seen why we could not discover the agent, for they kept none in stock, but manufactured it as they wanted it. He illustrates his views by a very well-drawn plate, showing the magnified borings.

Unfortunately, however, this subject has recently had devoted to it a paper,* by Mr. J. G. Waller, which is as full of exact information and careful deduction as is possible. Mr. Waller, unfortunately for Mr. Parfitt, proves beyond contradiction that this is really not a burrowing sponge at all; and his paper, though too long for insertion here, will be read by those interested in the subject with advantage. His arguments against the mechanical powers of the sponge seem to us to be quite sufficient. In the section of an oyster-shell, of which he gives a figure, we can see the burrows filled with a sponge, and at its distant extremity, where is the latest growth of the sponge, we find the membrane pellucid, with but few spicula. Going back into the older portions, this membrane gets gradually more full of colour, denser in character, the spicula increasing in number, until they become almost matted together. We then come to that part on the edge of the shell where the laminæ are wide and open, whose spaces, beside the burrows and connecting with them, the sponge has occupied, and here has communication with the outer world. Hitherto the dermal membrane

* Read before the Quekett Club, and published in its Journal, January, 1872.

has been protected by the shell it covers; now, as this is wanting, we find the means it takes for that purpose singularly effective. The spicula in all other parts of the sponge have been irregularly disposed on the membranes, which is a mark of the genus to which Dr. Bowerbank has given the characteristic name of *Hymeniacidon*. But here they form themselves into a regular wall, closely packed together, parallel to each other, so as to present a formidable array of pikes to any adventurous intruder who would attack the domicile. The sarcode here thickly overlays and invests it. Here are found the pores—the inhalent organs—not, however, very conspicuous, but sometimes distinctly seen with an inch lens. So that, instead of finding the sponge in possession of an apparatus of attack and destruction, which has been assigned to it, we see that its offensive powers, if we may so call them, have been concentrated into a system of defence, to screen it from assaults from without; and that no part of the sponge has so powerful an organization as this prepared for its protection.

Microscopic Structure of the Pitchstones of Arran.—This is the title of an instructive paper, with a plate, in the 'Geological Magazine' (January). We do not ourselves think that the specimens represent anything but mineral formations, though of course we cannot very well say, not having seen the specimens. The author (Mr. S. Allport) says that a thin section, examined under the microscope with a one-inch objective, is seen to consist of an amorphous glassy base, containing numerous long, slender prisms of a green pyroxenic mineral; the latter are occasionally isolated, but generally form the axes to which are attached innumerable minute pale green crystals, arranged in exquisitely beautiful groups; some wonderfully like fronds of ferns, others bearing the closest resemblance both in form and colour to some of the microscopic fresh-water algæ; in fact, the field of view appears to be crowded with minute ferns, and the most elegant sprays and tufts of *Batrachospermum*. The glassy base has a pale yellowish tint in the open spaces between the groups; but under a higher power the colouring matter is resolved into a mass of translucent granules and minute crystals, the latter being much smaller than the belonites which form the fern-like groups. A comparison of many specimens, affording gradations in size, shows clearly that all these crystalline particles consist of the same pyroxenic mineral as the larger prisms which form the axes. It is worthy of remark that the tufts and aggregations of belonites are invariably surrounded by a border of clear colourless glass. Placed between crossed prisms, the section is, if possible, still more beautiful; the tufts and sprays then appear to be formed of bright gold powder on a perfectly black ground, with fine brilliant dust scattered in the dark spaces between the groups.

The Gregarinidæ developed from Amœbæ.—The 'Lancet,' in a recent number, has taken up the subject of M. E. Van Beneden's paper, of which a short notice was given by us some time ago. It says that the author has followed, in the case of the *Gregarina gigantea*, the entire series of metamorphoses undergone by the masses of protoplasm proceeding from the psorospermia into a fully-developed

gregarina measuring two-thirds of an inch in diameter. He found in the intestine of the lobster small masses of protoplasm destitute of nuclei, finely granulated, and performing continuously ordinary amœboid movements. These bodies he compares to the *Protamœba primitiva* or *Protamœba agilis* of Haeckel, and classifies them with the true Gymnocytodes of that author. Bodies of precisely similar appearance are also found, however, which are incapable both of locomotion and of change of external form. In these a homogeneous marginal zone and a granulated internal mass come to be distinguished. The gymnocytode invests itself with a membrane, and is thus converted into a leptocytode (of Haeckel). Each cytode now thrusts out two arms, which are prolongations of its substance, and each of which is destined to become a gregarina. The first one that makes its appearance is the largest, and is characterized by its marked granulation and great mobility. It detaches itself from the cytode and becomes free. Before this occurs, however, the smaller arm is thrust forth, which is not granular, and is motionless. After the first or mobile arm has separated from the cytode, the second arm becomes as mobile as the first was, and absorbs the whole body of the cytode into itself, just as the embryo of a mammal absorbs the contents of the yelk-sac. The two protoplasmic arms thus produced from a single cytode—Pseudofilaria, as Van Beneden terms them—move with extraordinary vivacity in the intestine of their host. After the lapse of a certain time the movements become less lively; the pseudofilaria become shorter and thicker; nucleoli, and subsequently a nucleus, make their appearance in their interior. At a still later period a homogeneous cortical layer (membrane) is differentiated from the granulated contents, at one pole of which strongly refractile granules collect, and the whole organism now appears as a well-defined small gregarina, which soon augments in size.

Laboratory for Marine Zoology.—Dr. Anton Dohrn, in a letter to Professor Agassiz, who has communicated it for publication in the 'American Naturalist' (January, 1872), writes that he has matured a plan which has for many years been in the minds of many zoologists; that of establishing a large laboratory for marine zoology in the Mediterranean. He has obtained permission of the authorities of the city of Naples to construct a large building at his own expense, in the Villa Reale at Naples close to the sea, containing a large aquarium for the public, and extensive rooms for naturalists of every country. Dr. Dohrn, with two or three other German zoologists, will settle there and conduct the administration of both the aquarium and the laboratories. He wishes that information regarding this proposed laboratory be widely extended, and earnestly invites all who may go to Naples to visit the aquarium. An annual report of the work done and progress made at the zoological station will be published. A committee has already been formed to give further dignity and importance to the project, consisting of Messrs. Hemholtz, Dubois-Reymond, Huxley, Darwin, Haeckel, Leuckart, Van Beneden, &c., and in America Professor Agassiz.

Is Guano Bird-dung or Infusorial Deposits.—This question has recently attracted a good deal of attention from several naturalists, especially from Professor A. M. Edwards, of the Lyceum of Natural History of New York. Professor Edwards says, that in California there is a deposit of "Infusoria" improperly so-called, accompanied by bitumen, which bitumen the gentlemen of the State Survey believe has been derived from those "Infusoria," and that contiguous thereto we have guano deposits. At Payta, in Peru, Dr. C. F. Winslow discovered an "Infusorial" deposit, almost identical in character with the Californian one: near by are bitumen springs, and lying off the coast are the guano islands of Lobos, Chincha, Guanape, and others. At Natansi, Japan, we have extensive "Infusorial" strata and bitumen; it is not recorded whether guano occurs in that quarter. In the island of Barbadoes we have "Infusorial" strata, bitumen, and near by the guano islands of the Caribbean Sea; and he is informed guano is abundant on the small islands and rocks nearly throughout the West Indian Archipelago. In the island of Trinidad we have "Infusorial" strata and bitumen, and of course adjacent guano. At all of these localities volcanic action is evident, but we have some localities of guano without "Infusorial" strata or bitumen as yet recorded, while we have the celebrated "Infusorial" strata of Virginia, which by a little stretch of the imagination may be supposed to be related in some way to the petroleum of West Virginia and Pennsylvania. In Algeria we have "Infusorial" strata and bitumen, but he never heard of guano having been found near by. However, now that attention is called to this fact, it is to be hoped that more careful observations will be made connected with the subject, and he hereby calls on all sciencists and travellers to do all they can to assist in the elucidation of this interesting and important matter. From all of these facts, and others that he has collected of no less importance, derived from chemical and microscopical characters. he has come to the conclusion that guano is not the excreta of birds deposited upon the islands and mainland after its upheaval, but that it is the result of the accumulation of the bodies of animals and plants, for the most part minute and belonging to the group which Haeckel has included in a new kingdom, separate from the animal as well as the vegetable, under the name of Protista, and subsequently upheaved from the bottom of the ocean. Subsequent chemical changes have transformed it into guano, or heat and pressure have so acted upon it that the organic matter has been transformed into bitumen, while the mineral constituents are preserved in the beautiful *atomies* that make up the mass of the extensive "Infusorial" strata found in various parts of the world.

The Natural History of the Passifloraceæ.—Dr. Maxwell T. Masters, F.R.S., who is now almost our only British physiological botanist, has given the world a most valuable essay* upon the whole group of Passifloraceæ. In this he has devoted a special portion to the subject of the minute anatomy of the group, which is of great interest. Dr. Masters has not followed it out as closely as he might have done, but still his

* 4to, and two plates.

work is very good, and deserves the attention of microscopists. With regard to the sepals of this order he says that their lower or outer surface are covered by an epidermis, the cells of which are somewhat cuboidal, but sinuous in outline above, and thickened on their upper wall. They are interrupted here and there by oval stomata, bounded by two sausage-shaped guard cells containing chlorophyll, a substance not met with in most of the other epidermal cells. The inner or upper epiderm of the sepals is of a similar character, and is likewise perforated by stomata. Some of these stomata are of an imperfect character, in that the separation into two guard cells does not take place. The form of the aperture remains unaffected; but it is bounded by a single cell instead of by two. According to Von Mohl, the stomata originate from the subdivision of a single cell, each of the subdivisions becoming a guard cell. In the imperfect stoma above described there has been arrest of development, as it were, and the sphincter of the stoma is formed of one cell instead of by a pair. Charles Morren,* who was the first to notice this condition of the stomata, mentions the existence in *Passiflora quadrangularis* of still more imperfect stomata on the calyx, consisting merely of one semi-lunar guard cell, as if the fellow cell were suppressed. Morren states that he saw, in one case, an aperture, as of an ordinary stoma, between the side of this solitary sphincter cell and the adjoining epidermal cell. He has not himself seen these half stomata, probably because his observations have not been sufficiently extended. Beneath the outer epidermis are three or four rows of spheroidal cells containing chlorophyll. Within this (in *P. alata*) there is a quantity of loose spongy cellular tissue, consisting of irregularly branching cells, between whose subdivisions are left large spaces or *lacunæ*. These cells contain little or no chlorophyll. Traversing this tissue are numerous bundles of slender spiral vessels. The inner epidermis consists of flattened polygonal cells with oval stomata.†

The Microscope in the Diagnosis of Worms.—In the 'Birmingham Medical Review,' the first number of which has just appeared, there is an account of the use of the microscope for the above purpose by M. Bouchut, of Paris. M. Bouchut admits that we owe it originally to M. Davaine. The following brief account is taken from the 'Bulletin de Therapeutique':—"I have prepared for the microscope a small portion of excrement, in order that you may see how the diagnosis may be made, and that you may observe the form of the eggs of the lumbricus. (?) In the preparation you can see the eggs, oval in shape, with scolloped borders like a raspberry, and containing an inner sphere, in which are included some molecular granulations. The presence of these eggs in the fæces gives certainty to the diagnosis of the existence of worms in the intestines, and removes all hesitation about the employment of vermifuges."

The Microscopy of Tuberculosis.—Dr. C. F. Rodenstein has a long paper on this subject in the 'New York Medical Journal' for Decem-

* 'Dodonæa,' Part ii., p. 18.

† See also 'Transactions of Linnean Society,' vol. xxvii.

ber. Among his observations the following seems of interest. He says that in making experiments with blood corpuscles he has lately noticed that if a drop of blood, freshly drawn, be placed in an alkaline solution of carmine, the red corpuscles lose their power of forming rouleaux, and the white corpuscles absorb the carmine, seek each other, congregate in little masses, and seem to become agglutinated to each other. In a drop of blood prepared for microscopic inspection, by careful focussing there can be seen the whole field covered by *fine little rings*, which seem to form a delicate network, looking *somewhat* like the cornea of a fly seen with a low power; this is nothing but the red corpuscles of the blood which touch each other by their edges. Scattered over this delicate, pale network, there can be seen, here and there, little, bright red, cellular masses; these are the white corpuscles of the blood tinged with carmine. In specimens of pathological urine he has also seen sometimes, under the microscope, that pus-corpuscles have a tendency to approach each other, and to form adherent masses. Perhaps the formation of miliary tubercles takes place in a similar manner. The amœboid cells, when leaving the circulation, may bring with them a tendency to form into granulations, or the chemical or physical condition of the surrounding tissues may determine them to assume the form of minute nodules. We find sometimes, with these semi-transparent bodies, others rather of a fibrinous structure; they may represent a subsequent stage in the development of tubercularization. The lymphoid cells may have changed into fibrinous tissue by progressive metamorphosis. Virchow thinks that the fibrinous tissue represents the first stage of tubercular growth, which has not yet unfolded itself into the full-blown cellular tubercle. Langhaus, on the contrary, looks upon this formation as the full development of the cellular tubercle, and describes it as consisting of three zones, formed by the transformation of round cells into fibrillæ. Whether such bodies can be distinguished from minute fibromas, or whether the one or the other be the earlier stage of a continuous development, the tendency of all tubercular deposits to speedy decay has been universally recognized. Virchow says: "This structure, which in its development is most nearly related to pus, inasmuch as it has the smallest nuclei, and relatively the smallest cells, is distinguished from other more highly-organized forms of cancer, cancroid, and sarcoma, by the circumstance that these contain large voluminous corpuscles, with highly-developed nuclei and nucleoli. Tubercles, on the contrary, are always a pitiful production, and from the very outset perishable." And in this respect, also, tubercle betrays its origin and nature. The paper is of considerable length, and sums up the researches that have been made already on the subject.

The Young of Chironectes pictus.—A letter from Professor Agassiz is published in 'Silliman's American Journal of Science and Art' for February (1872), which is of great interest, as it tells us a good deal about the young of this fish. The letter is to Professor Pierce, and after some preliminary details of the nature of the nest, &c., which was found in the Gulf Stream, it says that a more careful examination very soon

revealed the fact that the elastic threads which held the Gulf-weed together were beaded at intervals, sometimes two or three beads being close together, or a bunch of them hanging from the same cluster of threads, or they were, more rarely, scattered at a greater distance one from the other. Nowhere was there much regularity observable in the distribution of the beads, and they were found scattered throughout the whole ball of sea-weeds pretty uniformly. The beads themselves were about the size of an ordinary pin's head. We had, no doubt, a nest before us, of the most curious kind; full of eggs, too; the eggs scattered throughout the mass of the nest and not placed together in a cavity of the whole structure. What animal could have built this singular nest was the next question. It did not take much time to ascertain the class of the animal kingdom to which it belongs. A common pocket lens at once revealed two large eyes upon the side of the head, and a tail bent over the back of the body, as the embryo uniformly appears in ordinary fishes shortly before the period of hatching. The many empty egg-cases observed in the nest gave promise of an early opportunity of seeing some embryos freeing themselves from their envelope. Meanwhile a number of these eggs with live embryos were cut out of the nest and placed in separate glass jars to multiply the chances of preserving them, while the nest, as a whole, was secured in alcohol, as a memorial of our unexpected discovery. "The next day I found two embryos in one of my glass jars; they occasionally moved in jerks, and then rested for a long while motionless upon the bottom of the jar. On the third day I had over a dozen of these young fishes in my rack, the oldest of which began to be more active, and promised to afford further opportunities for study. . . . . But what kind of fish was this? About the time of hatching, the fins of this class of animals differ too much from those of the adult, and the general form exhibits too few peculiarities to afford any clue to this problem. I could suppose only that it would probably prove to be one of the pelagic species of the Atlantic, and of these the most common are Exocœtus, Naucrates, Scopelus, Chironectes, Syngnathus, Monacanthus, Tetraodon, and Diodon. Was there a way to come nearer to a correct solution of my doubts? As I had in former years made a somewhat extensive study of the pigment cells of the skin, in a variety of young fishes, I now resorted to this method to identify my embryos. Happily we had on board several pelagic fishes alive, which could afford means of comparison, but unfortunately the steamer was shaking too much and rolling too heavily for microscopic observation of even moderately high powers. Nothing, however, should be left untried, and the very first comparison I made secured the desired result. The pigment cells of a young *Chironectes pictus* proved identical with those of our little embryos."

The Anatomy of the Pennatulians.—This continues to be admirably given in Professor Kölliker's fine treatise, which is being published at Frankfort. The work, of course, is not yet completed.

A Rare Fungus has been found and presented to the Manchester Philosophical Society (December) by the Rev. J. E. Vize, M.A., of

Forden, near Welshpool. The present was a slide of *Xenodochus carbonarius*, Schl., and Mr. Vize reported that this rare fungus occurs near Welshpool, in a railway cutting, with a south-westerly aspect, well sheltered by a hill and a wood. The first appearance on the leaves of *Sanguisorba officinalis*, L., was noticed in the middle of May, when the Lecythea-form was in perfection, but the stems and other portions of the Burnet were greatly distorted by it. A month afterwards the magnificent vermilion-coloured spores were well sprinkled over the leaves, the form of which was unaltered. In the middle of July the intensely black brand spores made their appearance, many of which had twenty or more articulations, and were plentifully scattered over the leaves in tufts. Mr. Vize stated that he had not watched the transition state from the Uredo to brand-spores, but he hoped to do so if opportunity offered.

Action of Quinine on the White Corpuscles of the Blood.—This subject, in which Binz and Stricker held somewhat different views, has recently, says the 'Lancet,' been taken up by Herr Kerner, who contributes a paper on this subject to the last part of Pflüger's 'Archiv,' being incited by the observations of Mosler on the cure of certain cases of leucæmia by the administration of quinine. Kerner remarks that it is quite possible to obtain a neutral salt of quinine, and in his experiments he used the chloride and the carbonate. He drew small quantities of blood from cats and dogs, and applied a one-tenth solution of this salt in proportion to the blood of 1 part to 4000, upon a microscopic stage maintained at blood heat. The result was striking. The white corpuscles became round and darkly granular, and the movements were very speedily completely arrested. It of course became interesting to compare these effects with those produced by other neutral salts, and in pursuing this investigation to some extent he found that salicin, caffein, atropine, and arseniate of potash were all either wholly indifferent or possessed only the slightest influence. Quinine therefore exerts a remarkable action on the white corpuscles of the blood, independent of its antiseptic properties.

The importance of Angular Aperture being stated in objectives, has been pointed out of late by several of our correspondents; but the remarks of one who signed himself "B" have been taken up by the 'American Naturalist' (January, 1872). This journal says that the estimation of any angular aperture, so well expressed by "B," is perfectly familiar and undisputed among experienced microscopists, although its exact bearings are not always easily apprehended by beginners; and that microscopists need occasional caution in regard to it may be inferred from the case in point, where an accomplished writer stated an extraordinary performance of a lens without mentioning the range of its apertures or the aperture at which he worked it. The peculiar and entirely independent qualities of lenses of low and of high angles are everywhere understood alike; but the extent to which success has been attained in this country in the construction of high angles cannot be appreciated abroad when "B," evidently well informed on other points, would not be surprised to hear that a one-

fifth of excessive resolving power had an angular aperture of 150° or 160°. Anyone in this country would be "surprised" to hear that its highest angle was less than 170°. The 'Naturalist' thinks that makers should always engrave the angular aperture upon the mounting and on the boxes of their objectives. The neatness and sufficiency of this plan, however, is marred in the case of many modern objectives whose screw-collar adjustment gives a wide range of powers and angles. Exactly at what point of adjustment the measurements should be made in these cases is one of the most difficult points to be settled in endeavouring to obtain a uniform nomenclature in regard to the works of different makers. At least for the present, until some standard degree of adjustment can be agreed upon, both the highest and lowest figures should be given where the range is considerable.

Lymph Follicles in Vagina.—The 'British Medical Journal' says that Lowenstein* shows that the mucous membrane of the vagina is not destitute of lymph follicles, as is generally asserted in anatomical text-books.

British Ostracoda.—We wish to direct attention to the fact that Mr. G. S. Brady has had issued, by the Linnean Society, his splendid 'Monograph of the Recent British Ostracoda.' It is a work which takes its rank with the fine volume of Baird, and the more thorough memoirs of Lilljeborg, Fischer, Zenker, Claus, Sars, and others. The classification is that proposed by G. O. Sars, son of the distinguished Norwegian zoologist, Prof. Michael Sars, in his 'Oversigt af Norges marine Ostracoder,' published in 1865. The Ostracoda are represented by the little two-shelled water-fleas, about half a line or less in length, which swim over the bottom or creep over submerged plants. As remarked by the author, "the geographical and bathymetrical distribution of the Ostracoda is a matter of the greatest interest as illustrating the probable condition under which the various fossiliferous strata have been deposited."

The Blind Fishes of the Mammoth Caves of America.—One of the best papers we have seen for a very long time in the 'American Naturalist' is that on the above subject, by Mr. F. W. Putnam, in the January number. It is too long for an abstract here, but we specially commend it to our readers' notice.

Stauropteris Oldhamia.—At a meeting of the Philosophical Society of Manchester (January 9th), Mr. E. W. Binney, F.R.S., exhibited some specimens of a fossil plant resembling the *Psaronius Zeidleri* found in the Upper Foot Coal Seam, near Oldham. This species has been described by Corda, in his 'Beitrage Zur Flora Der Vorvelt,' and figured in plate xi., but has not hitherto, he believed, been met with in the British coal-fields. The Oldham specimen appeared to him to be a petiole, of about one-eighth of an inch in diameter, and is of a nearly circular form in its transverse section, two-thirds of it consisting of a zone of strong parenchymatous tissue and an internal axis of vascular tissue arranged in four radiating arms of an irregular oval

* 'Centralblatt,' 35, Sept. 2nd, 1871.

form, resembling a St. Peter's cross. As he could not connect the specimen with a stem of *Psaronius*, he proposed to call it *Stauropteris Oldhamia*. In the above-named coal, as well as that of the Lower Brooksbottom Seam, there is a great variety of beautiful petioles which have not yet been described. Some of them evidently belong to the genus *Zygopteris*, and may probably be discovered in connection with their stems, but most of them have been found detached, and sometimes mistaken for the rootlets of *Stigmaria*. From some specimens in his cabinet he is led to believe that Cotta's *Medullosa elegans* is merely the rachis of a fern or a plant allied to one.

NOTES AND MEMORANDA.

Death of Mr. Patterson, F.R.S.—We regret to announce the death of Robert Patterson, F.R.S., which took place on the 14th of February, at Belfast. He was well known as a distinguished zoologist and as the author of Patterson's 'Zoology for Schools.' He did much service in national education. His microscopic work was not of equal importance with his general zoological researches.

The 'Amateur Microscopist' is the title of an American work just published. It is by Professor John Brocklesby, M.A., and is illustrated by 247 figures on wood and stone. We have not yet seen it, so of course we retain our opinion of it.

A Text-book of Pathological Histology.—This valuable book, which is by Professor Edmund Rindfleish, of Bonn, has been translated into English from the second edition by Dr. W. C. Kloman and Dr. F. T. Miles, both American physicians.

Mr. Darwin in the Academy of Sciences.—We are glad to learn from the 'British Medical Journal' that Mr. Darwin has been placed first on the list for the forthcoming election of a Corresponding Member in Zoology of the Academy of Sciences of Paris, and will therefore no doubt receive the honour. His supporters were MM. Milne-Edwards, Quatrefages, and Lacaze-Duthiers.

Excursion of the 'Hasler.'—We learn from an American contemporary that this vessel sailed on its voyage of discovery on December the 4th, and will touch first at St. Thomas Island. The steamer burns less than three tons of coal a day, and can thus run 8000 miles on 150 tons of coal, a remarkable saving of fuel. Professor Agassiz has taken out abundant stores for preserving specimens, and deep-sea nets and hooks specially adapted for catching fish at great depths. This journal (the 'American Naturalist') also publishes a letter to Professor Pierce, the Superintendent of the Coast Survey, in regard to the aims of the dredging party.

A Paper on the Œsophagus of the Hornbill (*Toccus melanoleucus*) has been presented to the Zoological Society at one of its January meetings, by Professor Gulliver, F.R.S. It was an appendix to a former paper by him on the taxonomic character of the muscular sheath of the œsophagus of the Sauropsida, read at a previous meeting of the Society.

A Substance like Ambergris, but which was clearly not that substance, was exhibited at a late meeting of the Lyceum of Natural History of New York. Professor A. M. Edwards, however, said that this substance had been presented several years since at a meeting of the Lyceum by a gentleman of the name of Southworth, who owned a large tract of land near Bahia, in Brazil, where it occurred in large quantities. At that time it had been referred to him for examination and report. He had determined that it was deposited in now extinct lakes beneath whose beds were bitumen springs, the lighter oils from which substance had infiltrated into and impregnated the mud. Sometimes the roots of plants, the remains of leaves, and even wood were found imbedded in it, but no diatoms or other microscopic organisms, by means of which the character of the water beneath which it had been deposited could be determined. The owner proposed, and in fact had to some extent used it for the production of gas for illuminating purposes, as the town of Bahia had been in this way lighted. If it had not been so light and bulky, it had been proposed to ship it abroad for distillation.

Prizes for Amateur Microscopists.—We believe the prizes mentioned beneath are now open to Fellows of this Society. They are as follows:—(1) The Countess of Ducie's Prizes for the best lists of the ponds and other aquatic resorts for collecting purposes, within 20 miles of Charing Cross. 1st Prize, Three guineas. 2nd Prize, Two guineas. Rules:—1. The exact locality of the pond must be given, in order that it may be identified, and the name of the railway station nearest to it.—2. Each competitor to send in his lists sealed in a cover bearing a motto, and accompanied by an envelope sealed, in which is enclosed the real name, address, and occupation of the competitor. These are to be delivered not later than March 31, 1873, addressed—Secretary, Natural History Prizes, 100, Fleet Street, E.C.

(2) The Countess of Ducie's Prize for the best list of the ponds and other aquatic resorts for collecting purposes, within 20 miles of Charing Cross, with a list of the microscopical animals and plants found in them during each month of the year, commencing March 1, 1872. Five guineas. Rules:—1. The exact locality of the pond must be given, and the name of the nearest railway station.—2. The date of the visit must be specified.—3. When any rare or supposed new objects are found, specimens should be immediately forwarded to Walter W. Reeves, Esq., Royal Microscopical Society, King's College, Strand (Somerset House), for examination.—4. Each competitor to send in his lists and other information sealed in a cover bearing a motto, and accompanied by an envelope sealed, in which is enclosed the real name, address, and occupation of the competitor. These are

to be delivered not later than March 31, 1873, addressed—Secretary, Natural History Prizes, 100, Fleet Street, E.C.

N.B.—Importance will be attached by the Adjudicators to any *Notes and Records of Pond Life* made during each month of the year with reference to the *Local Distribution*, *Development*, or *Hybernation of the Species*.

The information might be sent in monthly, if preferred: such information would be considered confidential. At the close of the year the series of monthly reports thus sent in would be treated as the competition for the prize.

Adjudicators—Henry J. Slack, Esq., F.G.S., Secretary of the Royal Microscopical Society, author of 'Marvels of Pond Life,' &c., &c.; Walter W. Reeves, Esq., F.R.M.S., Assistant-Secretary to the Royal Microscopical Society.

CORRESPONDENCE.

Peculiarities of Resolution.

To the Editor of the 'Monthly Microscopical Journal.'

Boston, Mass., U.S.A., *Dec.* 29, 1871.

Sir,—I notice in your Journal for December that Mr. Wenham admits that in the illustration I have given in your number for November, that the rays are transmitted through the balsam-mounted object "in all directions." Mr. Wenham doubts the possibility of producing a set of combinations *above* the spherule to transmit a pencil of even 90°. Of course the accomplishment of this last singularly-supposed impossibility settles the question against his claim.

Well, allow me to say I have had constructed a low-power *dry* objective of clear focal distance of about $\frac{1}{10}$-in., and of 100° angular aperture at the point for *uncovered* objects,—open point, in other words. Before this objective I have placed "a little spherule" composed of two hemispherical lenses, and a thin film of fluent blood between. Easily and quickly done, no "cleverness." When the blood is between in a fluent state, the angular aperture measures the *same* as the dry objective alone. When the blood has dried, the angle contracts to 82° (+). Now it is evident enough that the liquid blood between the plane surfaces of the hemispheres opens the angle of transmission through the spherule to about 120°, all rays beyond being "totally" reflected. When the blood has dried, the ordinary case of total reflexion at *interior* plane surfaces obtains, and the pencil is cut down to 82° (+), *i. e.* incidence of 41° (+), at the *plane surface* of the lower hemisphere of "the little spherule."

Between Mr. Wenham and myself, of course this concludes the case. But for the benefit of students fresh to practical optics, I

propose (probably in your March number, if you will publish the proposed article) to set forth the applicability of the illustration to all immersion objectives. My proposition is that the balsam-mounted object can, in the case of the immersion objective, be viewed with the advantage of an angular pencil actually transmitted through the object of approximately 170°, more or less. But it should be *particularly noted* that this is not avowed for *water* as the medium *instead* of *air* above the slide-cover, but instead of air a medium approaching closely the refractive power of glass. Between immersion and dry objectives this is the remarkable difference, with the *latter* only 82° (+) can be obtained. This Mr. Wenham long since showed to be the case, and I would humbly remark that I was aware of this and of Dr. Robinson's demonstration before in "simple honesty" giving my first illustration in your Journal for July last, and to which Mr. Wenham so stoutly took exception.

Respectfully yours,

ROBERT B. TOLLES.

P.S.—Allow me to state as a fact of interest to microscopists that the resolution of *Amphipleura pellucida is not* (or any longer) a difficult achievement. With a true $\frac{1}{10}$-in. of only 90°, *even less than* 75°, I have repeatedly and plainly lined the valve, *using sunlight and blue cell*, the specimen of *A. pellucida* being one kindly supplied to me as a proper test specimen by Dr. J. J. Woodward, and received by him from London. This performance was not dependent on any special merit in the objective—any good immersion $\frac{1}{10}$th would do the same thing. To accomplish this I used a narrow angled 1 in., 10° swinging under the stage of the microscope as a *condenser*. This 1-in. was placed at the incidence (obliquity to the axis of Mic.) indicated above, *viz.* 45° *or less*. The focus for parallel (sun's) rays a *little* outside of the object.

R. B. T.

ON POSITIVE AND NEGATIVE ABERRATION.
By Dr. ROYSTON-PIGOTT.

To the Editor of the 'Monthly Microscopical Journal.'

MR. EDITOR, 2, LANSDOWNE CRESCENT, *Feb.* 7, 1872.

Sir,—As my paper printed in the Transactions of the Royal Society has been honoured by criticisms by two of your contributors, professing to point out errors of a mathematical nature, undetected by the accomplished mathematicians who scrutinized it, you will, I trust, permit me to observe that in a standard optical work the following passages occur, in which some of our Fellows may perhaps be interested:—

"We suppose in these pages light to proceed from *right* to *left*, and *positive* lines consequently to be measured from *left* to *right*."*

In the case of a converging lens Dr. Parkinson shows, as also

* Dr. Parkinson's, F.R.S., 'Optics,' page 13, 2nd Edition.

does Coddington (both Senior Wranglers), the aberration of a convergent lens is *negative*, the expression for which commences with the negative sign (−). Thus: "The aberration of the ray of S = $v' - v$ (page 119)

$$= -\frac{\mu-1}{\mu^2}\left\{\left(\frac{1}{r}-\frac{1}{u}\right)^2\left(\frac{1}{r}-\frac{\mu+1}{u}\right)-\left(\frac{1}{s}-\frac{1}{v}\right)^2\left(\frac{1}{s}-\frac{\mu+1}{v}\right)\frac{v^2y^2}{2}\right.$$

The distasteful sentence is thus given in the Transactions of the Royal Society, in a note, vol. ii., 1870:—

"It is convenient to define the aberration to be positive or negative, or the lens to be *over-* or *under-* corrected, by the simple fact that a convex lens causes the excentrical rays to cross the axis at a point nearer the centre of the lens than the centrical rays; in which case, and in all analogous cases, it may be said that the lens is under-corrected and afflicted with a negative aberration."

The language of mathematicians unfortunately somewhat differs on this point from the rule of thumb employed in the workshop. In mathematical language, if a line is measured to the right from a fixed point be called *positive*, that measured to the left is *negative*. Accordingly as the marginal rays of a convex lens fall nearer to it than the central rays, the aberration is called *negative*. Any ordinary convexo-plane, plano-convex or double convex, is said in mathematical language to leave a negative aberration, and this lens is under-corrected and *vice versâ*.

Now, Sir, as it is due to myself to give this explanation, after the manner in which this statement has been treated, I may be allowed to draw your attention to the construction of the *positive* and *negative eye-pieces*, and especially to the principles upon which these names have been bestowed by mathematicians upon these combinations.

I am, Sir, your obedient servant,

G. W. Royston-Pigott.

Dr. Pigott and Sir David Brewster.

To the Editor of the 'Monthly Microscopical Journal.'

February 12, 1872.

For the credit of the Microscopical Journal, and in justice to the scientific character of the late Sir David Brewster, Dr. Royston-Pigott, upon having the matter pointed out, may probably offer some explanation as to certain statements he is reported to have made in the current number of your Journal, at page 87.

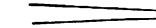
Dr. Pigott, referring to photography, states that "Sir David Brewster has laid down the principle that you cannot get a true picture if you use a very large object-glass upon a small object, on account of certain distortions, which, according to Sir David's principle, are caused by the mixing up together of all the images caused by *different areas* of the lenses, the result being a compound image but not a likeness:" he adds that, "to show the portrait cor-

rectly you must diminish the aperture of the glasses to that of the eye."

As Dr. Pigott has quoted Sir David Brewster, it is to be presumed he must be able to refer to some authority for his quotation, as it is hard to believe that Sir David Brewster would maintain that different areas of the surface of a lens corrected for spherical aberration would give separate or non-concurring images, the essence of a corrected lens or combination of lenses being that the whole area of its surface *concurs* in forming every part of the image at the focus. The fact just stated being demonstrable and well known, I presume it will be accepted.

But Dr. Pigott extends his doctrine to the improvement of the microscope, and if his fundamental principle were sound it would be equally applicable in both cases; but the fallacy of separate areas giving separate and *different* images of the same thing being demonstrable, it is unnecessary to pursue that matter farther. Dr. Pigott, however, in support of his argument, gives a microscopical experiment, which is not without interest. If a minute object be viewed under a glass of large aperture, according to Dr. Pigott "a great many views of the object will be produced, all different; yet all these views are mixed up into one compound image: the eye receives rays from every side of a minute object at once: the observer looks at it from innumerable points of view at one and the same instant:" and under these circumstances it is not to be doubted that very little could be seen. But, says Dr. Pigott, reduce the aperture of the glass, and the object will be seen in the greatest perfection; and here, I am happy to say, I am able so far to agree with him.

The truth is, comparatively few objects are seen to advantage under a large aperture; in most cases one half of the pencils of light cannot be brought into focus along with the other half, and the result is frequently "fog" and indistinctness.

With the large aperture the pencils have points of this shape  F, and only such parts of the image will be seen as may be *critically* in focus, as at F; while the pencils not in focus cause indistinctness. With the contracted aperture the points of the pencils are changed into this shape , which gives a greatly extended range, or depth of focus, almost equally good at D, E, or F; and features are thus distinctly seen at one view which, with the large aperture, would be entirely out of focus, and invisible.

D E F

But this has nothing to do with the "separate images, all different," said to be formed by different areas of the microscopic object-glass.

G. S. Cundell, F.M.S.

PROCEEDINGS OF SOCIETIES.*

Royal Microscopical Society's Annual Meeting.

King's College, *February* 7, 1872.

W. Kitchen Parker, Esq., F.R.S., President, in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations was read, among which, the Secretary stated, were 107 slides of Australian zoophytes, presented to the Society by Mr. Maplestone, of Victoria. These slides had been all mounted by Mr. McIntire, who had expended a large amount of time and trouble in the work. Mr. McIntire requested that the specimens be submitted to some competent authority for examination and determination of species.

The Secretary then proposed a vote of thanks to the donor of the gifts referred to, and moved that a special vote of thanks be passed to Mr. McIntire for the time and trouble and care he had taken in dealing with the slides just mentioned. A vote of thanks was unanimously accorded to Mr. McIntire.

The Secretary announced that Dr. Wallich would be proposed for election as an Honorary Fellow of the Society, in consideration of services rendered to microscopical science, and of the handsome donations of objects which he had made to the Society's cabinet.

Messrs. Gay and Groves having been appointed scrutineers, a ballot was taken for the election of officers for the ensuing year; the scrutineers reported that the whole of the gentlemen proposed on the list had been elected.

The Reports of the Library and Cabinet Committee were read, and also the Treasurer's cash statement.

The President then delivered the Anniversary Address, which will be found in full at page 89.

Dr. Lawson moved, "That the thanks of the meeting be presented to the President, and that he be requested to print his valuable Address." Dr. Lawson said it was utterly impossible for any Fellow present to overrate the importance of the subject; its vastness excited their wonder and their admiration of the patience and skill displayed in its investigation.

Dr. Braithwaite seconded the motion with great pleasure. Notwithstanding the fact that few present could appreciate the real value of the President's Address, it was very evident that he had brought forward the essence of a very great amount of work in a small compass, of which he hoped to hear more in detail at some future occasion.

* Secretaries of Societies will greatly oblige us by writing their report legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. 'M. M. J.'

The vote of thanks to the President, and the motion that he be requested to print his Address, were unanimously carried.

The President said he was very much obliged for the patience which the Fellows had exhibited during the reading of his Address; he was so unused to speaking or reading in public that he believed the very reading of his paper had weakened the effect of it. He hoped it would serve as the means of arriving at what is doing in reference to the subject. He was sure that if the young men would take up the study they would find it a most glorious field of work. The great thing to be done was to find out the facts of nature, and then everything would explain itself.

Mr. Hogg showed a specimen of "*Mycetoma*, or Fungus-foot of India," his report on which was taken as read.

The Secretary announced that at the next meeting a paper by Dr. Klein would be read "On the Earliest Stages of the Development of the Trout," and also a paper by Dr. Murie "On the Classification and Arrangement of Microscopic Objects."

Annual Report of the Society.

The Secretaries report that during the past year the Society has been well supplied with papers, extending over a great variety of subjects, and in many cases bringing forward new discoveries of interest and importance. It is found that scientific investigators appreciate the prompt publication of their researches, which the Society offers through the Monthly Journal.

The Society's collections of books, instruments, and objects may be generally described as in good condition; and, while scarcely any purchases have been made, valuable additions have been received in the donations specified in the subjoined list.

On the 10th of May and on the 24th of January two scientific evenings were held, and devoted to the exhibition of objects and apparatus of remarkable interest, and friendly conversation thereupon. On both occasions a considerable number of Fellows responded to the invitations of the Council and ensured success, both as regards the number, and character of the articles, preparations, &c., displayed, and many objects of interest were kindly lent by non-Fellows. It is intended to continue evenings of this description, of which due notice will be given; and it is hoped that each time an increasing number of Fellows will co-operate by bringing their microscopes, with objects of novelty or remarkable merit.

Books Purchased during the past Year.

Annals of Natural History. 2 vols.
Quarterly Journal of Microscopical Science. Vol. XIX.
The Handbook of British Fungi. 2 Vols.

Presented.	From
Traité du Microscope. Par Chas. Robin	*Author.*
Royal Society's Catalogue of Scientific Papers. Vol. V. ..	*Royal Society.*
Transactions of the Linnean Society. 3 Parts	*Society.*

Several Pamphlets and Papers, as well as the Journals of other Societies, in exchange for our own, have been periodically announced in the 'Monthly Microscopical Journal.'

APPARATUS AND SLIDES.

Microscope and quantity of Apparatus. By M. Chevalier, of Paris .. .. *Sir J. Sebright.*

Logan's Simple Microscope, with three powers .. .. .. *J. N. Logan, Esq., U.S.A.*

Spot Lens for Low Powers .. .. *Chas. Baker, Esq.*

229 Slides have been presented during the year, including a Möller's Diatomaceen-probe-platte. Given by the Rev. R. H. Nisbett Browne.

100 Vegetable Fibres, mounted and presented by W. T. Suffolk, Esq.

36 Palates of Victorian Mollusca and 107 Australian Zoophytes. Presented by C. Maplestone, Esq., of Williamstown, Australia, and kindly mounted for the Society by S. J. McIntire, Esq.

24 Slides of Intestinal Worms, sent over from the Agricultural Society of New South Wales.

12 Slides of Lepidopterous Scales, selected by the Chevalier de Cerbecq.

The financial state of the Society is shown in the Treasurer's account, appended hereto. It does not present any features calling for special remark.

RICHARD MEETAYER, TREASURER, IN ACCOUNT WITH THE ROYAL MICROSCOPICAL SOCIETY.

1871.	£.	s.	d.	1871.	£.	s.	d.
Balance from 1869	34	2	7	Paid Mr. Hardwicke for Journal	251	2	6
Received from Charter Fund	50	0	0	Rent, King's College, to Dec., 1870, and attendance ..	46	5	6
Dividend on 1059*l.* 6*s.* 2*d.* Consols to Jan. 5, 1871 ..	15	12	6	King's College, expenses of Soirée, 1870	34	10	5
Dividend on 1059*l.* 6*s.* 2*d.* Consols to July 5, 1871 ..	15	9	10	Books and binding, 1870–1	43	13	5
Composition	10	10	0	Stationery and printing ..	11	8	0
Subscriptions for 1869 ..	9	9	0	Reporter	9	9	0
" 1870 ..	49	7	0	Ray Society	1	1	0
" 1871 ..	357	0	0	Insurance to Christmas, 1872	2	8	0
" 1872 ..	5	5	0	Instruments	2	8	0
Screw tools sold	0	14	0	Petty cash	20	0	0
				Mr. Reeves' salary	63	0	0
				Collector	11	0	0
				Balance due from Treasurer	51	9	1
	£547	9	11		£547	9	11

Jan. 22, 1872. Examined and found correct,

JAMES HILTON,
HENRY PERIGAL, } *Auditors.*

Sixteen gentlemen have been elected Fellows in the course of the year, and one Honorary Fellow.

During the past year the Society lost three Fellows by death,—John Harker, Edward Wilkinson, and Sir Roderick Murchison. John

Harker, Esq., F.R.M.S., was elected in 1857 and died November 16th, 1871. Of the death of Mr. Wilkinson the Secretaries failed to obtain any accurate record: he was elected in 1863. Sir Roderick Murchison, Bart., F.R.S., &c., became a Fellow of the Society in 1846, but it does not appear that his attention was at any time directed to microscopical research. He was, however, highly distinguished by his various contributions to geological science, and in particular by his great work, 'Siluria,' in which he treated of the whole of the deposits of the Silurian strata from his own close observation in all parts of Europe. This work, the last edition of which appeared in 1867, includes the materials of the Silurian system, with such additions and corrections as were needed to the above-named date, and is a monument of geological research. One of the points which has been settled from Sir Roderick's observations is the position of the rocks of the north-west coast of Scotland, which had been supposed to be old red sandstone, but were at length found to underlie rocks containing Silurian fossils, and therefore to be classed as Cambrian. These rocks were, in fact, found to be the oldest Cambrian, lying below the fossil-bearing Cambrians of the Welsh coast and of the east of Ireland, which were esteemed the lowest fossiliferous rocks until the discovery of organic remains in the Laurentian rocks of Canada, in the valley of the St. Lawrence.

Sir Roderick, having acted for five years as secretary of the Geological Society, became president of that body in 1831-2, and again in 1842-3. He was one of the founders and most active members of the British Association for the Advancement of Science; for several years he acted as general secretary, and presided over its meeting at Southampton in 1846. He has from year to year taken the leading part in the business of the Geographical Section, and has communicated many important papers. In 1844 he was elected president of the Royal Geographical Society, was re-elected in the following year, and again in 1852 and in 1856.

List of Officers and Council of the Royal Microscopical Society, Elected 7th February, 1872.*

President.—William Kitchen Parker, F.R.S.

Vice-Presidents.—*William Benjamin Carpenter, M.D., F.R.S., &c.; John Edward Gray, Ph.D., F.R.S., &c.; *Sir John Lubbock, Bart., M.P., F.R.S., &c.; John Millar, L.R.C.P. Ed., F.L.S.

Treasurer.—*John Ware Stephenson, F.R.A.S.

Secretaries.—Henry J. Slack, F.G.S., and Jabez Hogg.

Council.—Robert Braithwaite, M.D., F.L.S.; John Berney, Esq.; *Charles Brooke, M.A., F.R.S.; *Thomas William Burr, F.R.A.S.; William John Gray, M.D.; Henry Lawson, M.D.; Henry Lee, F.L.S., F.G.S.; *Samuel John McIntire, Esq.; *Henry Perigal, F.R.A.S.; G. W. Royston-Pigott, M.A., M.D., &c.; Charles Stewart, M.R.C.S., F.L.S.; Thomas Charters White, M.R.C.S.

* Those with the asterisk before their names were elected new members.

Scientific Evening.

On the 24th January the Royal Microscopical Society held a scientific evening for the exhibition of objects and apparatus of novelty or remarkable interest, and for friendly conversation thereupon. To avoid crowding no general invitations were issued, and the gathering, of about 200, was almost entirely composed of Fellows, with a few visitors of scientific eminence.

From the subjoined list of objects and articles exhibited it will be seen that the suggestions of the Council were effectually carried out by the zeal and kindness of Fellows and their friends, and it was observed with much satisfaction that many Fellows residing in various parts of the country came from long distances to be present on the occasion.

This opportunity for quiet observation and interchange of ideas upon important topics was thoroughly appreciated by those present, and it is the intention of the Council, as stated in the Annual Address, to hold similar evenings at convenient intervals, of which due notice will be given.

The Society was indebted to Mr. How and Messrs. Howe and Thornthwaite for the loan of excellent lamps.

The Society exhibited a specimen of Mr. Stanistreet's micro-ruling on glass, the lines $\frac{1}{1800}$th of an inch apart, and a beautiful specimen of cupric sulphate with colloid silica prepared by H. J. Slack, Esq.

Mr. C. D. Ahrens: Petal of geranium, shown under a $\frac{1}{4}$-inch objective, with a new kind of erecting binocular microscope.

Messrs. Beck: A unique specimen of cinnabar, with crystals of cinnabarite; and a Podura scale under their new $\frac{1}{10}$-inch objective.

Dr. L. S. Beale: Ultimate nerve networks and plexuses of pale compound fibres with their nuclei distributed to the unstriped muscular fibres of the frog's bladder, mounted March, 1863.

Dr. Bruce: White blood corpuscles, their passage through the capillaries and their amœboid state.

Mr. John Browning: *Acarus Crossii*, kindly lent by W. G. Lettsome, Esq. The spectrum of metallic indium with his Universal Automatic Spectroscope, and seeds of *Gerardia communis* with Mr. Stephenson's binocular microscope.

Dr. Carruthers: Traquairia, a newly-discovered fossil Radiolarian from the Coal-measures, supposed to be the first Radiolarian found below the Chalk.

Mr. Thomas Curteis: Parasite of elephant.

Dr. W. J. Gray: Section of finger with nail *in situ*.

Mr. Edward George: Fructification of Hepaticæ. The spores and elaters of *Scapania undulata*, &c.

Mr. F. W. Gay: The very pretty little Nudibranchiate mollusk from the Victoria Docks, *Embletonia Grayii*.

M. A. de Souza Guimaraens: Cysticercus from a rabbit.

Mr. James How: Two microscopes and his new lamp.

Dr. George Johnson: Hypertrophied muscular walls of arteries (chronic Bright's disease).

Mr. Henry Lee: Sponge spicules *in situ*, *Pheronema Grayii*.

Mr. W. T. Loy: Section of an entire blow-fly, *Musca vomitoria*.

Mr. S. J. McIntire: *Templetonia nitida* alive, the scale of ditto with Powell's $\frac{1}{8}$th and Wenham's truncated lens. The Test Podura mounted to show distinctive characters.

Mr. J. Needham: Human louse changing its skin, and a section of human lung injected.

Mr. L. Norman: A large frame of wood sections, and one of fossils from the Coal-measures.

Mr. Frederick Oxley: An erecting arrangement for dissecting, &c., under the binocular compound microscope, being a modification of the arrangement described by Mr. Ward, in No. 37 of the 'Monthly Microscopical Journal.'

Mr. W. K. Parker (the President): Some very beautiful slides of Foraminifera.

Mr. George Potter: *Alceonella stagnorum*, alive.

Messrs. Powell and Lealand: Podura scale, 3000 diameters, and *Pleurosigma angulatum*, 2000 diameters, under their new $\frac{1}{25}$th objective.

Mr. M. Pillischer: Some very fine injected preparations.

Dr. Royston-Pigott: A kratometer for taking micrometric measurements very readily and ascertaining the power of objectives.

Mr. Walter W. Reeves: Diatoms *in situ*, *Podosira Montagnei*.

Mr. Edward Richards: Two new portable lamps.

Messrs. Ross: Beaded structure of *Pleurosigma angulatum*, shown with Wenham's truncated lens and parabola under their $\frac{1}{10}$th object-glass.

Mr. W. T. Suffolk: Stellate hairs of *Pomidaris apetela*, N. O. Rhamnaceæ.

Mr. Charles Stewart: One of the Entomostraca, *Nebalia bipes*, polarized. Zœa stage of one of the crabs; the heart may be seen in its pericardium immediately beneath the dorsal spine. Globular dentine from the tooth of walrus, and two stages of the development of the hair and hair-follicle, *viz.* first, as an ingrowth of the deep layer of the epidermis; second, the cells in the middle of the ingrowth become modified to form the hair, the outer ones remaining to constitute the hair-follicle.

Mr. John W. Stephenson exhibited his new erecting binocular microscope, and the following beautiful specimens prepared by Dr. Klein:—Nerves and connective-tissue corpuscles of the cornea; nerves of the conjunctiva of rabbit; and a vertical section of skin of negro's head, with the hair-follicles greatly curved.

Mr. Charles Tyler: A series of sponge sections and sponge spicules from Barbadoes, Japan, Jamaica, Australia, India, North America, and the British Isles.

Mr. Thomas C. White: Preparations of the dental pulp and a curious form of hippuric acid polarized.

Donations to the Library and Cabinet from January 3rd to February 7th, 1872:—

	From
Land and Water. Weekly	*The Editor.*
Society of Arts Journal. Weekly	*Society.*
Nature. Weekly	*Editor.*
Athenæum. Weekly	*Ditto.*
Popular Science Review. No. 42	*Ditto.*
Journal of the London Institution, Nos. 10 and 11	*Institution.*
Transactions of the Linnean Society, Vol. XXVII. Part 4, and Vol. XXVIII. Part 1	*Society.*
Report of Surgical Cases in the U.S. Army. Circular No. 3	*Surgeon-General U.S.*
Journal of the Quekett Club	*Club.*
Two Slides of Crystals with Colloid Silica	*H. J. Slack, Esq.*
107 Slides of Australian Zoophytes sent over by C. Maplestone, Esq., and mounted for the Society by S. J. McIntire, Esq.	
Six Injected Preparations	*Moritz Pillischer, Esq.*
Nine Slides of Insect Scales	*T. W. Wonfor, Esq.*
A Polarizing Medium	*Thos. Armstrong, Esq.*

The following gentlemen were elected Fellows of the Society:—

Charles Gibson, Esq.
David Johnson, Esq.
William Boyd Moss, M.D.

WALTER W. REEVES,
Assist.-Secretary.

BRIGHTON AND SUSSEX NATURAL HISTORY SOCIETY.

January 11th, 1872.—This Society, which has been in existence for eighteen years, and during that time done good service in stimulating research and in promoting scientific discovery, gave its first invitation soirée to the members and their friends, some 200 of whom met in the Library and Board-room of the Dispensary.

The walls and tables of both rooms were covered with a large and interesting selection of specimens, drawings, and photographs, illustrative of each branch of Natural History, in the hanging and classification of which efficient service was rendered by Messrs. R. Glaisyer, Penley, G. Scott, Saunders, C. P. Smith, T. W. Wonfor, and Walsh.

The leading objects exhibited in the Library were some very beautiful cases of British birds, collected and mounted by Mr. H. H. J. Nicholls; a white cock pheasant and male and female little gull, in mature plumage, by Mr. Pratt; a fine collection of shells, by Mr. R. Glaisyer; a very curious series of Brighton beach pebbles, containing choanites, by Mr. Glaisyer; fine series of copper ores from the Burra-Burra Mine, by Mr. Wonfor; chalk and other fossils, by Messrs. Saunders and Dennant; specimens illustrating the Post-Pleiocene at Brighton, by Mr. J. Howell; vertebræ of Iguanodon, from the Isle of Wight, by Mr. J. Sewell; rubbings from Egyptian monuments, by Mr. Nourse; female white ant, crocodile, &c., by Mr. E. Moore; while the walls were covered with geological, ethnological, and other diagrams, lent by Mr. Wonfor.

In the Board-room a central table was occupied by microscopes contributed and presided over by Dr. Hallifax and Messrs. Glaisyer, Gwatkin, Marshall Hall, Turner, Sewell, C. P. Smith, Nash, and Wonfor.

The most striking objects exhibited in this room were a fine collection of British butterflies and moths, by Mr. W. Wonfor, jun.; Indian butterflies, moths and beetles, by Mr. Wills; a series of wasps and wasps' nests, by Dr. Ormerod; emerald in quartz rock, from South America, and emerald butterfly, said to be found only in the neighbourhood of the same mine, by Mr. Curtis; Japanese silkworms, moths, and cocoons, by Dr. Badcock; about 150 polished specimens of woods used in manufactures, by Mr. Saunders; series of British mosses and lichens, and enlarged drawings of fructification and dissections, by Mr. C. P. Smith; a very fine collection of ferns and lycopodiums, by Mr. Walsh; New Zealand ferns and sea-weeds, by Mr. Penley; double cocoa nut, by Mr. Hollis; petroleum, in all its forms, from shale and crude black oil up to white wax and manufactured articles, by Mr. Nash; case of saxicava and pholas, taken from burrows in hard limestone—the perforated rocks also shown — by Mr. E. Charlesworth; *Pheronema Grayii*, newly-discovered silicious sponge, dredged off the Portuguese coast, by Mr. Marshall Hall; *Euplectella speciosa*, another silicious sponge, by Mr. Sewell; flying fox, from Australia, by Dr. Badcock; large tusks of walrus and hippopotamus, by Mr. Parkinson; specimens of saw fish, cow fish, and parrot fish, by Mr. Sewell; a fine series of flint implements, collected recently by Dr. Stevens, Messrs. Wonfor, H. Saunders and Hilton, at St. Mary Bourne, Cissbury, Portslade, and the immediate neighbourhood of Brighton; polished stone weapons, found in England, Canada and the West Indies, bronze celts, axes, &c., chiefly found in Sussex, Early British, Saxon and Norman gold and silver coins, by Mr. G. Scott; bronze paalstabs, by Mr. R. Glaisyer; Roman vase, found in a garden on Round Hill, by Mr. Wonfor; series of photographs, illustrative of food products, eggs of parasites, diatoms, Nobert's lines, &c., by Mr. T. Curties (Holborn); very beautiful micro-photographs, by Drs. Hallifax and Maddox and Mr. Hennah; about 150 coloured plates from Hedwig's original drawings of mosses, fungi and lichens, with enlargements of parts and organs, by Mr. Wonfor; a very ingenious hygroscope, made from the awn of an erodium, which was delicately sensitive to variations of moisture in the atmosphere, by Dr. Hallifax; &c., &c.

Tea and coffee were provided during the evening, which was one of enjoyment to all present.

Previous to the soirée an ordinary meeting was held, when Messrs. Shireff, J. Wood, Infield and Howell were elected ordinary members, and Miss Hill a subscriber to the library.

Mr. Wonfor announced the receipt of 'The Climate of Uckfield,' by Mr. C. L. Prince, from the author; the 'Eighth Annual Report of the Belfast Naturalists' Field Club,' and 'Proceedings of the Eastbourne Natural History Society,' from the Secretaries. Votes of thanks were given to the donors.

Reading Microscopical Society.*

October 10th, 1871.—Captain Lang presided, and, after the usual business, read a short paper entitled "Another Hint on Selecting and Mounting Diatoms," in which he more particularly described a method, devised and adopted by Mr. Tatem, for obtaining separate diatoms from balsam-mounted slides. Specimens of diatoms taken from old slides and remounted were shown as illustrations of results.

He also exhibited a new form of wooden slide, with sunk cell, for opaque objects. This was obtained from Mr. Crouch, of London Wall.

Mr. Tatem exhibited arranged mounts of diatoms obtained from mixed and dirty balsam-mounted slides, and also specimens of *Difflugia spiralis*.

Mr. Austin exhibited as specimens of microscopic fungi, *Mucor ramosum*, *Phragmidium obtusum*, *Puccinia syndenesiarum*, &c.

Mr. G. Davies exhibited Möller's type slide.

November 7th, 1871.—Mr. Tatem contributed a short paper "On the Conjugation of *Amœba*," which appears in this Journal.

Captain Lang exhibited twelve slides of diatoms, each containing a separate species, shown in its two principal aspects of front and side views. The genera and species were *Biddulphia aurita*, *B. Tromeyi*, *B. pulchella*, *Gomphonema geminatum*, *Triceratium favus*, *Melosira arenaria*, *Navicula clepsydra*, *Pinnularia lata*, *P. alpina*, *Campylodiscus costatus*, *Surirella splendida*.

Mr. Tatem exhibited diatoms from Rio Janeiro and Puerta Segura.

December 5th, 1871.—Captain Lang exhibited, mounted side by side, on the same slide, the two valves of an entire frustule of *Surirella fastuosa*, picked from Singapore shell cleanings, and separated from each other under the microscope. He drew attention to the great dissimilarity of the two valves, the median space of the one being broadly lanceolate, whilst that of its fellow is linearly lanceolate; the costæ of one valve being, of course, proportionately shorter than those of the other, for he does not consider the irregular markings within the median space to be in either case the continued costæ, though Dr. Greville has described them as such. They appear to him entirely disconnected.

It has long been a well-known fact that the upper and lower valves of certain diatoms, as *Achnanthes* and *Cocconeis*, are not identical in structure, but he is not aware that this has been found to be the case in any of the very numerous species of *Surirella*.

Mr. Kitton, in his controversy with the Rev. E. O. Meara in the 'Quarterly Journal of Microscopical Science,' vol. viii., N.S., justly remarks upon the great difference in the size, outline, and striation of this particular species, but does not allude to the diversity in the valves of the *same* frustule.

Captain Lang also exhibited wings and elytra of rare butterflies and beetles from Brazil, sent to him by Captain Perry.

Mr. Tatem exhibited mounted diatoms, and Mr. Austin small fungi and mounted desmids.

* Report furnished by Mr. B. J. Austin.

South London Natural History Club.

The first annual soirée of the South London and Natural History Club was held on November 30th, at the "Horns" Assembly Rooms, Kennington Park, and proved a gratifying success.

Notwithstanding the adverse circumstance of inclement weather, nearly five hundred ladies and gentlemen were present during the evening, and appeared to appreciate the efforts to delight them, put forth by those who contributed towards the display. In addition to the members of the South London, representatives of the following associations exhibited:—The Quekett Club, the Croydon Club, the Sydenham and Forest Hill Club, together with numerous other friends and the under-mentioned opticians,—Messrs. R. and J. Beck, Bailey, Baker, Collins, Horne, Thornthwaite, How, Moginie, Richards, Stanley, Steward, and Swift.

Dr. J. D. Hooker, F.R.S., contributed four Californian photographs, and Mr. W. W. King displayed a large collection of photographs from the various plant-houses and gardens at Kew. Mr. J. D. Russell and Messrs. Dawes of Camberwell exhibited cases of preserved specimens; and a triangular aquarium, the invention of a member of the South London Club, attracted much attention. The personal welfare of the exhibitors was cared for by a specially-appointed committee, and at intervals musical performances were given by various professional and amateur friends.

The living objects shown were comparatively few in number; but *Conochilus*, *Volvox*, *Carchesium*, *Embletonia*, *Opalina* (a parasite from the frog), and various rotifers and water-fleas were to be seen.

The club may be congratulated on the unequivocal success of the first conversazione, nothing having transpired to mar the arrangements.*

An Ordinary Meeting of this Club was held at Glo'ster Hall, Glo'ster Place, Brixton Road, on Tuesday evening, January 16th, 1872. Mr. Deane, F.L.S., presided.

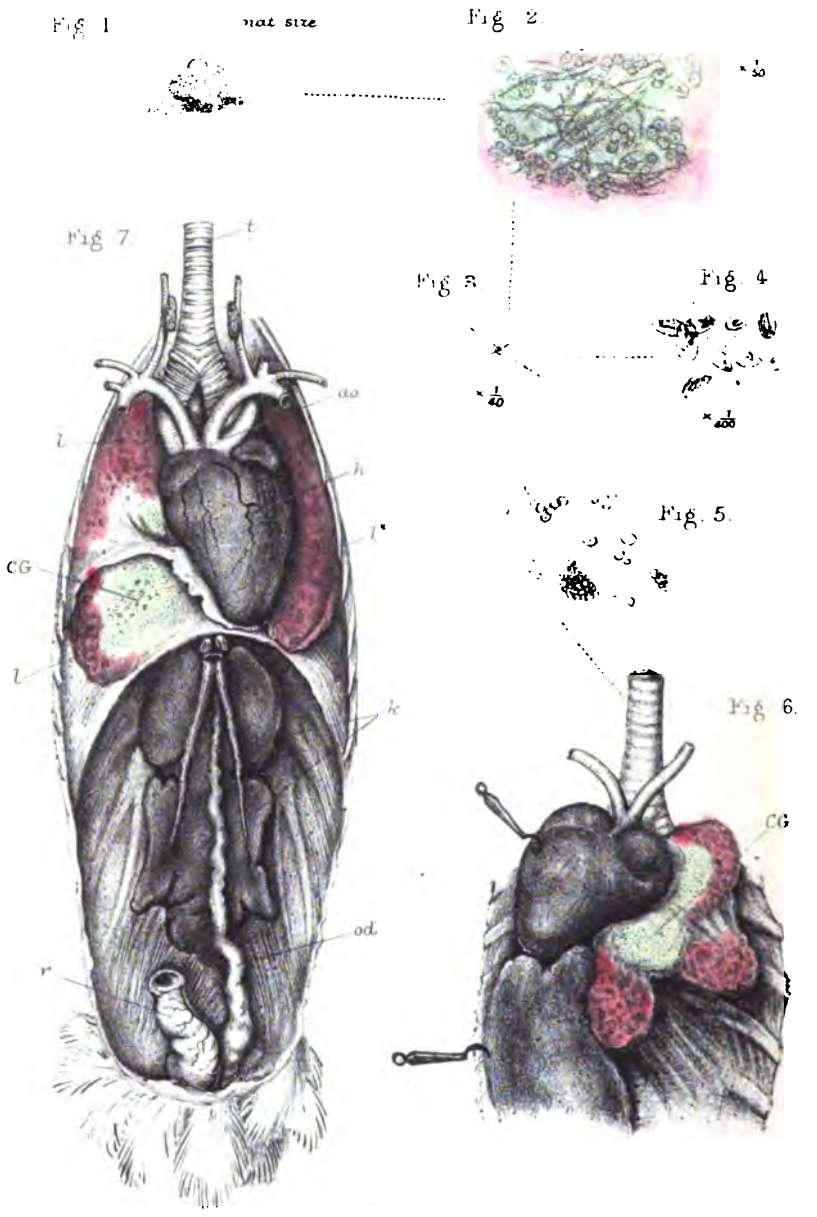
Fifteen gentlemen, proposed as members, were balloted for, and duly elected.

Mr. Bott, F.G.S., then read a paper "On the Chalk Formation," which, being in no way microscopic, we of course cannot publish in this Journal.

After it was read, a vote of thanks was unanimously accorded to Mr. Bott for his valuable paper.

The meeting then resolved itself into a conversazione; a paper having been announced for the next meeting, on Tuesday evening, February 20th, by Mr. Stewart, M.R.C.S., "On the Application of Polarized Light to Microscopical Subjects."

* See 'South London Press,' &c., Dec. 9, 1871.



C. Bourgeau del. et lith. J. Marie. W. West & Co. imp.

Vegetable Organisms on Pleura of Living Birds.

THE

MONTHLY MICROSCOPICAL JOURNAL.

APRIL 1, 1872.

I. — *On the Development of Vegetable Organisms within the Thorax of Living Birds.** By Dr. James Murie, F.L.S., F.G.S., &c.; Lecturer on Comp. Anat. Middlesex Hosp.; formerly Pathol. Glasg. Roy. Infirmary; Assist. Conservator Roy. Coll. of Surg., Eng.; and late Prosector Zool. Soc., Lond.

(*Read before the* Royal Microscopical Society, *Jan.* 3, 1872.)

Plate XII.

The fact of lowly-organized vegetable structures finding a nidus and flourishing in the bodies of animals and man, is one well known to microscopists, physiologists, and pathologists. When these microscopical parasites, Algæ or Fungi, as the case may be, effect a lodgment on the exterior of the body, whether of the lower or higher animals, they not infrequently give rise to peculiar, tegumentary diseases. Such, for instance, are,—the forms of Favus

EXPLANATION OF PLATE XII.

Fig. 1.—Portion of the vegetable organism and its plastic membranous nidus from the pleura of the Great White-crested Cockatoo, *Cacatua cristata*, Linn. Sketched when fresh and of natural size.

„ 2.—Magnified view of part of same examined under a low power, and showing filamentous threads (mycelium) and echinulate spores.

„ 3.—Crucial arrangement of two filaments, at the junction of which a spore is attached.

„ 4.—Some of the cells and spores of the above highly magnified.

„ 5.—Sporules and basement membrane from cryptogamic growth in the Rough-legged Buzzard, *Archibuteo lagopus*, Gm.

„ 6.—Anatomical sketch of the thorax of the Rough-legged Buzzard, the heart being dragged to the right side to expose the cryptogamic vegetation on the surface of the lung.

„ 7.—The opened thorax and abdomen of the Kittiwake Gull, *Rissa tridactyla*, Linn. Designed to illustrate the natural position of the fungus patch towards the visceral organs; the gullet, stomach, liver, and intestines having been removed.

The lettering to this and preceding figure is as follows:—cg, cryptogamic growth. *l l* right, and *l** left, lung. *h*, heart. *ao*, aorta and other vessels. *t*, trachea. *k*, left kidney. *od*, oviduct. *r*, rectum.

* An abstract of this paper was laid before the Biological Section at the meeting of the British Association in Edinburgh, August, 1871.

of the dermatologists; the matted condition of the hair peculiar to the Polish people, hence termed Plica Polonica; the "fur"-like exudation from the bodies of flies;* the "mould" of gold-fish;† and many others more or less known, but too numerous to mention.

But while for the most part the vegetable microscopical parasites have a location on the surface of the body, it by no means follows that the internal organs are free from them. On the contrary, they abundantly flourish in the whole length of the alimentary canal.

Besides many well-known varieties developed in the mouth, gut, &c., one peculiar form from the stomach, *Sarcina ventriculi*,‡ has been described by the late Professor Goodsir§ and others.

It may be remarked, that in the greater number of instances where minute vegetable organisms attach themselves to interior parts and organs, these have directly or indirectly communication with the atmosphere; and hence their production is accounted for by aerial conveyance of the spores.

A fresh interest accrues in those cases where epiphytes arise within closed cavities of the animal body, and as it were spring haphazard from dense living tissues. Does such phenomena bear upon those doctrines of "spontaneous generation" which have been so noisily bruited of late? At all events, here the mode of entrance of the germs, their subsequent development, botanical characters as elucidated by the microscope, relations in different animals, tissues attacked whether primarily diseased or caused by the parasitical implantation, are all questions of fundamental importance.

Perfectly aware of shortcomings and how many problems remain to be solved as respects the life history of these cryptogamic plants in deep-situate vital parts, I yet do not hesitate to place on record some limited observations *en passant*. This, partly to give a faithful anatomical sketch of the vegetation *in situ* (a desideratum in cases of kind), and partly to stimulate others to further researches in a field truly within the sphere of the members of this Society.

Three instances have come under my own immediate inspection, in which a vegetable-like mould has been developed within the thoracico-abdominal cavity of members of the feathered tribe; and the position and microscopical characters of which I herewith describe.

1. An adult male specimen of the Great White-crested Cockatoo, *Cacatua cristata*, Linn., was received in exchange by the Zoological Society, on the 9th May, 1868, and died on the 6th June following.

* See Dr. Ferdinand Cohn's admirable memoir, 'Empusa muscæ und die Krankheit der Stubenfliegen.'

† Dr. Hughes Bennett, 'Roy. Soc. Edinb.,' vol. xv., p. 284.

‡ Placed by Robin, 'Végétaux Parasites,' under the genus *Merismopædia* of Meyen.

§ 'Edinb. Med. and Surg. Journ.,' 1842, p. 430.

For several days prior to death the bird had diarrhœa, but no other symptom of illness was noticed. In my examination of the body, the most notable feature was the presence of a layer or thin film of a pale greenish, mouldy, fungus-looking substance upon the pleural surface of the lungs. The underlying pleura was somewhat thickened and of a delicate roseate tint, apparently from injection of the minute capillary vessels. The lungs themselves exhibited spots of lobular pneumonia, that is, were irregularly hepatized. The intestines were nearly empty, the contents alone being bad-smelling fluid matter.

The cryptogamic growth as exposed on the pleural surface was little over 0·25 inch in diameter, and irregularly outlined. To the eye it looked like a mouldy patch, of a white colour with a greenish tinge. A hand lens showed a diminutive forest of suberect filamentous structures. A portion of the mould placed under the compound microscope, with a low power revealed constituents as subjoined. An abundance of linear filaments crossing and so interwoven with each other as to form a kind of open reticular meshwork. Some of these filaments were simple threads, but the greatest number had a denticulate character. Distributed amongst the capillary meshes were innumerable echinate circular cells apparently nucleated. These cells or spores here and there had attachments to the filaments, and in some cases at the margin of the field I observed a crucial or stellate figure produced by a spore being situate at the junction of two obliquely-placed threads. With a still higher magnifying power I could distinguish a set of oval or elliptical cells distinctly nucleated.

2. In the case of a European Rough-legged Buzzard, *Archibuteo lagopus*, Gmelin, whose pathological condition presented no other feature of importance, I likewise detected a fungous-like vegetable growth spread over the pleural membrane of the left lung. This bird, also an adult male, had been only about a month in the Gardens, and died on the 31st August, 1868, no symptom of disease meanwhile having been noticed.

In this case a greater area of the lung was covered by the mould, but which in colour and general appearance was a counterpart of that of the cockatoo.

3. The third example was that of a female Kittiwake Gull, *Rissa tridactyla*, Linn., which was received by the Zoological Society 31st December, 1868, and died on the 7th March, 1869. For a short time it was noticed to pine, and take its food badly. The body on death was in a poor condition, although its plumage seemed good. The viscera showed nothing specially worthy of note, excepting the right lung and its investing membrane. The latter had attached to it a pale greenish film of vegetable mucor similar to the two foregoing birds, and in this case, whether primarily or as a

secondary consequence, the pulmonary tissue itself had become somewhat atrophied. The vegetable parasite, moreover, seemed to have extended its areal distribution towards the ribs and breast-bone, the inner lining membrane (periosteum) of which, and even the bones themselves, were roughened.

The microscopic characters of the cryptogamic substance in this case agreed in most respects with that of No. 1; the echinate cells and filamentous threads being very distinct. Some other duties occupied my immediate attention at the time, so I could only satisfy myself of near identity with former cases, whilst my artist produced a faithful drawing of the vegetation *in situ*.

Probably one of the earliest, well-authenticated observations of vegetable growth within the body of a bird is that of the celebrated John Hunter, in his notes on the dissection of a Sea-gull (*Larus marinus*).* I quote it in full; for, from only being published of late years, though it is a century since his memorandum was written, Continental and other authorities do not allude to it. Moreover, it possesses another interest from the gull having been alive when undoubtedly the growth sprung up; for some writers have hinted that fungi may be developed subsequent to death, which this instance demonstrates to the contrary.

Hunter says:—"This bird I kept in the garden upon flesh for about a month; and about four or five days before I killed it, it was taken ill, so that it did not eat, but grew worse and worse till I killed it. On opening the abdomen I saw a number of white spots; some on the kidneys, some on the membranous partitions, and others upon the stomach. These I found to be chiefly mould: some parts of them were green, and had a down [Mucedo] upon them. This mould must have formed before death; and, although one can hardly believe it, yet so it must have been, for it was seen in less than twelve hours after death; and if a little could have been produced in that time, yet the whole could not, for some of it was more than a quarter of an inch thick. The membranous partitions were thick and inflamed, and I account for this mould in this way. We know that there is air within the cavity of the abdomen, which is taken in by the windpipe and goes through the lungs, from thence through the diaphragm into the abdomen. Now, if this air was at all confined, and did not get out for a supply of fresh to get in, it would certainly putrefy the juices that were thrown out by inflammation, and these juices might become mouldy before death. This is a hint that the air in an emphysema should be let out, and not allowed to become putrid."

Dr. Bennett's paper, already referred to, contains a succinct repertory of facts and literature concerning parasitical vegetables

* 'Essays and Observations,' edited by Prof. Owen, 1859, vol. ii., p. 329.

on living animals; but with respect to development within the thorax, &c., of birds, Dr. Charles Robin's* résumé has more completeness. The latter describes and refers to several kinds of fungi found by competent observers in situations nearly identical with those I have detailed. The following true, or supposed, distinct species of *Aspergillus* are denoted by him:—

A. candidus, Micheli.
A. glaucus, Fries.
A. nigrescens, Robin.
A. —— sp. ?, Müll. and Retz.—specimen from the Snowy Owl.
A. —— sp. ?, Mayer—specimen from the Jay.
A. —— sp. ?, Deslongchamps—specimen from the Eider Duck; Robin says probably *A. glaucus*.

According to M. Robin's determination of the epiphytes, six forms are noted; yet he only ventures to name three species, and one of these, his *Aspergillus nigrescens*, seems at present rather to be distrusted by our highest English authorities on Mycology. This reduces the really certified specific forms to two, *A. candidus* and *A. glaucus*.

As regards the cryptogamic vegetations which I have encountered, are they algæ or fungi? I put it so, as at the meeting of the British Association in Edinburgh, 1871, Mr. Cooke threw out the hint of the possibility of their belonging to the first-mentioned group. I had myself stated, and believe I have fair grounds for considering them to be members of the Order Mucedines, and species of the genus *Aspergillus*. The location, visceral cavity of a bird, is that wherein previous observers have registered their occurrence. The microscopical structure, I presume, endorses the opinion; for we have in the interwoven thread-like cells a representative of mycelium, and in the cells, echinulate spores identical with those of *Aspergillus glaucus*. On comparing my drawings of the microscopic structures with those of Müller and Retzius,† Deslongchamps,‡ Spring,§ and Robin,‖ I find them in most respects identical. A careful revision, moreover, of descriptions and original figures of *Aspergillus*, of several botanical writers,¶ establishes additional conviction.

* 'Hist. Nat. des Végétaux Parasites qui croissent sur L'Homme et sur les Animaux Vivant.' Paris, 1853, p. 545 *et seq.*

† 'Ueber Parasitiche Bildungen,' *l. c.* hereafter, Tab. viii., fig. 3.

‡ 'Note sur les mœurs du Canard Eider et sur des moisissures dévelopées pendant la vie, à la surface interne des poches aériennes d'un de ces animaux,' *l. c.* pl. xi., figs. 4, 5, 6.

§ 'Sur une Mucédinée dévelopée dans la poche aérienne abdominale d'une Pluvier Doré,' *l. c. infra*, Figs. 2 and 3 in plate accompanying paper.

‖ *L. c.* Champignons, pl. ii., fig. 4, *a*, *b*, *c*, *d*, *e*, *f*, et fig. 13.

¶ Sowerby, 'Eng. Fungi,' Lond., 1797–1803, pl. 378, figs. 5 to 9; Meyen, 'Pflanzenphysiologie,' figs. 20–22, pl. x., vol. iii.; Berkeley, 'Introd. to Cryptogamic Botany,' fig. 68, &c.

Including those already mentioned, the birds in which fungi have been discovered are as follows:—

European Rough-legged Buzzard, *Archibuteo lagopus*, Gmelin.
Red Buzzard, *Buteo rufus* (*Falco rufus*)?*
Snowy Owl (*Strix*), *Nyctea nivea*, Daud.†
The Jay, *Corvus glandarius*, Temm.‡
Common Bullfinch, *Pyrrhula vulgaris*, Linn.§
White-crested Cockatoo, *Cacatua cristata*, Linn.
Parakeet, —— Gn., —— sp. ?‖
The Pigeon, *Columba*.‖
Domestic Fowl, *Gallus*.‖
Common Pheasant, *Phasianus colchicus*, Linn.¶
Golden Plover, *Charadrius pluvialis*, Linn.**
Ruddy Flamingo, *Phœnicopterus ruber*, Linn.††
Stork, *Ciconia*, —— sp. ?‡‡
Common Swan, *Anas olor*, Gmelin.§§
Eider Duck, *Somateria* (*Anas*) *mollisima*, Linn.‖‖
Kittiwake Gull, *Rissa tridactyla*, Linn.
Great Black-backed Gull, *Larus marinus*, Linn.

From the foregoing it results that seventeen kinds of birds are liable to attacks of fungi, either the precursor and causing, or a sequence of disease terminating fatally. Different avine groups are each represented, Raptores, Passeres, Scansores, Gallinacea, Grallatores, and Natatores, so that we may infer the entire class is predisposed under given but as yet unknown conditions.

I think it will hardly be advanced by anyone that these cases support the idea of spontaneous derivation, seeing that spores might be carried by the lungs close to the seat of their germination; penetration of the tissue and development thereafter following in quick succession. The route of entrance of the germs by the respiratory organs is more than a likely one; for, as Berkeley¶¶ very justly says, "the spores of our common mould float about everywhere; and as they grow with great rapidity, they are able to establish themselves on any surface where the secretion is not sufficiently active or healthy to throw off the intruder. Where the spores are very abundant, they may sometimes, like other minute bodies, obstruct the minute cells of the lungs."

The penetration of the spores through the pulmonary and pleural tissues is a matter of less wonder,*** judging from what is

* Observed by Herr Dubois, quoted by Johannes Müller, *infra*.
† Müller and Retzius, Müller's 'Archiv,' 1842, p. 203, Taf. viii., ix.
‡ Mayer, in Meckel's 'Archiv,' 1815, p. 310.
§ MM. Rayer et Montague, 'Journal Institut.,' 1842, p. 270.
‖ E. Rousseau et Serrurier, 'Compte Rendu,' 1841, xii., p. 18.
¶ Robin, 'Soc. de Biolog.,' Juin 20, 1848, and 'Vég. Par.,' p. 258.
** Spring, 'Bull. l'Acad. Sci. Belg.,' 1848, t. xv., p. 486.
†† Owen, P.Z.S., 1832, p. 141.
‡‡ Heusinger, 'Bericht v. d. Kong. Zoot. Anst. z. Wurzburg,' 1826, p. 26.
§§ Jaeger, Meckel's 'Archiv,' 1816, p. 354.
‖‖ E. Deslongchamps, 'Ann. des Sciences Nat.,' 1841, p. 371, pl. 11.
¶¶ 'Outlines of British Fungology,' p. 69.
*** *Vide* Robin's remarks, *l. c.*, "Pénétration des spores dans les cavities closes," p. 283.

known respecting fungoid development in insects, wood and other hard substances.

I may here note that Johannes Müller thought he had discovered another cryptogamic form of the genus *Peziza* in the cases which came under his observation. This moot point has been discussed by M. Spring and Professor Robin. From what I have seen in the three birds, Robin's demonstration of the supposed *Peziza* being only circular spots of basement or false membrane, upon which matrix the mycelium of *Aspergillus* is borne, appears the correct view. In my own case I easily recognized a pellicle of fibrinous exudation with the characters attributed and carefully figured by Deslongchamps.

The interest at present attached to cryptogamic developments as a source of disease, has given rise to much discussion on the Continent; and in this country, along with the theory of spontaneous generation, almost a literature of its own.*

It is not my intention to enlarge on the pathological aspect of the present cases, but the impression left on my mind is that to the development and fructification of the Mucedines the death of the birds may be attributed, inasmuch as the specific lesion was due to their presence. But I couple as a proviso my impression that an antecedent state of the system admitted of the development of the fungus.

To those who doubt the capabilities of fungoid growths in active vital parts, solid and otherwise, I would advise them carefully to study Robin's chapter v. (*l. c.*, p. 278), "Action Exercicée par le Végétal sur l'Animal;" to be convinced that the so-called Mycetoma (Madura, or Fungus-foot of India) is quite within the range of possibility, irrespective of primary putrefactive process.

Dr. Leidy, no mean observer, on discovering many new epiphytic forms, says,† with justice,—that we have entophyta in luxurious growth within living animals, without affecting their health; yet at the same time he admits that there are cryptogamia capable of producing and transmitting disease, as in the case of muscaridine, &c.

P.S.—Prof. W. T. Thistleton Dyer has lately drawn my attention to the Reports on Botany, Ray Soc., 1846, p. 424, where it is mentioned a Scaup Duck, *Fuligula marila*, Linn., had mucor or blue mould in the lungs and membranes.‡ Other cases I have quoted are also referred to in the Report (*l. c.*).

* I need here but refer to Prof. Huxley's Address, Brit. Assoc., Liverpool, 1869; Prof. Lionel Beale's 'Protoplasm,' his 'Disease Germs,' 1870; and Dr. Bastian's 'Modes of Origin of Lowest Organisms,' and 'The Beginnings of Life,' 1871, as typical of what I mean.

† 'Proc. Philad. Acad.,' 1848–9, vol. iv., pp. 228, 229.

‡ Yarrell, in 'Ann. Nat. Hist.,' vol. ix., p. 131.

II.—*Some Remarks on the Finer Nerves of the Cornea.*

By Dr. E. Klein.

(*Read before the* Royal Microscopical Society, *March* 6, 1872.)

Plates XIII. and XIV.

It is not long since that I had the opportunity to give a description of the nerves of the cornea.* If I undertake to speak again on the same subject, there are two reasons: first, I am able by using a new method—though only a modification of the common method of staining with chloride of gold—to demonstrate very easily, and with great certainty, even the finest nerve fibres.

The great advantage which this method has over the one I have previously described is, that the preparations show most abundantly the finer nerve fibres, and that the latter come into view very sharply defined; accordingly they can be examined with the greatest ease even under a lower power, as 250 to 300, and with a small amount of light.

EXPLANATION OF PLATES XIII. AND XIV.

Nerves of the Cornea.

Fig. 1.—Horizontal preparation of nerves of the proper substance of the rabbit's cornea. *a*, coarser non-medullated nerve trunk. *b*, finer non-medullated nerve fibres. × 300.

„ 2.—Horizontal preparation of the sub-epithelial nerves of the guinea-pig's cornea. *a*, coarser non-medullated nerve trunks of the sub-epithelial plexus. *b*, finer non-medullated nerve fibres. *c*, finest non-medullated nerve fibres of the sub-epithelial network. × 300.

„ 3.—Oblique section through the deeper epithelium of the anterior surface of the rabbit's cornea, and the superficial layers of the proper substance. *a*, coarser non-medullated nerve trunk. *b*, brushes of fine non-medullated nerve fibres. *c*, fine non-medullated nerve fibres of the deep intra-epithelial network. *d*, deeper epithelial cells. × 300.

„ 4.—Horizontal preparation of the rabbit's cornea. *a*, coarser non-medullated nerve trunks. *b*, fine non-medullated nerve fibres. *c*, brushes of finest sub-epithelial nerve fibres. × 300.

„ 5.—Horizontal preparation of the guinea-pig's cornea. Superficial intra-epithelial network of fine non-medullated nerve fibres. × 300.

„ 6.—Horizontal preparation of the rabbit's cornea. Superficial intra-epithelial network of fine non-medullated nerve fibres. × 300.

„ 7.—Horizontal preparation of the frog's cornea. *a*, non-medullated nerves of the coarser plexus of the proper substance of the cornea (nerves of the first order). *b*, *c*, and *d*, fine non-medullated nerve fibres (nerves of the second and third order). × 300.

„ 8.—Horizontal preparation of the frog's cornea. *a*, non-medullated nerve trunk of the first order. *b*, non-medullated nerve fibres of the second order. *c*, non-medullated nerve fibres of the third order. *d*, non-medullated nerve fibres of the fourth order. *e*, corneal corpuscles. *f*, nuclei of the same. Hartnack Immersion, Nov. 10. Copied from the 'Quarterly Microscopical Journal,' October, 1871.

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* 'Quarterly Microscopical Journal,' October, 1871.

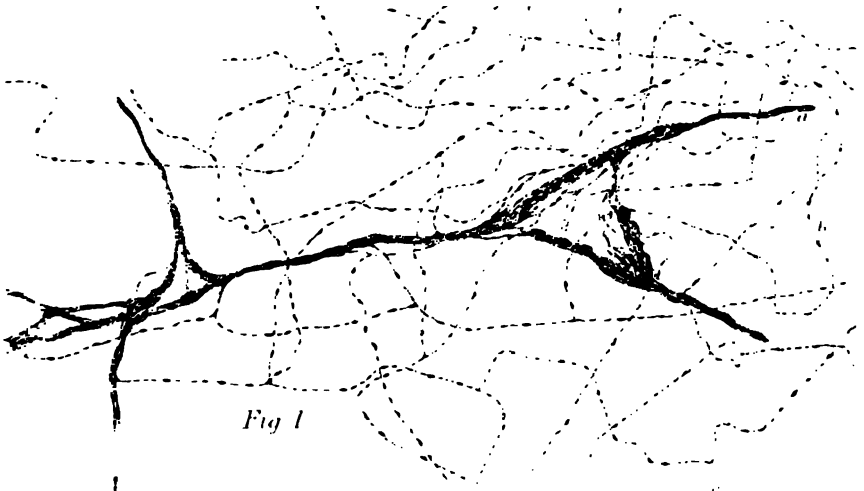

Fig 1

Fig 2

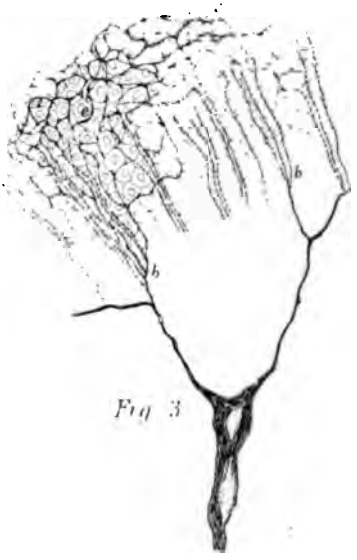

Fig 3

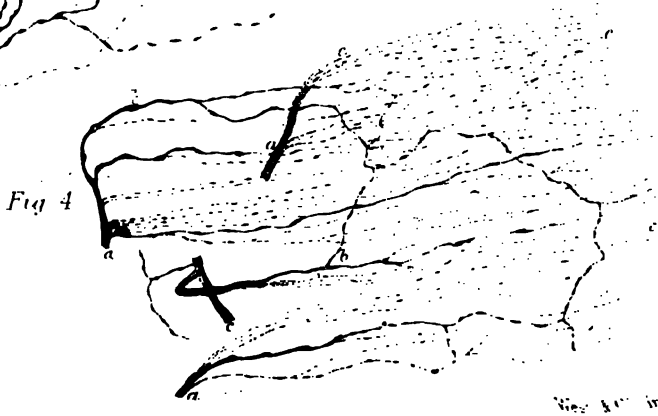

Fig 4

E. Klein del. — New Yor... inv.

The Intimate Structure of the Cornea.

This method is as follows:—The cornea of a rabbit or a guinea-pig is cut out (it is of no consequence if this is done a quarter or half an hour after death) and placed in a half per cent. solution of chloride of gold. The cornea of the rabbit remains in the solution for from one hour and a half to two hours; that of the guinea-pig for from one hour to one hour and a quarter. After that the cornea is washed in distilled water, and exposed to the light in distilled water for from twenty-four to thirty-six hours (the water being changed twice, or oftener); after this time has elapsed the cornea is transferred into a mixture of one part pure glycerine and two parts distilled water, where it remains for two or three days. Up to this time the cornea has not assumed a darker colour than ash grey, perhaps having in a small degree a violet tint; at all events, the whole of the cornea is transparent. The cornea is then brushed over on its anterior surface under water with a fine camel's hair brush very gently, so as to remove the precipitates of the gold salt.

It is quite sufficient to brush it only once very gently and delicately, so as to clear the surface, even if not over its whole extent, still over the greater portion of it. No great practice is required to obtain from such a cornea, with a sharp razor, thin horizontal and oblique sections, by placing it simply with its concave surface on the volar side of the last phalanx of the first finger and fixing its scleral border with the thumb of the same hand. The sections are examined in glycerine.

The sections which are made in an oblique direction through the anterior epithelium and the superficial portions of the proper substance of the cornea are as valuable as the horizontal sections. The latter are necessary for the examination of the sub-epithelial nerve fibres as well as for the intra-epithelial nerve fibres, both separately, although in a binocular microscope there is no difficulty whatever in seeing how the latter spring off from the former.

The examination of the oblique sections proves this thoroughly.

I have prepared, by the above-mentioned method, sixteen corneæ in a relatively short time, and not one of these has failed. Of course, not all of them were in all parts equally perfect.

I do not wish to give any long description of the relation of the sub-epithelial plexus of the coarse non-medullated nerve fibres, or, more properly speaking, of the sub-epithelial plexus of the nerve trunks, nor of the sub-epithelial network of fine fibres, nor of the immense number of very fine fibrillæ which spring off, in bundles like a brush, from several branches of the sub-epithelial trunks, which branches run close to the under surface of the anterior epithelium. I wish only to mention shortly the fine nerve fibrillæ which enter the epithelium, and there form a deep and a superficial intra-epithelial network. I refer simply to the description I have already given, and to the adjoining figures.

In all the preparations obtained by the method above mentioned, the nerve fibres, up to their finest ramifications, appear as sharp, outlined structures, of a dark colour. No other element is coloured besides the nerve fibres. Of the cellular structures of the proper tissues of the cornea nothing is to be seen. Although the epithelium is also not, at least not for a pretty long time, coloured, still its every cell is very distinctly to be seen.

In all these preparations the fact which can be deduced with certainty is, that only the trunks of the sub-epithelial plexus, which as well as the trunks which are situated deeper in the substance of the cornea are only bundles of fine nerve fibrillæ, are provided with a nucleated sheath. In all other nerve fibres, *i. e.* the nerve fibres in the proper substance of the cornea (see Fig. 1), the fine nerve fibres of the sub-epithelial network, as well as the very delicate fibrillæ, which close under the epithelium spring off like a brush (see Figs. 2, 3, and 4), and, finally, the nerve fibrillæ of the intra-epithelial networks,—in all these, I say, there is nothing like nucleus to be found.

As regards the frog's cornea, I also refer to the description and illustrations I have given in my first memoir in the 'Quarterly Journal of Microscopical Science.' Here also only the nerves of the coarser plexus are provided with a nucleated sheath. The nerve fibres, which I have described as nerve fibres of the second order, and which run very often in a winding course, sometimes also rectilinear, exhibit nuclei only in very few places. The nerve fibres of the third order, which run mostly rectilinear, and which are distinguished like the fine nerve fibres in the rabbit's cornea by more or less regular varicosities, no longer exhibit nuclei. These nerve fibres of the third order, which remain for a long distance undivided, join by rectangular fibres, so as to form a trellis-work, the meshes of which are relatively broad; in the same manner also the nerve fibrillæ of the fourth order, which join to a dense network upon the corneal corpuscles themselves, have no nuclei.

We find therefore also in the cornea of the frog, that the finer nerve fibres do not possess any nucleus. The special mode in which the finer nerve fibres of the corneal substance run is not a peculiar one, if we consider that the arrangement of the bundles of the corneal tissue is a regular one, and that the nerve fibres can only run between these bundles.

Whether the fine fibres described in the rabbit's and the frog's cornea are nerve fibres at all cannot be the subject of any doubt, if we admit, as regards the microscope, that our mental eye is not able to deny what our normal and healthy bodily eye sees as an atomical continuity.

The second reason why I take the opportunity to speak on the

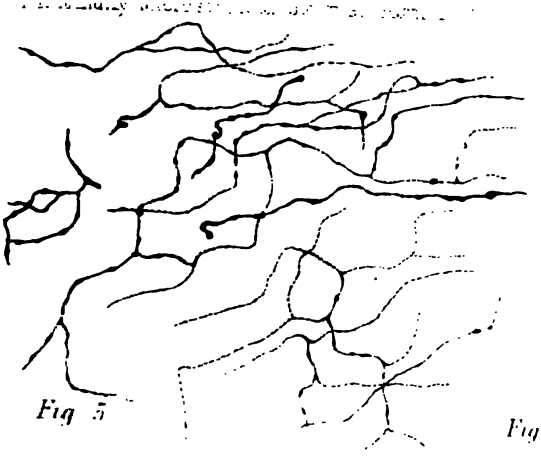

Fig. 5

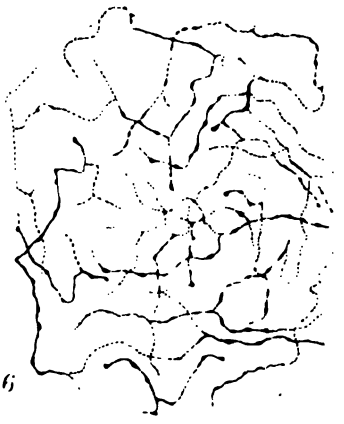

Fig. 6

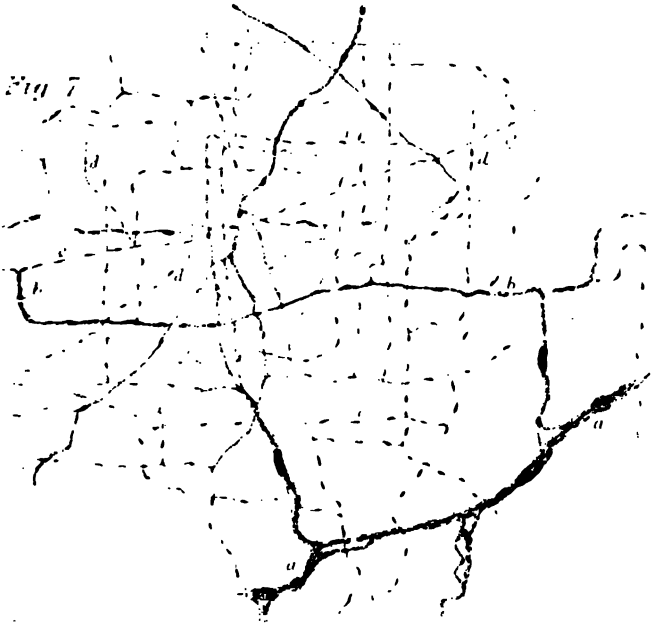

Fig. 7

Fig. 8

nerves of the cornea is that some remarks have been made by Dr. Beale before this Society, on finer non-medullated nerve fibres; how far these are right I shall be able to show. First of all, I find in his memoir in the 'Monthly Microscopical Journal' for January, 1872, page 4, the following sentences:—"My own conclusions on the ultimate distribution of nerve fibres were formed several years ago, at a time when terminal nerve-networks were denied in Germany, and it was supposed that only in a few exceptional cases did the axis cylinder of a nerve extend beyond the white substance. Not only are my networks of pale nucleated nerve fibres now accepted, but it is maintained that much finer networks of nerve fibres ramifying upon and amongst epithelial cells and other elementary parts, and even upon an individual mass of bioplasm (nucleus) have been demonstrated. At present, however, I cannot regard the observations upon which it is thought to establish this view more conclusive than those which a few years since led many to the conclusion that the axis cylinder sprang from the nucleus or nucleolus of the central nerve cell."

The first descriptions of a terminal network of non-medullated pale nerve fibres are due to Remak. Remak* described networks of non-medullated nerve fibres, which surround the striped muscular fibres, and which Remak regarded as terminal networks.

As regards the striped muscles, these assertions of Remak have been confirmed by Schaafhausen,† by the aid of carmine and staining. Next comes the assertion about terminal networks of non-medullated nerve fibres of common sensation in the skin and the mucous membranes. Kölliker, in 1856, described a network of pale nucleated non-medullated nerve fibres in the skin of many mammals.‡ Kölliker did not regard this network as a terminal one.

About the same time, Axmann describes network of non-medullated nerve fibres in the skin of the frog. Further, I have to state that His described in the substance of the cornea of mammals a terminal network of pale non-medullated nerve fibres.§

Then I have to notice the assertions of Meissner of a terminal plexus of non-medullated nerve fibres in the submucosa of the intestine.‖

A short time after that Billroth described in different places of the mucous membrane of the digestive tract terminal networks of pale non-medullated nucleated nerve fibres.¶ Next comes the confirmation of His's terminal network of non-medullated fibres in the

* 'Archiv für Anatomie der Physiologie,' 1843, p. 189.
† 'Amtl. Berichte der Naturforscher.' Vers. zu Bonn, 1859, p. 193.
‡ 'Zeitschrift für wissenschafll. Zoologie,' Bd. VIII.
§ 'Beiträge zur normalen und pathologischen Histologie der Cornea.' Basel, 1856.
‖ 'Zeitschrift für rationelle Medizin,' VIII., p. 364. 1857.
¶ Müller's 'Archiv,' 1858, p. 148.

cornea of mammals by Arnold,* who, in 1862,† described also in the conjunctiva of several mammals terminal networks of non-medullated nerve fibres. Saemisch then amplified our knowledge about the terminal network of non-medullated nerve fibres in the cornea of mammals.‡

Further we have to notice, that as regards the smooth muscular fibres Kölliker made assertions about non-medullated nerve fibres in the smooth muscular tissue of the urinary bladder of the frog;§ according to Kölliker the non-medullated nerve fibres form on some places a network both in the muscular tissue as well as on very small arteries and veins.

Then Auerbach described in the smooth muscular wall of the intestine non-medullated nerve fibres forming plexus.‖ After that Klebs described networks of pale non-medullated nerve fibres in smooth muscular tissue,¶ and still later ** Arnold and His described terminal networks of pale nucleated nerve fibres in the muscular coat of large vessels.

Beale's first assertion about a terminal network of non-medullated nerve fibres refers to striped muscles, 'Philosophical Transactions,' 1860, and therefore seventeen years after Remak and one year after Shaafhausen. Beale has been supported by Kölliker (1862).

About the capillary vessels Beale was the first and the only observer who described non-medullated nerve fibres, which accompany the vessel, bend round it, and join by lateral branches;†† it is further due to the truth to say that Beale is the only one who described (and this was pointed out long before also by Klebs, Virchow's 'Archiv,' 1865, Bd. 32, p. 185) a real network of non-medullated nucleated nerve fibres in the muscular coat of smaller arteries.‡‡ Beale described (and illustrated by most beautiful illustrations) that in the papillæ fungiform as well as in the papillæ filiform the non-medullated nerve fibres form a plexus.§§ In the papillæ fungiform Beale is not quite sure whether the fine nerve fibres terminate below the epithelium, or between its cells.

We see therefore that the second stage of the doctrine of the nerve termination (the first stage being this, that the medullated nerve fibres bend round and back in loops), that is, the doctrine of

* Heidelberg, 1860.
† Virchow's 'Archiv,' Bd. 24.
‡ 'Beiträge zur Anatomie des Auges.' Leipzig, 1862.
§ 'Verhandlungen der phisik.—medicin. Gesellschaft zu Wurzburg,' 1862. 1 Heft. January. p. 1.
‖ 'Ueber einer plexus myentericus.' Breslau, 1862, p. 12.
¶ 'Centralblatt,' 1868, No. 36.
** Virchow's 'Archiv,' Bd. 28.
†† 'Philosoph. Transact.,' 1863, pl. lx., figs. 44 and 47; and 1865, pl. xxii., fig. 15.
‡‡ 'Philosoph. Transact.,' 1863, p. 563, pl. xl., figs. 45 and 46.
§§ Ibid., 1865.

terminal networks of non-medullated nerve fibres, is, except the capillaries and the muscular coat of small arteries, neither in striped muscles, nor in the cornea, nor in the skin, nor mucous membranes of different kinds, due to the source which Beale asserts in the above-mentioned paper,* although he has advanced this doctrine in many respects in a remarkable degree.

On the other hand, further investigations show, that THIS doctrine of terminal networks has been shaken in many respects. I wish to call attention to the fact that in striped muscles the inter-muscular network was not accepted as a termination, neither at the time of Shaafhausen, nor of Beale, nor up to the present time; that is proved by the great number of excellent researches on nerve terminations in striped muscle in the most various classes of animals, by Doyère, Meissner, Wedl, Walther, Munck, Kölliker, Rouget, Krause, Engelmann, Kühne, Cohnheim, &c., &c. By these observers it has been shown, that the nerve fibres do not terminate as inter-muscular, but that there exist also intra-muscular fibres, whose exact mode of termination in various cases has not yet been fixed. Of course if Beale, even at present, adheres to an inter-muscular termination, I am quite able to understand his inability to proceed further, because by his method of preparation, only inter-muscular nerves can be shown.

A good fresh preparation of isolated muscular fibres, which Dr. Beale does not appear to have ever examined, will show him that even in the frog, not to mention the muscles of insects and reptiles, there are intra-muscular nerves.

In smooth muscular tissue it has been shown further, that it is possible to trace fine non-medullated nerve fibres beyond this known plexus.†

As regards the nerves of common sensation, in the cornea substance, first of all, Kühne‡ has shown that beyond the supposed terminal network there are to be traced very fine nerve fibrils, which are, according to Kühne, in communication with the corneal corpuscles. Then it has been shown by Floyer,§ that from the sub-epithelial network of fine non-medullated nerve fibres spring off fine fibrils, which enter between the cells of the anterior epithelium. Cohnheim,‖ Kölliker,¶ and Engelmann** have extended this assertion to a very remarkable extent.

As regards the skin, it has been shown that beyond the plexus

* 'Mon. Micr. Jour.,' January, 1872.

† Klebs, *l. c.*, Frankenhäuser, 'Die Nerven der Gebärm und ihre Endigung in den glatten Muskelfasern,' 1867; Arnold, cap. iv. Stricker's 'Histologie.'

‡ Kühne, 'Gazette Hebdom.,' tome ix., Paris, 1862; und 'Untersuch., über das Protoplasma,' Leipzig, 1864.

§ Floyer, 'Archiv für Anatom. und Phisiolog.,' 1866. Heft II., p. 180.

‖ Cohnheim, 'Centralblatt,' May, 1866; No. 26, Virchow's 'Archiv,' 38 Bd.

¶ Kölliker, 'Sitzungsber der phisik—medic. Gesellsch.' zu Wurzburg, Juni, 1866.

** Engelmann, 'Die Hornhaut des Auges.' Leipzig, 1867.

of non-medullated nerve fibres there are to be traced fine non-medullated nerve fibres amongst the epithelial cells.*

For the mucous membranes it has been shown in the same manner, that beyond the plexus of non-medullated nerve fibres belonging to the proper mucous membrane are to be traced fine non-medullated nerve fibres amongst the epithelium.†

I must admit that I agree with Beale in not defending a free termination for nerves of common sensation. Because I have been able to convince myself, amongst other points (nerves of the tadpole's tail, nerves of the serous membranes, nerves of the blood-vessels): 1. That in the substance of the cornea I could trace beyond the known fine non-medullated nerve fibres, still finer fibrils than those which have been supposed to be in connection with the corneal corpuscles, and which finer fibrils still join to form a network.

2. That the non-medullated nerve fibres which enter the anterior epithelium of the cornea and of the nictitating membrane of the frog, do not terminate in free extremities, but join in a network.

3. That the observations made upon the epithelial nerves of the mucous membrane of the vagina (Chrschtschonovitsch) and mouth (Elin) made under my direction, do not show free terminations.

Beale does not admit that there exist fine nerve fibres which spring off from the already well-known plexus in the cornea, mucous membrane, skin, &c., and enter the epithelium—and regards the above-mentioned plexuses (which as I have pointed out are not to be called *his* plexuses) as terminal. For the nerves of the cornea especially, I am able to demonstrate as clearly as possible that his views are entirely erroneous. What Beale calls finest ultimate nucleated nerve fibres are only compound fibres; of the really finest fibres, which have nothing to do with nuclei, Beale has seen absolutely nothing. I must most decidedly call that view erroneous, according to which the presence of a nucleus is in any way decisive anatomically of fine non-medullated nerve fibres, whether in cornea, skin, mucous membrane, or elsewhere. So long as a non-medullated nerve fibre has a sheath, a nucleus can also be found upon it; but when the fibre has divided into single fibrillæ, or into small bundles of these, the sheath (and consequently the nucleus) is no longer to be found. The fibrillæ which come off from a non-medullated nerve fibre, either singly or in small bundles, have absolutely nothing to do with any nucleus whatever. Beale apparently no longer adheres to his former view‡ that "all fibres which can be followed for a considerable distance, which refract like true nerve fibres, and exhibit an appearance more or less

* Langerhans, Virchow's 'Archiv,' Bd. 44; Podkopaeff, Max Schultze's 'Archiv,' Bd. V.; Eberth, Max Schultze's 'Archiv,' Bd. VI.

† Chrschtschonovitsch, 'Sitzunsber. der Akad. der Wissen in Wien,' 63 Bd., Februarheft, 1871; Elin, Max Schultze's 'Archiv,' Bd. VII., 4 Heft.

‡ 'Philosoph. Transact.,' 1863, p. 565.

granular . . . . . must clearly be pronounced nerves." And,* "In all cases, as far as I can ascertain, the ultimate terminal fibres are pale and granular, exhibiting nuclei at varying intervals." For in a recently-published paper,† which is stated to have been read before this Society on 6th December, 1871, he admits that his delicate nerve fibres are compound, that they are fibrillar, and that the nuclei are usually "situated more or less on one side." We see therefore that he here withdraws from his previously-expressed opinions, although he uses the same Plate‡ to illustrate his new views, which had already done service in illustrating the old ones.§

He partly abandons also his former views, and of this I am very glad, as he admits that‖ "it is quite possible that there may be still finer fibres." This encourages me to hope that by the use of the above-described method of staining with chloride of gold, he will be led entirely to abandon his views about the fine nerve fibres of the sub-epithelial network of the cornea of mammals, about fine nerve fibres among the epithelial cells, and about the presence of nuclei (bioplasts, or germinal matter). In this last paper of Beale's, where he now admits that the delicate nucleated nerve fibres are still compound and fibrillar, and by doing so only confirms what was already known years ago, even from the study of fresh preparations alone, he asserts that the fibrillar structure is to be seen in his preparations, and not in gold or osmium preparations. This I must call a gross error, because on the one hand, as I have pointed out, the very same preparations of his, which, until 1868, had only a granular appearance, in February, 1872, appeared fibrillar. On the other hand, it has been shown in several cases that, both in gold and in osmium preparations, fine non-medullated nerve fibres, which spring off from the plexus of nucleated fibres, still show a fibrillar structure.

I agree with Beale up to a certain point when he says that in general with his method (carmine and glycerine) he is not able to demonstrate more than the plexus of nucleated non-medullated nerve fibres, although I have been in the agreeable position of ascertaining on one of his own preparations of the cornea, that his view that the nucleated fibres are the finest and ultimate, by no means agrees with the facts.

By the help of the gold method we are able to show with certainty that all hypothetical objections, based upon the probability or improbability of the existence of nerve fibres among epithelial cells, vanish completely, as they are at once exploded by simple observation of the facts.

* 'How to Work with the Micr.,' 4th edit., 1868, p. 332.
† 'Mon. Micr. Jour.,' Feb., 1872, p. 47.
‡ Pl. VI., 'Mon. Micr. Jour.,' Feb.
§ 'Philosoph. Transact.,' 1863, pl. xl., fig. 43. ‖ 'Mon. Micr. Jour.,' p. 46.

As regards Dr. Beale's doubting or denying all that he is not himself able to show, I venture to recommend to his attention the following sentences:—. . . . "seems to conclude, in too many cases, that what he has failed to see does not exist." * Further, "It is possible that for many years to come some observers will persist in terming everything in which they fail to demonstrate distinct structure, connective tissue."† Further, "It is remarkable how positively many authorities deny the existence of structures which they have failed to demonstrate."‡ And, finally, "Moreover, many observers seem to have determined in their own minds what appearance a fibre should present to be entitled to be regarded as a nerve, and then they arbitrarily assert that a fibre which does not present these characters cannot be nervous; and even if it be continuous with an undoubted nerve fibre, it is put down as connective tissue."§

* Beale, 'Philosoph. Transact.,' 1863, p. 563.
† Ib., *loc. cit.*, p. 567. ‡ Ib., *loc. cit.*, p. 567. § Ib., *loc. cit.*, p. 568.

III.—*Note on the Resolution of Amphipleura pellucida by certain Objectives made by R. and J. Beck, and by William Wales.*

By Dr. J. J. Woodward, U. S. Army.

(*Read before the* Royal Microscopical Society, *March* 6, 1872.)

In the 'Monthly Microscopical Journal' for Sept., 1871, p. 150, I recorded the resolution of this difficult diatom by an immersion $\frac{1}{8}$th made by Mr. Tolles, of Boston, and sent photographs exhibiting the performance, to the Editor, as well as to the Royal Microscopical Society.

The same desire to do full justice to all honest efforts to improve the microscope, which led me to make that brief publication, induces me now to give like publicity to the performance of the new immersion $\frac{1}{10}$th (so-called) of Messrs. R. and J. Beck.

In the first instance, I thought it worthy of note that, by excellent workmanship and high angle, a lens of such comparatively low magnifying power could achieve so great a feat of resolution. In the second instance, the objective having more than double the magnifying power, the result is only surprising on account of the extremely low price at which the makers have undertaken to furnish it.

Mr. Jos. Beck exhibited during his visit to America an immersion $\frac{1}{10}$th of excellent quality. One was ordered for the Army Medical Museum, which has recently come to hand, and it is of its performance I now speak. These glasses are remarkably cheap as compared with any similar ones hitherto brought to my notice.

The objective received at the Museum is characterized by great flatness of field, freedom from colour, and depth of penetration. Its performance on *Amphipleura pellucida* may be fairly judged from the accompanying photograph,* which is magnified 1025 diameters. On the Nobert's nineteen-band plate I get it satisfactorily through the sixteenth band. As to its magnifying power and equivalent focal length, I obtained the following results:—With 50 inches distance from micrometer to screen the magnifying power was 570 diameters at uncovered, and 630 at full correction for cover. Taking uncovered as a standard, therefore, the focal length should be rated at about $\frac{1}{11}$th inch. The angle of aperture at covered is about 160°.

A few days after taking the photograph of *Amphipleura pellucida* above referred to, I made another photograph of the same diatom with an objective made for the Museum two years ago by Mr. Wm. Wales, of Fort Lee, New Jersey. This is the objective

* Photographs were received from the author, and fully bear out what he states.—Ed. 'M. M. J.'

to which I referred in the 'Monthly Microscopical Journal' for December, 1869, p. 292, as resolving the seventeenth band of the Nobert's nineteen-band plate. The picture of *Amphipleura* obtained exceeds all I have been able to do on this diatom with any objective except the immersion $\frac{1}{16}$th (so-called) of Messrs. Powell and Lealand. I forward a print herewith showing a pair of frustules, both handsomely resolved from end to end, with about 1500 diameters. For comparison I send a picture of the same frustules taken with 1650 diameters by the Powell and Lealand $\frac{1}{16}$th.

The Wales's objective was invoiced to the Museum as a $\frac{1}{30}$th. A determination of its magnifying power, however, shows that at uncovered it magnifies rather less, at covered rather more, than the Powell and Lealand objective. I get by actual measurement the following results:—At 50 inches distance from micrometer to screen this objective magnifies 890 diameters uncovered, 1250 covered. Its equivalent focal length at uncovered is therefore $\frac{1}{16}$th very nearly.

In conclusion, I may add a few words with regard to the use of *Amphipleura pellucida* as a test-object. This diatom is a useful and valuable test for immersion objectives of $\frac{1}{5}$th inch focal length or less. Lower powers can only hope to resolve it if possessed of excessive angular aperture. When, however, it is desired to discriminate between small differences in the excellence of objectives intended for the most exquisite resolution, a more subtle test is required, and this will be found in the nineteen-band plate of Nobert, by those who take proper precautions in its use. Those, however, who believe they have secured resolution whenever they see lines in the higher bands of the plate, without duly considering their number, must not be surprised if objectives they have accepted as resolving the ultimate bands of the plate fail to show the striæ on even the coarsest frustules of the *Amphipleura pellucida*.

IV.—*Stephenson's Erecting Binocular.*

By J. W. Stephenson, F.R.A.S., Treasurer R.M.S., and Actuary to the Equitable Assurance Society.

(*Read before the* Royal Microscopical Society, *March* 6, 1872.)

Plate XV. (Lower part).

In concluding the description of an erecting binocular microscope, which I had the honour of bringing before the Society in the year 1870, I remarked that possibly improvements in the arrangement or angles of the prisms might subsequently be devised.

It soon became evident that this was so, and that the mass of glass used in the lower prisms, by which the light on leaving the objective is divided, and the image laterally inverted, might be considerably diminished by bringing the latter closer to the back combination of the object-glass, and also that by this means higher powers than those which I had hitherto employed might be used; at the same time, for reasons to be hereafter explained, it appeared desirable to alter the angles at which the bodies are inclined to the perpendicular, from 75° to 66½°.

Under the present arrangement, two truncated rectangular prisms are still employed for dividing the light and inverting the image, but their dimensions are so far reduced that they are capable of being inserted into the object-glass itself (Plate XV., lower part); this is accomplished by placing them in a small brass tube, which is fixed in and projects beyond the nozzle of the instrument, but without in any way affecting the screw; they are each ·68 of an inch in length, ·412 of an inch in width, and ·2 of an inch in thickness, and are inclined to each other at an angle of 4¼°; this makes the angle between the bodies 9½°, and the imaginary point towards which the eyes converge nearly 15 inches.

This angle was selected as being generally the most suitable, bearing in mind the desirability of maintaining the ordinary length of body, and of giving an amount of convergence which should relieve the eyes of the strain not unfrequently experienced in the use of a binocular. In some instances where the eyes of observers are either closer together or wider apart than usual, it may be desirable to deviate from this angle in either direction, to meet the requirements of a particular case.

It is a matter of vital importance that the prisms be not too

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EXPLANATION OF PLATE XV. (Lower part).

The Plate represents an ideal section of the front of the microscope, removing half of the binocular prisms, the objective, and their fittings. The objective is shaded with oblique lines.

long in proportion to their thickness, for if this be so, the central rays, being reflected immediately on entering near the lower edges of the prisms, will strike the opposite sides and never emerge.

It is needless to say that if the central rays are thus lost, good definition is impossible.

I have given the length and thickness of my prisms as ·68 and ·2, but it is certainly undesirable that the length should exceed three and a half times the thickness.

The very unsatisfactory results produced by the Nicol's prism, when used as an analyzer in a binocular, was the primary cause of the alteration in the angle at which the bodies are inclined to the perpendicular.

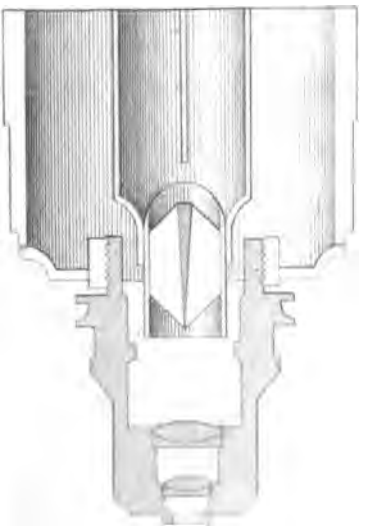
It was evident that the analyzing prism might be entirely dispensed with by substituting for the upper reflecting prisms a highly-polished and properly-worked plate of black glass, so placed that the light should be reflected from its surface at the polarizing angle of 56¾°, and that this change could be accomplished without in any way affecting the character of the instrument as an erecting binocular by altering the angle at which the bodies were inclined from 75° to 66½°. The box containing the upper prisms of the ordinary arrangement is withdrawn when polarized light is used, and the analyzing plate substituted; the result is improved definition and more light than with the ordinary Nicol.

I have spoken of the box containing the upper *prisms*, but in the original description of the instrument there was one prism only; that prism I have now divided, and the two thus formed are cemented together at such an angle that the light enters and emerges at right angles to the surfaces, care of course being taken that the reflecting surfaces remain in the same plane.

Still anxious further to improve the arrangement, I have caused a plane mirror to be prepared by Mr. Browning, and silvered by the beautiful process employed by him in the manufacture of reflecting astronomical telescopes. This, like the analyzing plate of black glass, can be immediately substituted for the upper prisms if desired. By its use we obviously get rid of the whole of the glass, and two surfaces; but, on the other hand, light is not totally reflected from silver, as it is by interior reflexion, and metal will tarnish.

I have thought it desirable to bring these alterations before the notice of the Fellows, as by the former less than one-fifth of the original quantity of glass is employed in the manufacture of the upper prisms, and powers up to an $\frac{1}{8}$th of an inch can be used with facility; whilst by the latter, in getting rid of the Nicol's analyzing prism we secure perfect illumination of both fields, better definition and more light.

The development [illegible]



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V.—*On a presumed phase of Actinophryan Life.*

By J. G. Tatem.

Plate XV. (Upper part).

Actinophrys sol is so often seen in the conjugated condition, and the progress of coalescence between two or more individuals so frequently watched, that I need not refer to the mode of its accomplishment. Nor is it necessary that I should remind the reader that much difference of opinion formerly existed as to the nature of the process; authorities having been equally divided on the subject, some regarding it as a Reproductive—a copulative act, others contending that it is due to accidental contact and partial fission only. Later observers have, however, determined the question by demonstrating the formation and expulsion of swarms of "embryonic germs" as the consequence of the union.* Whether or not these germs, so copiously diffused into the surrounding water, can ever be kept under observation, or be artificially maintained in the conditions favourable to and for the length of time necessary for their development, from embryo to maturity, and that, too, through many possible mutations of form, is altogether problematical. But may we not hope that, among the multitude of minute and scarcely-recognized monadiform beings, one or more may be detected which we may reasonably connect with the *Actinophryan* germ in its progress towards adolescence. I think so; and to one such I would wish to draw your attention.

In water in which the *Actinophrys sol* is more or less abundant, especially in the later months of autumn, certain minute organisms about $\frac{1}{1000}$ to $\frac{1}{500}$ are usually found. They are mostly of oblong but inconstant form, transparent, containing a few rather large granules having a greenish refraction, and armed posteriorly with soft-looking spines (pseudopoda), variable in length and number in individuals, and in the same individual also, from time to time (Plate XV., upper part, Figs. 1–3). Their motion is that forward oscillatory one which is effected by the vibrations of a single fine anterior filament, and is moderately quick, occasionally almost rapid. This may be long continued, but patient observation will show that, after some uncertain, wayward movements, they will gradually settle down, flatten out circularly, throw out pseudopoda, sometimes twice the diameter of the disk, and become in all respects perfect *Actinophrya sol* (Fig. 4). In this stage the true Actinophryan voracity is displayed. *Astasia margaritifera* abounded in

* *Vide* Mr. Waller's paper in 'Journal of Quekett Club,' vol. ii., p. 93; and President's communication to Reading Microscopical Society, reported in 'Mon. Micr. Jour.,' vol. iv., p. 334.

the water in which these incidents were first traced, one or more of which invariably fell victims to each Actinophryan, and in their metabolic efforts to escape, until they finally sank engulfed, presented a singularly interesting spectacle. This stationary condition is maintained for a longer or shorter time, the pseudopoda are then withdrawn, the former shapes resumed, and the onward vibratory movements renewed. I may add that they show a tendency to aggregate, and are mostly limited to a small portion of the field.

The question here arises, are we to regard these little organisms as independent forms, or as phases—and advanced ones—of Actinophryan life? I incline to the latter view; for may we not reasonably expect that, equally with creatures of a higher type of organization, the humble *Actinophrys* will fulfil those universal laws of nature which dictate that, no matter how frequent the propagation by self-division—no matter how numerous the parthenogenic descents—every living organism must sooner or later revert to the congress of two individuals, to secure the perpetuity of race by means of the fecundated germ, and that the fixed or feebly locomotive forms of animal life shall find in their actively motile embryonic developments the mode of effecting their dispersion in space.

VI.—*On the various Phenomena exhibited by the Podura Test under ordinary and extraordinary Microscopic Resolving Powers.* By G. W. Royston-Pigott, M.A., M.D., F.C.P.S., Cantab., late Fellow of St. Peter's College, M.R.C.P., F.R.A.S., M.R.I., F.R.M.S.

The variety and subtlety of this queen of test-objects under various treatment of illumination and magnified imagery strike the eye of every observer with astonishment, who examines the phenomena presented under different conditions of illumination, and different adjustments in the objectives and eye-pieces, and greater or less penetration or depth of focal vision. The microscopist with whom this precious, I would almost say, peerless object has been a prime favourite, possesses within it a complete *Thesaurus* of signs by which he can pronounce upon the performance of his glasses. In this very wealth of optical effects lies its chief value and interest. So long indeed as the object was made to show itself in one "standard" form, as the *ne plus ultra* of microscopy, and so long as both makers and purchasers of glasses conjointly decreed the standard test, and forbade, under penalty of contempt and ridicule, any other view, further advance appeared unnecessary.

The opticians were fairly credited with the most honest intentions when they constructed the glasses to show this standard and no other. They naturally desired to display microscopic power in its most approved and effective form as a *summum bonum* of art. The exclamation markings survived as a test for forty years. I am fortunate to possess an ancient scale of the Test Podura and one of Pritchard's doublets 1–80th of an inch focal length, capable of showing these veritable markings. Why the resolution of these markings into much more minute structure by the glasses of to-day, when these same markings could be seen forty years ago with a doublet, should be so much doubted, appears almost unaccountable; and offers an interesting phase in the science of microscopy. No such a thing happened when the lined objects, such as diatoms, gave way to squares and dots; more complete resolution was hailed with satisfaction as a sign of progress. Science is not without many examples of outcry against, and disbelief in, new truths.

I attach very much importance to the phenomena displayed by the Podura Test-scale for several reasons:—

1. They are a severe test for chromatic and spherical errors.

2. An objective and eye-piece which is competent to thoroughly resolve this test from its primary markings into its secondary and tertiary forms may be pronounced super-excellent.

3. The scale is eminently fitted also to display the amount of penetration of an objective, as it possesses structures which are placed in two different planes; the one nearer and the other farther from the objective.

4. The scale gives a new lesson in microscopy, teaching the observer how to deepen or regulate the planes of focal vision independently of mere focussing.

Indeed, objectives can be so adjusted and compensated as to enable the observer to view either the upper or the lower structure, or both structures at once.

5. The scale is further valuable as embracing almost every form of difficult definition: lines, points, beads or molecules, and brilliant disks of light, as well as the colours due to films of various thickness as shown in "Newton's Rings."

THE SHADOW PHENOMENA.

The standard (?) markings of forty years' pedigree may be seen with a good inch objective, or with a powerful doublet. A startling fact is here disclosed. *Only a very small angular aperture is requisite to display them.* The markings (! ! !) are therefore independent of large aperture. If we could imagine a Podura scale 1000 times larger than usual, there does not seem the smallest reason to doubt that we could see these markings with the eye alone at a

distance of ten inches, *i. e.* with an angular aperture of about *one degree.**

The club mark or note of admiration is therefore entirely different in its nature from a diatomic line-mark primarily made out by a third-rate objective. For these lines cannot possibly be defined with so small an aperture even as 15°: perhaps 30° is the limit. In proportion as the angle is enlarged, *cæteris paribus*, diatom markings come out more and more distinctly. This broad fact points to the conclusion that these Podura marks are *shadows*; they disappear with oblique light. Parallel rays perpendicular to the scale or coincident with the instrumental axis display them most clearly. A great variety of such shadows or obstructions of light may be shown.

(*a*) If a number of the *Formosum* diatoms be selected which lie across each other irregularly, a great number of black shadow-patterns are developed, easily seen with a low aperture and power, many of them very closely imitating the Podura marks or clubs. Of course where light cannot get through on account of intricate internal refractions and reflexions in a mass of brilliantly light-transmitting spherules, we get darkness or black markings.

(*b*) If we diligently search among slides rich in overlying *ribbed* scales, with a low power, we shall be rewarded with beautiful darkness-patterns in many forms; and the more nearly the ribbings of the superimposed scales are parallel to each other the longer appear the markings of the wavy group.

(*c*) Whenever minute cylinders of a light-refracting character lie athwart each other, black shadows are shown near the points of each intersection: with splendid definition these black shadows can be discerned even in the plume-hairs of the gnat at their points of intersection. Spun glass shows them in the highest perfection.

I regard, therefore, the black marks on the Podura seen with a low power and small angular aperture as simply jet-black shadows from the stoppage of light-transmission.

Some allusion must now be made to the phenomena approximately exhibited by minute transparent cylinders intersecting each other.

Sir David Brewster improvised fairly good lenses by placing two small cylindrical bottles filled with water at right angles to each other; their intersection formed a lens which produces a fair image of a distant object. In October, 1870, Mr. Moginie procured for me a number of fine glass rods; when these are placed in contact in one plane, and another similar set is also placed behind them at right angles, brilliant images of a distant flame are beautifully dis-

* In other words the base of a triangle, with the pupil of the eye for base and a point 10 inches distance for the apex, would subtend nearly one degree.

played, and as the angle at which the glass rods intersect is changed, distorted images are formed of great variety.*

Now when a scale is composed of *ribbing* approximately cylindrical, and when there is a double set, the one crossing the other, as in the Lepisma, an approximate definition shows a bead-like appearance at the points of intersection, as noticed long ago by Mr. Beck: these *quasi* beads are limited to the points of intersection, and are very sparse and few. A more profound and *recherché* definition dissipates these false beads and displays a surprising wealth of beading composing the entirety of each Lepisma ribbing in both sets (described by the writer in 1869).

Molecular Definition.—Whenever molecules, or spherules, or beads are arranged in straight linear rows and in close contact, whether in diatoms or scales (before the glasses receive those exquisite corrections necessary to disintegrate the cylindrical appearance into its component beading), their primary appearance under an approximate definition is always cylindricular, that is formed of *cylindricles.*

I may remark that some of the intersections on Lepisma near the edge closely simulate Podura markings; and the elongated beads described by Mr. R. Beck in this Journal, resolve themselves into straight clusters of minute beading; similar to those of the resolved *Podura clubs or markings.*

The molecular definition of these bodies is clearly dependent upon the definition of each molecule. The very delicate line of darkness formed by the points of contact between two adjacent molecules, and the tracery-shading produced by their curvature, offer some of the most severe trials of the delicacy of development possessed both by the eye and the instrument. Here we are landed as it were in a fairy shadow land. Too much light overwhelms the eye and obscures these delicate forms. The least practicable and a bluish light is the best. Sir W. Herschel recommended a quarter of an hour's absolute darkness before submitting the eye to view the faintest objects in the heavens: this rule is too much violated by the microscopic student. A dark room, a shaded eye, calmness, and above all, absence of prejudice, are essentials to microscopic research, not always attainable.

Mr. Slack's Silica Beads.—A new form of test is here given. On some of his slides isolated silica spherules are copiously visible. Singly observed, there is a bright halo round them with the best glasses; when two happen to touch, their distinct individuality is obscurely defined. If they were as small as Podura molecules or beads, and placed accurately in a straight row or rouleau, their reso-

* I left these with Mr. Curteis, who kindly fitted them up, and Mr. Hennah, of Brighton, subsequently photographed some interesting effects, which he did me the honour to present to me.

lution would be primarily cylindrical, secondarily molecular: as a third and higher order of excellence the bright band of the secondary spectrum would diminish in breadth, and each spherule would present under direct illumination a focal point of light.

Mr. Slack's Silica Films.—No more instructive object has been seen than these slides. First of all, we know what we are looking at and what we ought to see. When our Secretary, with the humour he so richly possesses, suddenly asked me at the Society's room what I saw on the microscopic stage, through an old Ross' $\frac{1}{4}$, I at once said, "Some cylindrical fibre, but what, I could not tell!" *He then informed me they were cracks in a film of silica*; yet they seemed to me absolutely cylindrical fibres. I have noticed the same appearance in Nobert's lines. Here we see what the microscope may misrepresent. The interval between the edges of the two separated films was filled up by a phantom-cylinder! Two square edges were thus transfigured. *It is evident that there ought to have appeared two black lines*, with more or less prismatic colour resulting from the usual refractions at right-angled prisms. This experiment is exactly the reverse of the former. There were no beadings, but cylinders conjured from square glass or silica edges!

When the definition of these edges was accomplished in the most perfect manner obtainable, two black edges with a secondary spectrum were distinctly observed with Powell and Lealand's best immersion $\frac{1}{8}$th and a variety of other glasses.

There is one other cause of colour observable. Let us suppose we have resolved the scale *Podura macrotoma* into ribs, and then the ribs into parallel rows of bluish and reddish beads: now rotate the scale slowly, and these colours disappear. Some are of opinion that these colours are due to diffraction, others to polarization of thin films. However that be, these double beads are generally invisible in perfection, except when showing nearly complementary colours.*

Molecular Ribbing and their Intersections.

Advancing now in the scale of difficulty and double intersecting rouleaus of beads crossing at various angles in contiguous planes, above and below, we get we will suppose:—

Primary Definition.—At the exact places where these rouleaus cross there is at first sight an obliteration. Where two spherules lie exactly the one over the other, a point of bright light is developed. In this way the Podura scale may be made to appear studded over

* Our late highly-esteemed President brought me this specimen to resolve for him; the charming delight which he displayed on seeing their complete resolution with a half-inch objective made a lasting impression of his unaffected amiability and candour. I subsequently had the honour of showing them to Dr. Lawson with an inch objective.

with bright points distant from each other about the length of eight or ten beads, according to the nature of the intersections.

With my Powell and Lealand's 1–16th immersion I develop alternate obliteration, and two short black lines intensely black, and between them, beading. In fortunate cases both our old friends, the clubs or *shillelaghs*, as well as two black lines and two different sets of beads besides, are visible.

I have never been able to see these double short black lines in any scales but Poduræ.

Subsidiary Forms presented by the Podura Test.

I. The lowest and worst form of development is the appearance of a few isolated beads at some of the intersections of the molecular ribbing. This can be readily produced by entirely deranging the corrections of a superb objective.

II. Another imperfect form of definition is a bulbous swelling, like a knot in a vein. A varicose swelling in surgery is an irregular bulging of the blood-vessel at irregular intervals. A few large and smaller bullets forced into an india-rubber tube give the idea of varicose swellings. But notwithstanding the frequent manner in which my distinguished friend, Colonel Woodward, has applied the term varicose as representing the appearance of the Podura ribbing, this description seems to me hardly to apply to an apparent ribbiness wholly composed of distinct beads closely packed together and separable into individual molecules. In the varicose idea there is continuity, in the beaded form there is juxtaposition, like spheres packed closely in an elastic and transparent tube, or like a necklace.

III. *A rippling undulation of beaded ribs*, as though a series of necklaces were placed parallel upon the small ripples often seen after the tide is out upon a sandy beach. This form appears by a change in the cover correction.

In observing Podura with the water lens and a cracked cover, the fluid is seen to insinuate itself in straight lines, precisely following the course of the beaded ribs; bead after bead disappears and reappears under a dry lens, as the fluid gradually dries up. In some cases I have observed pressure of the object-glass upon the slide (delicately administered) to cause the escape of an oily fluid which returns to the scale backwards and forwards as the pressure is changed by the fine focussing adjustment, and the course of this oil most clearly demonstrates the existence of *level* and not undulating beaded ribbing.

IV. The appearance of continuous ribs is the primary form of development. Colonel Woodward, with that acumen and perspicuity which so highly distinguish his communications, informs us that he has observed this valuable fact (long known to myself) that the

beaded ribs lie altogether above the exclamation marks or clubs. "The varicose ribbings are first seen, and the exclamation marks do not become distinct till after the scale is focussed through." The ribbing or streak is described by Mr. Beck in his work on the microscope, and declared by him also to be out of focus, a drawing of which is given (Plate VII., Fig. 4).* It was not then suspected that the true structure of the scale was put out of court, as it were, and that to see the "markings" distinctly we were obliged to focus absolutely through the structures, justifying the Colonel's words, "these exclamation marks are probably illusive or spurious appearances, notwithstanding the great distinctness they attain under favourable conditions." In confirmation of this opinion I may be permitted to state that these markings cannot be finely seen unless the objective is *spherically under-corrected.*

V. If one examines with a pocket-lens the several sets of photographs, so beautifully done by the accomplished Colonel, and which he has most handsomely placed at my disposal for reproduction, we find numerous instances in the Podura Test-scale of beautifully-formed spherical beads in close juxtaposition. It is difficult to imagine that different laws should govern the structure of the same scale. If it is distinctly beaded in one part, one can scarcely with propriety say it is merely varicose in another.

Postscript.

Now that the sun is low in a wintry horizon and easily available as a *direct illuminator*, I recommend, with great pleasure, to the inquiring majority of our Fellows the following *crucial experiment* on the Podura. Select a sunny aspect about 10.30 A.M. (with the sun about S.E.); arrange a Podura scale, the best you have, with its quill towards the S.E.; then incline your microscope gradually till the sun's uninterrupted rays impinge less and less obliquely upon the under surface of the slide. You will find a position, according to the angular aperture of your objective, when the scale becomes illuminated on *a dark field.* At that instant you will behold once for all, if your glass be of first-rate quality, the whole scale studded universally with a molecular structure hitherto unsuspected; but revealing at a glance the poverty of old-fashioned definition.†

* "The two appearances (Figs. 4 and 5) on one and the other side of the focus when the adjustment of the object-glass is incorrect." The very accurate drawing given unconsciously by Mr. Beck of the Podura ribbing is truly remarkable. The exclamation markings are seen in a different plane, and therefore out of focus. Figs. 4 and 5, in Mr. Beck's Plate VII., are well worth attention.

† As a first essay the young microscopist may be recommended to experiment at first upon the largely-beaded scales, as *Podura macrotoma* and *Podura degeeria.*

PROGRESS OF MICROSCOPICAL SCIENCE.

Fungous Growths in Mollusk Shells.—In a paper read before the Manchester Philosophical Society on the 26th of February, Mr. Mark Stirrup exhibited sections of shells of mollusca, showing so-called fungoid growths. He referred to Dr. Carpenter's report on shell structure, presented to the meeting of the British Association in 1844, in which especial mention is made of a tubular structure in certain shells, and he cites the *Anomia* as a characteristic example. In the last edition of 'The Microscope,' Dr. Carpenter, he said, withdraws his former explanation of this structure, and now refers it to the parasitic action of a fungus. Mr. Stirrup showed sections of this shell penetrated by tubuli from the outer to the inner layers of the shell, and it is upon the inner layer that the curious appearance of sporangia, with slightly-branched filamentous processes proceeding from them, present themselves. The parasitic view is strengthened by the fact that these markings are not found in all parts of the shell, and are certainly accidental. Professor Kölliker maintains the fungoid nature of these tubuli in shells as well as in other hard tissues of animals, as fish scales, &c. Wedl, another investigator, considers the tubuli in *all* bivalves as produced by vegetable parasites, and that no other interpretation can be given. This view does not seem to be borne out by the section of another shell which was exhibited, "*Arca navicula*," in which the tubuli are always present forming an integrant part; they are disposed in a straight and tolerably regular manner between the ridges of the shell; moreover, they have neither the irregularly-branched structure nor the sporangia.

A Conspectus of the Diatomaceæ.—The 'Lens,' which is the quarterly journal containing the Transactions of the State Microscopical Society of Illinois, has at length, after long delays, caused of course by the terrific fire at Chicago, made its appearance. We must congratulate the Editor and Publishing Committee upon the fact. Of course it was difficult to get out a first number in a place so grievously injured by fire as Chicago; still it is a very good number, and we have no doubt that its successors will surpass it in excellence. The most important paper is one taken from the 'Monthly Microscopical Journal,' and it too is the only paper which possesses a plate, also taken from the 'Monthly Microscopical Journal.' Still there are some good essays, and among them is that whose title heads this paragraph. In this the author, Professor H. L. Smith, of Hobart College, N.Y., attempts to give a synopsis of the names of every genus of the Diatomaceæ, and the arrangement is apparently excellent. The explanation of the terms in use is a good one. It seems to us though that the author would have done better had he admitted fully the appearance of striæ as only an apparent structure that microscopic observation tended to show was merely a linear arrangement of dots or hemispheres.

The 'American Naturalist' on the dispute between Mr. Wenham and Mr. Bicknell, which was conducted in the pages of the 'Monthly Microscopical Journal,' is somewhat severe on Mr. Wenham. The writer, who signs himself C. S., and who is therefore easily recognized, says, that for some three or four years some American microscopists have been calling attention to the deception commonly practised by most working opticians in calling the "'power of their instrument less than it really is—*i.e.* calling an objective a quarter-inch when its focus is really but one-fifth or one-sixth of an inch, or an eighth when actually one-ninth or one-tenth,—and some now approach to one-twelfth.' In the 'Monthly Microscopical Journal' for December, 1871, Mr. F. H. Wenham writes a paper in reply to one of Mr. E. Bicknell's on this subject, in which he takes Mr. Bicknell to task for exposing the deception,—and admits the truth of the charge. Here we have a gentleman, well known throughout the microscopical world as one of the most accomplished *theoretic opticians* of London, generally supposed to be the principal adviser of the working opticians, not apologizing for, but practically defending the imposition, one that has been exposed and complained of by Dr. Wm. B. Carpenter.' Mr. Wenham says 'a scientific microscopist gives the diameters with his illustrations and the nominal power of his object-glass; this quite meets the case.' In this Mr. Wenham is entirely wrong; it does not meet the case. A power of one thousand diameters obtained with a one-inch objective is a very different thing from one thousand diameters obtained with a one-tenth, *unless the one-inch is ten times as good an instrument as the one-tenth.* The scientific microscopist should give with his illustrations, not only the amplification he employed, but the real focus of the objective, and the name of the maker, as astronomers do in the case of their telescopic observations. He further says, 'In such a difficult and complex arrangement as a high-power object-glass, it is almost impossible for all the makers to work to the same magnifying standard.' That of course depends on the knowledge of optics possessed by the workman, but has nothing to do with the matter. When the object-glass *is made*, the focus can be measured, and the glass named accordingly. The nearer the actual power comes to that intended, so much the more credit to the maker; the further it is from what he sells it for, the more to his discredit. It is an axiom in microscopy that the lower the power of a glass that will give a certain result or effect, the better the glass. Mr. Wenham's comparison with the steam-engine is as inappropriate as Hartnack's objection to English microscopes, that with their wheels and screws they look like a steam-engine."—*American Naturalist*, February.

The Muscular Fibres of the Small Bronchi.—On this subject Professor Rindfleisch, in the 'Centralblatt' of February 3rd, makes the following remarks, which are thus briefly abstracted by a contemporary:—1. The smallest bronchi have a special and distinct layer of transversely-disposed muscular fibres, which, in the places where the former debouch into infundibula, are specialized into a sphincter; they are capable of considerable dilatation, and have under their epithelial layer a finely-meshed network of capillaries,

like those of the lungs. 2. The circular fibrous tracts of the smaller bronchi send loop-like processes into the mouths of the infundibula, which penetrate to the fundus of the same. At from two to four points, tracts of smooth muscular fibres surround the infundibula in an annular form; these muscular rings lie mostly in the inwardly-directed portions of the alveolar septa. 3. All these muscles are in the so-called brown consolidation of the lung, in a condition of hyperplasia; those who have seen them in morbid specimens will be able to recognize them, though they are very delicate, in normal lungs.

Nemosoma elongata.—Mr. Joseph Sidebotham, F.R.A.S., read a paper on this species at a recent meeting of the Manchester Phil. Society. The author having discovered a considerable number of specimens of this very rare species under bark of elm, at Beeston, Notts, in November last, and having the opportunity, carefully observed its habits, of which he gave a detailed account, illustrated by specimens and by portions of bark and diagrams; showing also specimens and drawings of *Hylesinus vittatus*, on which it is parasitic.

The Diatom Hoax.—Our readers may remember our referring to an article some time ago, in which the most monstrous microscopic absurdities were broached. We certainly did not consider its matter to be at all amusing. However, the 'American Naturalist' of February considers that it was a hoax. Speaking of it, it says, many readers have enjoyed, in a late medical journal, the ingenious essay on test-objects, in which the new immersion one-seventieth of 191^{c}, wet with fluoric acid and illuminated by a new eccentric parallelopiped with fluorescent rays exclusively, is represented as revealing that the structure of *Pleurosigma angulatum* is like the Nicholson pavement; and that a new diatom, fortunately rare, has beads more than one hundred and forty-seven millions to the inch, which are invisible by all other lenses and to all other observers. They will be further amused by learning from the 'Boston Journal of Chemistry' that some foreign medical journals have seriously reviewed this burlesque and discovered it to be a hoax.

Absorption of Insoluble Matter by Animal Membranes.—This subject, which cannot be worked out without the help of the microscope, has lately had a series of experiments made upon it by Dr. Auspitz, of Vienna, who has arrived at the following conclusions on the subject:—1. That, in mammals, insoluble matter (starch-flour granules), starting from the peritoneum and subcutaneous tissue, is able to reach the lungs, and through these organs to enter the general circulation. 2. That these granules, in order to go over into the veins, pass through the lymphatic system. (That they are taken up exclusively in this way, is not as yet proved.) 3. That the epidermis always presents a considerable, though only relative and not absolute, obstruction to the absorption from the integumentary surface. 4. That the absorption is essentially promoted by the mediation of fat, which goes over into the circulation in the same manner as starch-flour, though even more easily. Finally, the supposition may be offered, even if the direct

proof is provisionally deficient, that all that is true of starch-flour, and in a higher degree of fat, may also be asserted of other insoluble bodies of finer division and, therefore, less permanence of form than the starch-flour. The supposition is not in any way contradicted by the discoveries of Auspitz, made in connection with his well-known inunction experiments with mercury.

The Examination of Sponges under the Microscope.—In a paper published last year in the 'Annals of Natural History,' and which is remarkable, as it is the paper in which Mr. Carter, F.R.S., demonstrates the true form of the sponge-cell (on which we published Prof. James-Clark's paper in our last number), the author gives some hints which may be useful to our readers. He says that, for the most part, all marine sponges (save the *Clionidæ*, which may be in deciduous shells) begin to perish within forty-eight hours after they have been taken from their natural habitat, although their attachment to the piece of rock on which they may be growing remains uninjured; and even if they survive a little this period, they are voraciously devoured by the crustaceans which may be confined with them—just as in all similar and serial microscopical inquiries, whether free or confined, the minute crustaceans are thus the most defeating agents. With the putridity or dissolution of the sponge comes a development of infusoria; and if, under such circumstances, *one* Vibrio is seen to pass across the field, the microscopist may as well give up all further research into the phenomena of the *living* sponge. On the other hand, if the seed-like body be taken from a living piece of *Spongilla* and placed in a watch-glass with water, it may be kept under a quarter-of-an-inch compound power until the young *Spongilla* issuing from it has gone through all its phases of development from its first appearance to its full completion, which may be seen both elementarily and collectively; while during this time, having a plurality of seed-like bodies growing in different watch-glasses, the experiment of feeding the young *Spongilla* with carmine or indigo, which soon points out, by its colour, the position and grouping of the sponge-cells, together with the passage of the particles in through the pores of the dermal sarcode, thence to the ampullaceous sacs, and then the discharge of the ingesta through the excretory canal system—all may be deliberately watched under the same microscopic power, with so little difficulty and yet so accurately, that there is no merit whatever in recording observations of the whole process. It was in this way that Mr. Carter obtained the data published in his paper "On the Ultimate Structure of *Spongilla*," confirmed by similar observations on large pieces of *Spongilla* taken directly from the tank.

The Centripetal Fibres of the Spinal Chord have been recently submitted to some experiments by Dr. Dittmar. Rabbits under the influence of woorara were the animals experimented on, and the experiments which he carried out led him to believe that it is certain that there is a system of fibres in the substance of the spinal chord which, though they do not belong to the nerve roots, are capable of responding to the action of direct stimuli, and can transmit the impulses thus generated along the whole length of the spinal chord

up to the medulla oblongata, where they undergo reflexion into motor nerves.

The Vienna Syphilis Corpuscles.—A medical journal, quoting from the 'Allg. Wiener Med. Zeit.' of February 6th, says that the latter contains a serio-comic article on this discovery of Lostorfer, which, for a few moments, shook such men as Stricker, Hebra, and Skoda off their balance. Dr. Wedl, the author of the article, says:—"These corpuscles will, at the next meeting of the Medical Society, be solemnly buried in Dr. Stricker's museum. The sympathy of the profession is requested under these painful circumstances, and the Society will doubtless institute special masses for the repose of the departed. It is very lucky that the members did not, in their hurry, have medals struck for the discovery; and there is time left to send counter orders to Paris and London, to stop enthusiastic researches which might bring some blame on the Vienna Medical Society. The latter may derive from this mishap the lesson to beware of allowing itself to be made a trumpet to ephemeral discoveries." The 'Lancet,' however, finds it difficult to believe—and we quite coincide with it—that observers like Stricker and Hebra would have been carried away by imperfect experiments. It is clearly stated that different kinds of blood were placed under Dr. Lostorfer's microscope (he not knowing whence the blood came), and he constantly recognized his peculiar corpuscles in blood coming from patients affected with syphilis. Time will show who is right.

Alternation of Generations in Fungi, taking place in different Plants.—This phenomenon, though asserted to occur by Professor Oersted, is disbelieved rather by Mr. Cooke. He believes it takes place in the same plant as in the case of *Bunt*, but he feels great difficulty in believing in this process, where the generations were passed in different plants, until confirmed by other observers. If the spores of *Æcidium Berberidis* were taken from the Barberry and sown upon young wheat plants, and all these plants became infected with corn mildew (*Puccinia graminis*), to which wheat is but too prone, it certainly seemed premature to say that the spores of the *Æcidium* caused the *Puccinia* to be developed as a second generation; whereas it is much more probable that the germs of the mildew already lay dormant in the wheat, and, at most, the sowing and growing of the *Æcidium* spores only stimulated the mildew to a more rapid development. He said, at one of the meetings of the Quekett Club, that he certainly thought such a theory more probable, and quite as sound as the other.

Hydra carnea in Lake Superior, U.S.—The United States' Lake Survey Report has not yet been issued, but a portion of the zoological records have been published in 'Silliman's Journal.' From it (by S. I. Smith and A. E. Verrill) we learn that this beautiful *Hydra* (agreeing with Ayer's description of this species) was very abundant at the eastern end of St. Ignace, upon rocks along the shore and near the surface, frequently completely covering quite large surfaces where they were protected from the direct sunlight, and was

also brought up in many of the dredgings from 8 to 148 fathoms. In 32 fathoms, Neepigon Bay, and in 59 fathoms, off Simmon's Harbour, it was brought up in abundance from a soft clayey bottom. In the deep dredgings, it frequently came up near the bottom of the clay in the dredge, and was evidently not caught while the dredge was near the surface.

Transmutation of Form among Protozoa.—Professor A. M. Edwards, of America, has published some curious notes on this subject, which we shall probably reproduce in full in an early number. For instance, he records (as have some of our own microscopists also) the passage of an *Amœba* into a *Kolpoda* or *Paramecium*. Hence he considers the *Amœba* the young of *Paramecium*. This paper contains other matters of importance, which we must defer noticing at present.

Foraminifera of the English Chalk.—Dr. W. K. Parker, F.R.S., our President, and Prof. T. Rupert Jones, have published, in the 'Geological Magazine' for March, a paper on the Rev. Henry Eley's work on the above. The paper is little more than an emended notice of Mr. Eley's work, so that those who have not his work may profit by his views. It is well worth referring to by those who are interested in this particular branch.

NOTES AND MEMORANDA.

Microscopical Diagrams.—A large lot of these, 180 in number, size about 22 × 16 inches, are about to be sold in America; it may interest some of our readers to know of it. They are finely executed by a well-known English microscopist, and were prepared to illustrate a series of lectures on Fungi, Foraminifera, Polycistins, Seeds, Infusoria, &c., &c., &c. They are mostly painted on tinted paper in water colours, so as to give good contrast and be readily seen. Many of them represent unique specimens observed by the author, and are of the greatest value to the student of the lower forms of animal and vegetable life. Address, Naturalists' Agency, Salem, Mass., U.S. of America.

Changes of Current in Lakes affect Distribution of Diatomaceæ, &c.—The 'Lens,' the first number of the American Microscopical Journal which has been promised so long, contains an account of a paper by Mr. Babcock, "On Chicago Hydrant Water," showing that shortly after the course of Chicago river was reversed by the deepening of the Michigan and Illinois Canal, there was a marked diminution in the amount of organic matter in the hydrant water. He accounted for the change on the theory that the southerly lake current flowing along the western shore had brought to the crib the lighter vegetable products of the northern streams, and that the diversion of the river channel, by causing this current, opposite the city, to swerve slightly

to the west, had admitted to the crib the purer water from the deeper portion of the lake. He based this theory mainly upon the fact that while the hydrant water had never contained any of the stipitate diatoms growing in abundance along the shore, the free unattached forms were always present, together with several species whose origin had not previously been determined, but which he had found in abundance in several of the more northern streams.

Health of Professor Huxley.—Our readers will be glad to learn that Professor Bence Jones, the medical adviser of the Professor, asserts that the statement that the Professor has shown a tendency to phthisical disease is quite unfounded in fact. Mr. Huxley has never at any time exhibited any disposition to lung mischief. He is suffering from overwork only, and there is every reason to expect that he will soon return to England in good health. He gave up his place at the School Board in order to spare himself on his return.

How to make Sections of Small Fragments of Soft Tissues.—The following facts are stated by Dr. Woodward in the new number of the 'Lens'; most of them are, however, familiar to English workers. Where the fragment is too small to be conveniently held between the finger and thumb of the left hand while the knife is guided by the right, it may advantageously be imbedded in some suitable substance. In the German laboratories a mixture of wax and oil is extensively used for this purpose, and has been highly lauded. At the Army Medical Museum we have long since learned to give the preference to paraffine, as more readily handled, more cleanly, and yielding altogether better results with less trouble. A sufficient quantity of paraffine having been melted over a spirit-lamp in a small porcelain capsule, or any suitable vessel, is poured into a mould made by folding a piece of ordinary writing-paper into any convenient form, and the fragment of tissue is held in the middle of the fluid with a pair of forceps until the cooling paraffine is hard enough to retain it in place. It is then left to itself until the paraffine is quite cold, when the paper is unfolded, and sections can readily be made by cutting through the paraffine in any desired direction. The sections as made are dropped into alcohol, in which the tissue is readily disengaged from the paraffine by a pair of small forceps.

Collins's Light Corrector.—A very useful piece of apparatus was exhibited by Mr. Collins (17, Great Portland Street) at the Quekett soirée, consisting of a brass stage-plate with a groove in which rotates a diaphragm of four apertures, one of them being open and the other containing blue glasses of special tint, and one with a finely-ground surface. These effectually correct the yellowness of all artificial illumination, making the light soft and agreeable to the eyesight as well as improving the definition. It is in fact an improvement on Rainey's Light Modifier so as to obtain more varied effects, and does not require any special fitting, as it can be used on any microscope.

CORRESPONDENCE.

Mr. Cundel's anxiety for the Credit of this Journal and Sir David Brewster's Scientific Reputation.

To the Editor of the 'Monthly Microscopical Journal.'

Mr. Editor,

Sir,—Whilst admitting the intended courtesy of Mr. Cundel's letter upon some remarks which I had the honour of addressing on the 3rd January last to the President of the Royal Microscopical Society, in which he appears to call me to account "for the credit of the 'Microscopical Journal,' and in justice to the scientific character of the late Sir David Brewster;" I may be allowed to reply, first of all, that it may be safely assumed that the credit of this Journal is quite secure under the protection of its present Editor; and, secondly, that no puny efforts of mine could possibly injure the world-wide reputation of Sir David, of whose writings I am an admiring student.

Now, Sir, though Mr. Cundel complains thus of my statements (rather curtly reported), I hope he is not a photographer, lest Sir David's own words should further on meet his unqualified disapproval. If Mr. Cundel would have given himself time and opportunity for reading Sir David's 'Optics,' he would have saved me the trouble of encroaching on your valuable space.

Sir David excited the wrath of photographers in general by his unsparing criticisms bestowed upon wide-angled lenses, and I shall make a few extracts from his writings to illustrate this point, in deference to Mr. Cundel's wish. Amongst other things Sir David says:—

"As the pupil of the human eye is little more than two-tenths of an inch in diameter, we may regard the picture on the retina as a correct representation of external objects, in so far at least as its correctness depends upon the size of the lens which forms the picture . . ."

"Let us suppose that a lens *four* inches square is employed to produce upon a plane surface the image of any object, and that the size of the pupil of the eye is two-tenths of an inch; then, as there will be several hundred areas equal to that of the pupil in the lens, the image given by the lens will be a compound image consisting of several hundred perspective views of the object taken from several hundred different points of sight, each distant two-tenths of an inch from its neighbour, and all those on the margin of the lens distant three inches and eight-tenths from those opposite to them. *Such a jumble* of images cannot under any circumstances be a true representation of the object."

"Let us now apply these results to the photographical pictures of the human bust as taken in a camera. The human face and head consist superficially of various surfaces, some vertical, some horizontal, and many inclined at all angles to the axis of the lens by which they are represented on a plane surface. . . . .

"The right-hand marginal part of the lens may introduce into the portrait the left eye, or the left ear, or the left side of the nose: . . . . the lower part of the nose, the interior of the nostrils, the lower part of the upper lip, and the lower part of the chin will be introduced by the lower marginal parts of the lens; while the top of the head, the upper parts of the lip and the eyelids will be introduced by the upper marginal parts of the lens. . . . .

"*A monstrous* portrait *of the human bust is thus obtained by the photographer, the monstrosity increasing with the size of the lens.* The nature and character of the portrait will thus vary with the superficial form of the lens, which may be circular, oval, square, rectangular, triangular, or of any irregular form; and in this way remarkable modifications of photographic portraits may be produced merely by varying the shape of the (exposed surface of) the lens. . . . The hideousness of photographic portraits is universally admitted . . . . the true cause modified doubtless by others, is the size of the lens, even if the lens is optically perfect." (Page 70.)

"The photographer, therefore, who has a genuine interest in his art will receive these truths with gratitude; and by accelerating the photographic process with the aid of more sensitive materials, he will be able to make use of lenses of very small aperture, and thus place his art in a higher position than that which it has yet attained. The photographer, on the contrary, whose sordid interests bribe him to forswear even the truths of science, will continue to deform the youth and beauty that may in ignorance repair to his studio, adding scowls and wrinkles to the noble forms of manhood, and giving to fresh and vigorous age the aspects of departing or departed life."

The next part of Mr. Cundel's letter, Sir, refers to the value of *small* aperture in microscopic object-glasses.

I wish to say here that Sir David Brewster's opinion upon this point has been quite as decidedly expressed elsewhere. It is when an object is in relief, presenting appreciable depth as well as breadth, that distortion may so illusively occur in wide-angled objectives. Dr. Carpenter has well shown this in his observation of the effect of a wide-angled objective in binoculars causing *spherical foraminiferæ* to appear standing up *egg-shaped.* Sir David expressed himself with his usual eloquence and decision on this very point, and he applies the principles of truthful photography to illustrate these effects.

"It has been demonstrated," says he, "that all objects in relief are misrepresented by large lenses when their pictures are taken in the camera obscura. The human face divine is caricatured. Parts invisible are displayed, and parts visible are deformed. When Polyphemus admired Galatea she was not the beauty who fascinated Acis: and we think a national reward should be offered to the daring Ulysses who should extinguish the orb of every photographic Polyphemus in the land. But if the photographic lens thus deforms youth and beauty and age, and even trespasses on inanimate nature, what may we not expect from the Cyclopean eye of a twelfth-of-an-inch object-glass viewing microscopic objects in relief several thousand times less in diameter, and so near, it would see nearly the whole surface, were it a sphere? For the purpose of illustration, we may suppose the micro-

scopic object to be the head, in relief, of the Venus de Medicis, on a much smaller scale than the beautiful microscopic portraits of Mr. Dancer, of Manchester, and that the microscopical observer is requested to make a drawing of it. We cannot venture to say what would be its expression, but we are sure that it could have no resemblance to the original, both ears being fully brought out and almost the whole round of the head. In like manner every ridge in a microscopic object will show to the observer both its perpendicular sides as well as the side opposite the eye, and the resulting picture will be a coincident combination of a thousand different pictures, as seen from every point of the object-glass. The reason is therefore obvious why a large aperture shows lines that are invisible with a small aperture. The relief of a bust, or of a relievo either *basso* or *alto*, is best seen when we look at it in profile. Its height is then actually seen, whereas it is merely inferred when we look it full in the face. When the *raised* lines of test-objects are fully illuminated only obliquely, they are seen obliquely, and consequently much better than with a small aperture, which may not show them at all; not because the object-glass is inferior in *penetrating* * power, but because the thing looked at in one case is not the thing looked at in the other, and is actually a smaller object."

Hence the perfection of a microscope consists in its having the "smallest angular aperture consistent with distinct vision. Such a microscope will not show certain objects of great minuteness, but it will give a perfect representation of what it does show. The large angular aperture will show the same objects and others more minute, but whatever it does show will be a mockery of the truth."

Mr. Cundel will find further remarks by Sir David Brewster on the necessity of small apertures for truthful portraits, in the earlier reports of the papers of the British Association.

I am, Sir, your obedient servant,

G. W. Royston-Pigott.

Does Cliona Burrow?

To the Editor of the 'Monthly Microscopical Journal.'

Sir,—In your last number you were good enough to notice a paper of mine published in the 'Transactions of the Devonshire Association for the Advancement of Science, Literature, and Art,' 1871, "On the Boring of Mollusca, Annelids, and Sponges, into Wood, Rocks, and Shells."

At the conclusion of your remarks you say, "unfortunately for Mr. Parfitt," in allusion to a paper published by Mr. Waller in the 'Journal of the Quekett Microscopical Club.' Now, Sir, this exclamation of yours rather alarmed me; I thought I had either fallen into a tremendous error, or that Mr. Waller had discovered the whole

* This in modern language has been converted into resolving power, whilst penetration refers to depth of focus.—G. W. R. P.

problem of this mysterious boring question. I at once wrote off to a friend in London for the 'Journal of the Quekett Microscopical Club.' The return of post brought me the wished-for paper. I sat down at once and read it, and, Sir, what do you think my exclamation was when I had finished? why, like Charles Mathews in his celebrated character in "Used Up": he is supposed to have ascended Mount Vesuvius, and when asked what he thought of it, he simply said "*there was nothing in it*," and certainly there was nothing new in it. I cannot see that Mr. Waller has brought forward one single scrap of evidence to bear out his assertion that the Cliona does not make the excavations in which it dwells; he says that the holes are made by annelids; in some cases he is right, annelids do occasionally cross the path of a sponge, and the sponge takes advantage of the hole bored, or, as I have stated in my paper, "eroded."

When I was writing that part of my "Catalogue Fauna of Devon" containing the Spongiadæ, some hundreds of specimens passed through my hands, and each of these had to be critically examined. I consequently collected a mass of materials, and those bearing on the "boring" problem I used last year in the paper referred to. Mr. Waller says "we cannot possibly imagine a structure so feeble capable even of conveying the power of excavating at all without an entire subversion of the mechanical law, *viz. that an effect cannot be greater than its cause.* The feeble, simple character of this organism seems indeed to give us reason of its seeking for protection in holes and corners from external attacks."

We will place by the side of this two or three vegetable organisms, which I think will be admitted by all who know them that they are equally as feeble, if not more so, than this Clione in dissolving or burrowing into shell and rocks. The first, then, is *Verrucaria sublittoralis*, Leighton, which, curious enough, selects one or two species of acorn-shells for its habitation, the shells of which are sometimes very thickly perforated by it; and when the perithecia have fallen out of these holes, which they do when they get dry, and are sometimes washed out by the waves, and one seeing the shells then would declare they had been bored by some annelid. Pultney, and afterwards Montagu, both called one of these acorn-shells *Balanus punctatus*, thinking it peculiar to the species. The perithecia have the power of dissolving the shell and almost burying themselves in it.

Again, *Verrucaria calciseda*, D.C., has the power of sinking or burying its perithecia in the otherwise solid rock. And my friend Mr. H. J. Carter, in 'Ann. Nat. Hist.,' vol. v., 4th series, p. 79, says, in discussing Grayella, Osculina, and Cliona, "Undoubtedly, too, if the almost liquid mysogasters can work their way through hard wood to the surface; if the like delicate endophytes *Chytridium*, Pythium, &c., can pierce the horn-like coverings of Algæ, and the soft cell of *Zygnema* can dissolve its prison walls for exit and conjugation, the amœboid sponge can burrow amongst the layers of an oyster-shell for its subsistence." I think this is sufficient evidence to prove that equally *feeble* organisms have quite as great powers of working as is attributed to the industrious annelids, and the supposed indolent Clionas.

Mr. Waller endorses Dr. Bowerbank's view in reducing all the burrowing species of sponges enumerated by Mr. Hancock to the one *Cliona celata*. This statement is quite sufficient to convince me that Mr. W. has never seen another not uncommon species with similar habits to *C. celata*, namely, *Clione Northumbrica*, Hancock. This species is as distinct as any two species of a genus in all our British sponges, and I have no doubt but that Dr. Bowerbank will establish this as a species in his forthcoming volume on the additional species to his former volume on the British Spongiadæ.* There is, as I have stated in my paper, a very great similarity in the erosive workings of both annelids and sponges. The same may be said of the action of an acid on shell or limestone; and although I cannot prove by any test that these creatures use an acid for eroding their burrows, I feel convinced that such is the case. Take, for instance, the habit of *Halichondria suberia* and *H. ficus;* these species frequently invest the shell on which they attach themselves, and when this is the case, the shell becomes entirely dissolved, leaving its form entire in the inside of the sponge. The mere finding of an oyster-shell perforated full of minute holes, as stated by Mr. Waller, is not sufficient evidence, unsupported by facts, to convince anyone knowing anything of this subject.

Still pursuing this subject, I have added since I published my paper last year another fact in relation to the burrowing annelids.

In tracing the burrow of an annelid in an old shell of *Buccinum undatum* cast ashore at Exmouth, the burrow, I may say, was quite recent; it follows the direction of the whorls of the shell, keeping near the sutures; the hole is more or less irregularly excavated in a lateral direction, a transverse section of the burrow shows it to be nearly quadrate, with the above irregularity absent; but the most remarkable thing to me, and so far as I know it is new in the history of these burrowing annelids, is this, this burrow is regularly divided by transverse septæ. The diameter of the burrow is $\frac{1}{15}$th of an inch, and the septæ vary from the $\frac{1}{12}$th to the $\frac{1}{15}$th of an inch apart. The septæ are made up of very thickly-woven threads of what appear to be chitine. The object in building up these barriers across the burrow appears to me to be to protect the creature when at work, or to prevent the water diluting the erosive agent too much. Some water would naturally be left in the hole after the barrier was constructed, and this, no doubt, is quite sufficient for the creature's wants. But in this mode of working, the waste of material in building up these numbers of septæ must be immense on a small animal like this.

Unfortunately the annelid had escaped by a hole in the surface of the shell before I found it, I am therefore quite ignorant of the species which makes this remarkable burrow.

I am, Sir, yours obediently,

EDWARD PARFITT.

* See also remarks on this, 'Ann. Nat. Hist.,' 1870, pp. 80, 81.

PROCEEDINGS OF SOCIETIES.*

Royal Microscopical Society.

King's College, *March* 6, 1872.

Wm. Kitchen Parker, Esq., F.R.S., in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations was read, and the thanks of the meeting given to the respective donors.

A paper was read from Dr. Woodward, U. S. Army, "On Resolution of *Amphipleura pellucida* by a $\frac{1}{70}$ of R. and J. Beck, compared with other Objectives." The paper was illustrated by three photographs, taken by Dr. Woodward. The thanks of the meeting were accorded to Dr. Woodward.

In pursuance of the notice given at the preceding meeting, the ballot was taken for the election of Dr. George Charles Wallich as an Honorary Fellow of the Society, and the President announced his election.

Mr. J. W. Stephenson then read a description of "Stephenson's Erecting Binocular."

The thanks of the meeting were presented to Mr. Stephenson.

Dr. Klein then read a paper "On the First Stage of the Development of the Common Trout" (*Salmo fario*).

The President said he knew of nothing more interesting than the subject of Dr. Klein's paper. Anyone getting fairly started on the development of the embryo from the fecundative germ, had entered upon the most beautiful and interesting portion of science. The subject possessed charms for him which could be found in nothing else. In comparison with Dr. Klein's researches, his own seemed coarse and secondary. He wanted to know how the processes described took place; how the germinal mass through the amœboid particles could in some wonderful way fetch material from one part to carry it to another; what the fission of the primary parts meant. Numbers of such questions arose in the mind of the biologist in listening to such a paper as Dr. Klein's. His own work lay in the parts that formed right and left of the notochord, and in those parts that grew round the nervous centres; and he wanted it shown what was the difference between the parts that formed round the face downwards, and those growing downward from each side the notochord; what was the relation to the ear and the nasal sacs; and what the facial arches were; we did not know what to call them. A very interesting paper had come before the Linnean Society,† written by his friend St. George Mivart, on the laminæ that formed the *peritoneum*, as well as on those that formed the outer walls of the chest. All these were intensely absorbing researches, rivalling in wonder the questions that occupy even the astronomer himself. He (the President) had seen in Dr.

* Secretaries of Societies will greatly oblige us by writing their report legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. 'M. M. J.'

† 'Trans. Lin. Soc.,' vol. xxvii., pp. 369–392, plate lxiii.

Klein's preparations the involution of the blastoderm which went to form the internal ear; and of that growth outwards of the nervous centre, the form of which might be imagined by picturing to one's self a dumb-bell, in which there was a central raised mass, besides the two terminal knobs, these outer lobes being the rudiments of the most important membrane of the eye. Then there was a horny layer coming over in front to finish it. There was thus a double process in the formation of the organ of vision. Many fascinating marvels would be witnessed in the formation of these special organs, in the ear not less than the eye; its first forms, then, the enlargement of the labyrinth and the closing of it in, and the construction of its different parts. It was a subject which the younger Fellows of the Society should take up and work out.

Dr. Klein then read another paper, "On the Nerves of the Cornea."

Mr. Stewart (in answer to a question from the President) referred to the observations made by him at a recent meeting, when the question was introduced whether nerves passed in the way described, and repeated his offer to show his preparation to anyone who wished to see them. In examining even the small trunks of nerves it could be seen that they were composed of a number of fine fibres, and these fibres could be traced leaving the main skein in which they were associated, somewhat in the same way in which it might be supposed a wire cable could be split up. These fine fibres would be seen to be really formed of a number of laminated fibrillæ, which left the main trunk of nerves in numbers of from 300 to 500, gradually unravelling so minutely, that no graver could represent them.

A vote of thanks was then unanimously accorded to Dr. Klein for the two papers he had read.

Dr. Klein then exhibited vertical sections through the blastoderm of the trout, 12, 13, 14, and 18 days old, and some very beautiful preparations of fine nerve fibres of the frog's cornea, and also of sub-epithelial and intra-epithelial fine nerve fibrillæ of the rabbit's cornea.

The meeting was then adjourned until the 3rd April next.

Donations to the Library and Cabinet from February 7th to March 6th, 1872:—

	From
Land and Water. Weekly	*The Editor.*
Nature. Weekly	*Ditto.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal. Weekly	*Society.*
Journal of the Linnean Society, No. 66	*Ditto.*
Quarterly Journal of the Geological Society, No. 109	*Ditto.*
Berichte des Naturwissenschaftlich-medizinschen Vereines in Innsbruck	*University Buchhandlung, in Innsbruck.*
Om Siphonodentalium vitrium, &c., af Dr. Michael Sars	*L'Université Royale de Norvège, Cristiania.*
Mémoires pour servir à la connaissance des Crinoïdes vivants, par Michael Sars	*Ditto.*
Carcinologiske Bidrag til Norges Fauna, af G. O. Sars	*Ditto.*
Three Photographs of *Amphipleura pellucida*. By Dr. J. J. Woodward, U. S. Army	*Author.*
A Safety Stage for Ross' Microscope	*J. W. Stephenson, Esq.*
Four Slides of Mycetoma	*Jabez Hogg, Esq.*

The following gentlemen were elected Fellows of the Society:—

Phineas S. Abraham, B.Sc. and B.A.
A. de Sonza Guimaraens, Esq.
Robert King, Esq.
Henry E. Symons, L.R.C.P. Lond., M.R.C.P., &c., and
George Charles Wallich, M.D., an Hon. Fellow.

WALTER W. REEVES,
Assist.-Secretary.

BRIGHTON AND SUSSEX NATURAL HISTORY SOCIETY.

January 25th.—Microscopical Meeting. Mr. Hollis, President, in the chair.

Mr. R. Glaisyer announced the receipt from Mr. R. Beck, of London, of a test-slide of Podura for the cabinet.

Dr. Hallifax, who introduced the subject for the evening, "Parasitic Plants," remarked that a distinction must be drawn between those which simply attached themselves to organized beings, and those which lived at the expense of the plants and animals on or in which they grew, for they were both external and internal. The ivy, which attached itself by its suckers to a wall equally with a tree, and did not live on the juices of the plant, was an epiphyte, whereas the mistletoe, the best example of a true parasite, obtained its food at the expense of the tree on which it grew, although the tree did not seem to suffer. The mistletoe seeds, surrounded by a viscous substance, were, it is believed, carried by birds, became attached to the branches or trunks of trees, and there germinated, sending their fibres through the bark into the sap-wood, and they, continuing to grow at the same time as the tree, penetrated deep into its substance, and received from the plant its juices. With the exception of its not possessing true roots, it differed in no respect from any other perfect plant, for it elaborated the crude juices obtained from its host into woody fibre, &c., of a different character from that of the tree in which it grew. Thin sections, cut through both, showed that the cells lay side by side, different in character and not coalescing.

Of a more minute character were some of the fungi, for though many grew on organisms they did not make their appearance until decay and death had set in; but some, like the potato fungus, not only grew on the living plant, but completely destroyed it, penetrating into all parts of its substance. Other parasitic plants were the orobanches; but lichens simply attached themselves to plants for support.

Mr. Wonfor remarked that to the pseudo-parasites might be added the bird's-nest orchis (*Listera nidus-avis*), which did not appear to grow upon the roots of the beech, but only under beech-trees. He had never been able to trace any attachment to the roots. The dodders appeared to differ from some of the other parasites, for their seeds germinated in the soil and then attached themselves to the plant on which they grew, not only sending roots into the substance, but twisting and twining round it like the ivy. Hence they had been called stranglers.

Of fungi which grow on and at the expense of animals, he might

mention those which attacked living flies or moths and glued them to a window-pane, the caterpillar-fungus of Australia, the fungus-foot of India, ringworm, &c.

As regarded the mistletoe, there was no doubt birds were the chief agents in its propagation, for it was generally found high up on trees. One very curious fact must have been noticed, *viz.* that its most active period of growth was during the winter months, when the trees were said to be dormant. Then it was it flowered and ripened its fruit.

There had been great difference of opinion whether the Druids held sacred our *Viscum album*, or the *Loranthus Europæus*, commonly found in oaks in the south of Europe, whereas the former was now but rarely found on the oak; in fact, some, in ignorance of its existence, went so far as to say it did not grow on the oak in England; but examples could be found in all oak-growing districts. People seemed to forget that it was not the mistletoe, but the oak-growing one which was held sacred, and which, from its comparative rarity, was searched for with great care. The greater proportion of the mistletoe sold at Christmas came from the Channel Islands and France, where its berries were produced much earlier than in England. While it grew on a great number of trees, the apple-crab, poplar, and ash were the most common.

Mr. G. Scott mentioned that he had seen it on the laburnum.

Mr. Sewell had seen it growing on fuchsia trees in Jersey. An example of an internal parasite was the *Sarcina ventriculi*.

The meeting then became a conversazione, when Dr. Hallifax, Messrs. R. Glaisyer, Sewell, and Wonfor, exhibited male flowers of mistletoe, containing pollen, sections of the seed, leaf, stem, and of mistletoe and crab growing side by side, the caterpillar fungus, the potato fungus obtained in the autumn of 1865, and other interesting examples of parasitic plants.

Mr. Wonfor exhibited a very ingenious slide for opaque objects, consisting of a thin wooden slide, with circular cell turned and blackened, and having two very shallow grooves around, one for the covering glass, and another for a circular patch of gummed paper to fasten the cover. He had obtained them of Mr. Baker, Holborn, who supplied a dozen slides, with covers and patches to match, at 1*s.* 6*d.* per dozen.

In the account of the soirée of the Brighton and Sussex Natural History Society, in our last number, the names of Messrs. R. and J. Beck, as exhibiting some very exquisite photographs of microscopic objects, and of Mr. Hennah, as exhibiting a microscope and objects, were accidentally omitted. The omission was altogether due to the gentleman who furnished the report to the Editor of this Journal.

Fig. 1.

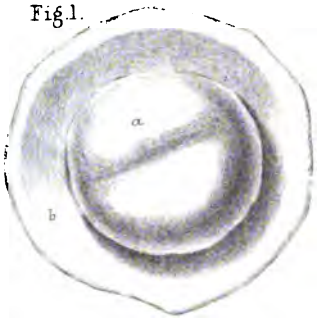


Fig. 7 A

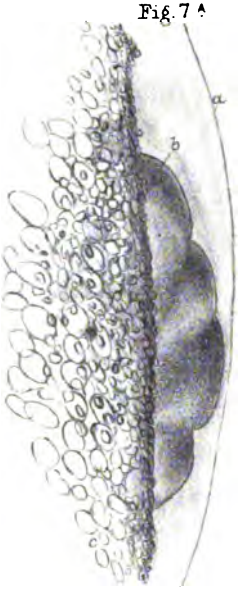


Fig 3.

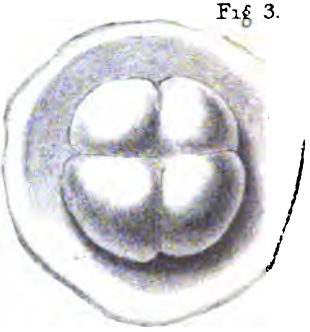


Fig. 2.

Fig 4.

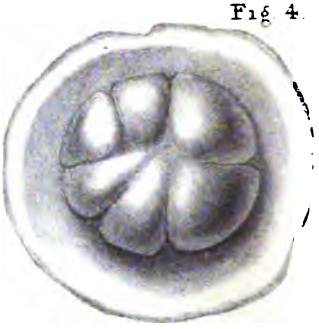


Fig. 7 B

Fig. 5.

Fig 6

Fig. 8.

E. Klein delt

Development of the trout.

W West & Co imp

THE

MONTHLY MICROSCOPICAL JOURNAL.

MAY 1, 1872.

I.—*Researches on the First Stages of the Development of the Common Trout (Salmo fario).* By Dr. E. KLEIN.

(*Read before the* ROYAL MICROSCOPICAL SOCIETY, *March* 6, 1872.)

PLATES XVI. AND XVII.

RUSCONI* was the first to describe the segmentation of the blastoderm of the egg of Teleostean fishes. His observations were made on the Cyprinoids, and he states that the mode of segmentation is quite of the same type as that of the Batrachia, known by Prevost and Dumas in 1824, and by Rusconi himself in 1826. The statement of Rusconi was confirmed by Lereboullet† for Pike and Perch;

EXPLANATION OF PLATES XVI. AND XVII.

DEVELOPMENT OF THE TROUT.

FIGS. 1, 2, 3, 4, 5, and 6.—Showing the different stages of segmentation of the blastoderm. *a*, blastoderm. *b*, yolk.

„ 7 A and 7 B.—Showing the blastoderm of different stages in the profile. *a*, vitelline membrane. *b*, blastoderm. *c*, fat-globules of the yolk.

FIG. 8 A, 1 and 2.—Elements of the blastoderm of four days.

„ 8 A, 3, 4, and 5.—Elements of the blastoderm of seven days.

„ 8 *a* and *b*.—Elements of the blastoderm of ten days.

„ 9.—Vertical section through the blastoderm of three days. *a*, blastoderm. *b*, yolk of the saucer-like depression.

„ 10.—Vertical section through the blastoderm of six days. *a*, blastoderm. *b*, yolk. *c*, fat-globules of the same.

„ 11.—Vertical section through the blastoderm of twelve days. *a*, blastoderm. *b*, segmentation cavity. *c*, large elements on the bottom of the cavity. *d*, superficial layer of flattened cells of the blastoderm. *e*, yolk.

„ 12.—Vertical section through a blastoderm of fourteen days. *a*, segmentation cavity. *b*, peripheral swelling of the blastoderm. *c*, superficial layer of flattened cells of the blastoderm. *d*, lower layer of cells of the thinner part of the blastoderm. *e*, large elements on the bottom of the cavity. *f*, yolk. *g*, fat-globules of the yolk.

„ 13.—Vertical section through the embryo of eighteen days. *a*, superficial or corneous layer. *b*, second or nervous layer. *c*, third or motoric-germinative layer (middle layer). *d*, lower or epithelio-glandular layer. *e*, yolk. *f*, central nervous system. *g*, chorda dorsalis.

* Müller's 'Archiv,' 1836.

† 'Annales des Sciences,' 4 S., T. 1, 2.

it has been further confirmed by Coste * for Gasterosteus and by Carl Vogt for *Coregonus palea*. Stricker † has made assertions respecting *Salmo fario*, which do not agree with those of Rusconi.

According to Stricker knobs grow from the blastoderm in an irregular series, which become separated by a constriction from the main body of the blastoderm; from the primary knobs grow up secondary ones, which likewise become separated.

According to Stricker the segmentation, that is the splitting of the whole blastoderm, is completed in forty-eight hours. Stricker came to these conclusions by studying eggs after hardening them in chromic acid.

I have studied the segmentation of the blastoderm of four sets of eggs of *Salmo fario* in the living condition, and I have arrived at conclusions, which are in many respects different from those of Stricker, and which agree as regards the first stages of segmentation with the observations made on other Teleostean fishes.

A fertilized trout's egg exhibits under the microscope, up to the sixth hour, a rather uniform aspect. The egg appears as a dull globe enveloped in a thick vitelline membrane, and shows irregularly-distributed fat-like globules.

The earliest time, at which I was able to perceive an indication of the blastoderm, was five hours and forty-five minutes after fecundation, when there appeared in one place a small, somewhat more opaque, irregularly-outlined spot, in the circumference of which the fat-globules had accumulated. If the breeding apparatus is kept in a cold place, as is generally done, about the tenth or twelfth hour after fecundation, the blastoderm seen from above, or better in profile, appears as a distinct mound, which lies on a saucer-like depression of the yolk: a space is consequently left between the vitelline membrane and the yolk (compare Fig. 7 A). In that part of the yolk which represents the bottom of the saucer-like depression lie accumulated the fat-globules.

As soon as the blastoderm appears as a distinct mound, there are to be seen on it very remarkable changes. If we observe a blastoderm carefully for a considerable time with a No. 4 Hartnack, we find at first that its border does not remain in the same condition, but exhibits in different places slight changes. On one place, for instance, where the blastoderm has been sharply outlined, its margin becomes more transparent and slightly projecting, as if the mass of the blastoderm at this point had sent out a flattened prolongation; after a short time the prolongation is retracted, and the general outline of the blastoderm becomes sharp as before. The same appearance occurs in different places. The prolongations of the blastoderm which appear and disappear are of very different

* 'Histoire de Dévelopm. d. Corps Org.'
† 'Sitzungsber. der Wiener K. Akademie der Wissenach.,' Bd. 51.

sizes. It is evident that these appearances are due to an amœboid movement of the blastoderm.*

Soon after the blastoderm exhibits the appearances above mentioned, there is seen on its surface a median clear stripe, which by-and-by becomes clearer; this clear stripe represents the first furrow of segmentation, by which the blastoderm becomes divided into two halves.

The blastoderm now seen from the surface appears as if notched at two opposite points.

Of the presence of the first furrow we may convince ourselves by looking upon the blastoderm in profile. If an egg on which the first furrow has been made out in the surface view is looked at in profile, which may be done successfully after some manipulation, we may perceive that the blastoderm exhibits only in certain positions a notch, by means of which it appears to consist of two knobs of equal size; if we turn the egg round, we observe that the more we approach an angle of 90°, as regards its previous position, the more the notch displaces itself, so that the knobs become of unequal size. If the angle of 90° is complete, the surface of the blastoderm appears quite smooth; that is, the blastoderm is now so situated that the furrow lies horizontally.

If we turn the egg again through an angle of 90° the blastoderm appears once more as if consisting of two knobs, that is, the blastoderm has such a position that the furrow stands vertically.

A few hours after the first furrow has appeared, there appears a second furrow vertical to the former: the blastoderm seen from the surface now exhibits a cross of clear lines, apparently such that the two quadrants of the one half are smaller than the two quadrants of the other (see Fig. 3). The blastoderm seen in profile now exhibits in all positions a notch; that is, it appears to consist of two knobs.

* Oellacher, in a recently-published paper about the germinal vesicle (Max Schultze's 'Archiv,' 8 Bd. 1 Heft), says on p. 14, "That the germ (of trout) raises itself from the yolk-groove shortly before the fecundation, contracts itself, and spreads out again: that has Stricker described ('Wiener Sitzungsber. der K. Akademie,' Bd. 51) and has attributed it to active contraction. . . . . I can only confirm these observations most decidedly."

To these very decided assertions I have to add the following:—

(1) Oellacher makes Stricker to say something that Stricker never has said. Stricker in the above-mentioned memoir concludes, from the peculiar appearances which chromic acid preparations exhibit and which he (Stricker) mistook as appearances of segmentation, that the blastoderm possesses the power of active movement, by means of which knobs grow up, which afterwards separate from the main body. According to Stricker this proceeding forms a material part of the segmentation. But that Stricker has said "that the blastoderm shortly BEFORE THE FECUNDATION raises from the yolk-groove, &c., &c.," of that I cannot find anything in the mentioned memoir.

(2) I allow myself to doubt at once whether it is possible to OBSERVE contractions of the blastoderm on an unfertilized ovum of trout; because, as I have already stated, there is nothing *to be seen* of the blastoderm in *the living condition* at this time, even not in the first hours after the fecundation.

There is to be noticed in some eggs a peculiarity of the first two furrows, which consists in this, that they do not cross each other at right angles, but form a double V, the points of which do not touch each other, but are joined by a straight line (see Fig. 2).

At the end of the first day the blastoderm of nearly all eggs exhibits the first two furrows; whether the blastoderm gets a third equatorial furrow I cannot distinctly say; I take it as very probable, because I have found that the blastoderm persists in the stage of the first two furrows a comparatively long time. The next stage I have noticed appeared thirty-six hours after fecundation, and consisted in this, that the blastoderm seen from the surface showed two quadrants in division, that is, six knobs (see Fig. 4).

Until the end of the second day we find the eggs in the different stages between this just mentioned and a stage in which the blastoderm showed, when seen from the surface, four whole furrows, that is, eight knobs. This was also the last stage which could be regarded distinctly as a regular one.

The blastoderm which I have looked at during the third day, in the surface as well as profile view, reminds me very much of what Stricker has described and illustrated, for we see that the knobs of which the blastoderm consists do not exhibit any regularity either in their position or in their size. I could not make out any regularity even when the blastoderm exhibited in the surface view only sixteen or twenty knobs.

Until the end of the fourth day, even when the blastoderm appeared on its surface like a mulberry, I could distinguish very easily the single knobs with a magnifying power of only 90 diameters, so that we can say that the segmentation is far from being finished.

However unequal the time may be in which the eggs of the same set become developed, I must still consider, from what I have seen in fresh preparations and sections, that the shortest period for the segmentation is nine days. Of course the segmentation can be accelerated, if we allow the eggs in very small numbers to develop in the ordinary temperature of a room.

The elements of the blastoderm between the fourth and the tenth day, which can be examined in the fresh state only by puncturing the egg and enclosing the blastoderm with oil after mounting it in the yolk itself, appear as pale finely-granulated bodies; in many of them there is a central vesicular nucleus-like body to be seen. The elements exhibit distinct amœboid movements: from the protoplasm of the body there grow out slowly hyaline knobs of different sizes, which after a short time are again retracted.

The elements of one and the same blastoderm exhibit a great difference in size. There are always a great many elements to be found apparently undergoing division (see Fig. 8 A).

The elements of a blastoderm of a later period (for instance ten days) appear somewhat peculiar in this respect.

If the observation is continued, we can see that some elements which have been at first of a spherical shape, become of a cylindrical shape; further, we can see that on the latter there appear transverse grooves at nearly equal distances, as if it would divide itself into a certain number of smaller spherical elements (see Fig. 8).

About the spreading out of the blastoderm on the saucer-like depression of the yolk; about the large elements which are to be found on the bottom of the segmentation cavity; about the growth of the blastoderm round the yolk globe like a cap, the peripheral part of which is thickened; about the foundation of the embryo at the peripheral thickening, I have only few things to add to what has been shown by Stricker.

Between the tenth and twelfth day the blastoderm, which in the last days of the segmentation has become much larger and thicker, is seen to grow very rapidly in breadth. A fresh egg shows the blastoderm in this stage more transparent at the centre than at the periphery. On a section we recognize that the central portion of the blastoderm is much thinner than the peripheral portion; the central portion does not touch the yolk, but is separated from the saucer-like depression of the yolk by a cleft. The elements, of which the blastoderm consists, are in the deeper layers of the central part as well as the peripheral part larger than in the superficial ones. The most superficial layer consists of very much flattened cells; on sections it appears, as if the blastoderm were covered by a thin layer of spindle-shaped cells. We recognize at the same time that the blastoderm is not raised from the saucer-like depression of the yolk for the whole length of the cleft, that is, the segmentation cavity, but rests on the latter from place to place, by means of columns of cells, as if by pillars. The cells of these columns—subgerminal processes—are larger than those of the blastoderm, and are coarsely granular: we find cells similar to these on the bottom of the segmentation cavity too; amongst these we recognize some, which are very much larger, and possess two, three, or more nuclei. There is nothing easier than to convince ourselves, with Rinek,* first, that these elements which lie on the bottom of the cavity are products of the blastoderm itself, that is, that just as the blastoderm is lifted from the yolk, there remain some elements on its saucer-like depression; secondly,

* Max Schultze's 'Archiv,' Bd. V.

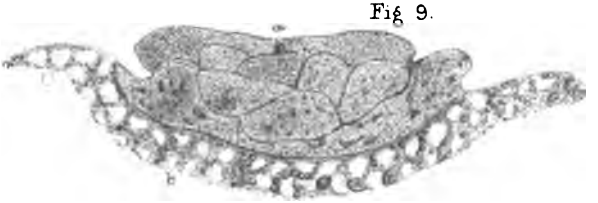
that the elements of the subgerminal processes separate themselves from the blastoderm, and remain on the bottom of the cavity the more the blastoderm is lifted off; and thirdly, that the further the egg is developed, the more these elements lying on the bottom of the cavity come outwards, so as to increase the peripheral thickening of the blastoderm (see Figs. 11 and 12).

The more the blastoderm grows round the yolk globe, the more the former becomes thinner in its central part. If we look at a section through the blastoderm of fourteen days, we observe that the central portion of the blastoderm is composed only of two layers of cells; the superficial layer consists of flattened cells, the deeper layer of spherical cells arranged more or less loosely in a row. The former layer is continued into the similar one of the peripheral thickening, the latter into the second stratum of the peripheral thickening; here this second stratum contains one or two layers of more or less cylindrical cells. Below these two strata the peripheral thickening contains a few more layers, at least two of spherical, distinctly granular, larger cells, with one or two nuclei. The cells of these latter layers are continuous with the large elements which lie on the bottom of the cavity (in this stage of development of course only at the peripheral part of the cavity; in the central part of the bottom of the cavity there are no more or very rarely such elements to be found).

About the fifteenth or sixteenth day the blastoderm has grown round somewhat more than a quarter of the yolk. In fresh eggs as well as in those hardened in chromic acid the contrast between the thickened peripheral part and the rest of the blastoderm is very striking. In eggs hardened in chromic acid, and deprived of their vitelline membrane, the former appears as a whitish ring; the rest of the blastoderm is so thin that the fat-globules of the yolk shine through it. About this time we observe that at one spot of the peripheral thickening a knob-shaped prominence shows itself which slopes towards the centre. This prominence represents the first trace of the embryo. In the subsequent stages (one day after) the embryonal prominence has already increased so far that it stretches like a cord from the elevated border towards the centre of the germinal area. We may now also observe with a lens that the end directed towards the centre is rounded, and further that the embryonal prominence projects beyond the extreme contour of the elevated border of the blastoderm in the form of a small knob. At the point of junction of the cephalic third (*i.e.* the portion turned towards the centre of the blastoderm) with the rest of the embryonal prominence we observe a shallow groove, the first indication of a dorsal groove, which is continued towards the centre (the head) and towards the periphery (the tail) a short distance, becoming *more* shallow, so that it may be

Fig. 8 A 1

Fig. 9.



8. A 3

8. A 2

Fig. 10.

8. A 4

8. A 5

Fig. 11.

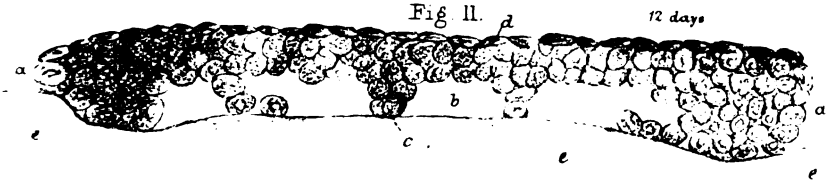


Fig. 12.

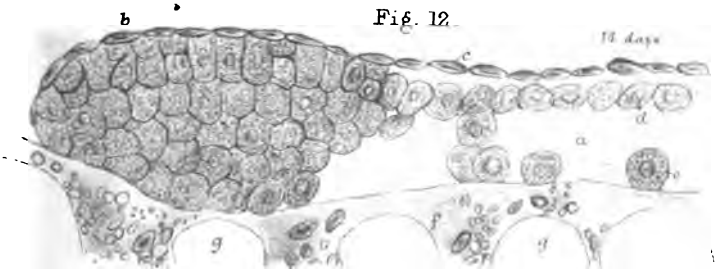
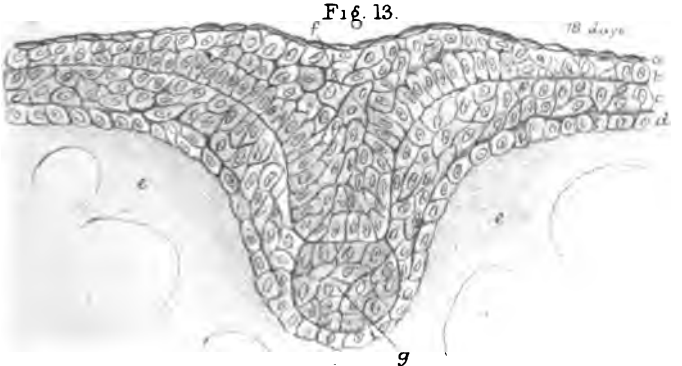


Fig. 13.



F. Klein del.

W. West & C° imp.

Development of the trout.

called rhombic. We observe further that where the groove is deepest a cord stretches from the embryonal prominence on both sides towards the centre of the blastoderm, where the cords of opposite sides meet, forming a ring which completely surrounds the cephalic extremity. This ring, which I shall name the secondary elevation, is somewhat thinner than the peripheral elevation, and at the time when I first saw it, it was so large, that its periphery, turned from the embryonal prominence, nearly reached the centre of the blastoderm.

In the immediately subsequent stages, until the period when the blastoderm has further grown round half the yolk, during which time the embryonal prominence has apparently become elongated, the circles increase in size, both the circle enclosed by the secondary elevation as well as that enclosed by the peripheral elevation; but both these elevations decrease in thickness.

From the stage when the peripheral elevation corresponds to the greatest circumference of the egg, the circle enclosed by the secondary elevation increases, whilst of course that of the peripheral elevation decreases, because the blastoderm now begins to grow round the other half of the yolk.

About the twentieth or twenty-first day the blastoderm has grown round the yolk so far, that a portion only of the yolk, no larger than the head of a pin, is exposed; the embryonal prominence is very long and thin.

Of a dorsal groove nothing more is to be seen. The peripheral elevation, now confined to a small ring, is just to be seen as such; the secondary elevation also, which now forms the equator of the egg, is just perceptible. Soon after (half a day to one day), the last indication of the peripheral elevation, as well as of the secondary elevation, disappears, and the last remains of the opening lying behind the tail of the embryo, on which the yolk was exposed, have disappeared. The blastoderm has become closed behind the tail, and has therefore grown *completely* round the *yolk*. We cannot therefore speak of the existence of an opening which should become the anus (*Rusconi* and *Stricker*).

If we make a thin section through the embryonal prominence of an egg seventeen or eighteen days old, which embryonal prominence has been hardened in very dilute chromic acid, we find that the embryonal prominence consists of the following layers: an upper layer corresponding to the corneous layer (Hornblatt); and then under this a nervous layer (Nervenblatt), consisting of several layers. Under this there follow two layers more (motorisch-germinatives Blatt and Darmdrüsenblatt), each of which consists again of two layers of larger cells. The nervous layer raises the

corneous layer in the form of two not very prominent elevations, so that a slight depression or groove results. We observe further that from the nervous layer a process extends deeply, and touches the chorda dorsalis, which has differentiated itself of the third layer, or motoric-germinative layer.

It is clear without more explanation that this median process of the nervous layer represents the central nervous system. (Compare Fig. 16.)

If we return to an earlier stage, or if we compare the sections which are made from nearer the caudal extremity, then we are convinced that the earliest indication of the central nervous system is always to be recognized as a solid process of the nervous layer, which is directed deeply, which process pushes before it the layer (lying under it in the middle line) more towards the yolk, and also becomes elevated towards the surface in the form of two elevations, which are, however, but slight.

Whether these elevations grow against each other so as to transform the slight groove between them to a canal, that is to say, whether the mode of establishing the central canal of the central nervous system is the same as in Batrachiæ and cheek, or whether the central canal, which is to be found in somewhat later stages in the central nervous system, develops as a split in the solid nervous process, I am not able to say with certainty; although a great number of sections from different periods were more favourable to the latter view than to the former.

II.—*On the Classification and Arrangement of Microscopic Objects.* By Dr. James Murie, F.L.S., F.G.S., &c.; Lecturer on Comp. Anat. Middlesex Hosp.; formerly Pathol. Glasg. Roy. Infirmary; Assist. Conservator Roy. Coll. of Surg., Eng.; and late Prosector to the Zoological Soc., Lond.

(*Taken as read before the* Royal Microscopical Society, *Feb.* 7, 1872.)

*Part II.**

The present communication is but a continuation of a paper laid before this Society, and published in their Journal February, 1869. At that time it was my intention to have comprised in one article all that I had to say on the subject, and with this intent I drew up data which I subdivided accordingly. As it turned out, the matter was deemed rather voluminous for single insertion in the Monthly Journal of our Transactions, and what I may term Part II. was deferred. Meantime circumstances altered the tenor of my way, and the MS. then written out was put aside, only now to be revised. Excepting therefore this unavoidable delay, no material change in my original plan has been made, further than a revision of nomenclature in the main divisions of the vegetable kingdom. This is due to my friend Mr. Carruthers, of the British Museum, whose advice I have followed in the choice of that classification most accordant with the present tendencies of botanists, and freest from ambiguity.

In Part I. I divided my contents into nine sections, but of these only three were treated of. At this stage I merely repeat the three titles, as under-mentioned; and without further preliminary proceed therewith.

1. *Introductory.*
2. *Various Arrangements of Objects.*
3. *Microscopic Collection, College of Surgeons.*

4. *Principles of Classification.*—In Part I. I dwelt at some length on the modes of arrangement adopted by practical histologists or in use in existing collections, and on the breadth of purpose in amassing so much microscopic material.

Let us for a moment consider if there are any general rules or principles which it is useful to bear in mind; whatever be the classification chosen. Among others, the following appear to me worthy of consideration:—

I. It is fundamentally important not to multiply similar specimens needlessly; but rather by judiciously displaying them wheresoever most weight would be attached to their presence, concentrate attention on their real import.

* Part I. will be found in 'M. M. J.,' vol. i., p. 69.

II. It is of consequence that objects allied to each other should not, on feeble grounds, be placed far apart.

III. Keep as much as possible to a uniform style of nomenclature and size of slide, whenever the specimens admit of such.

IV. Endeavour to obtain good typical specimens, and see that they are in a sound state of preservation before finally giving them a position in the cabinet.

V. Reject all lumber, kept with the idea that numbers add to the value of a collection; whereas they rather weaken its usefulness.

These broad rules of guidance or general principles being admitted, it seems that the ultimate end of system in dealing with natural objects—and this is especially applicable to microscopic textures—is the attainment of a comprehensive grasp of the affinities and structural relations of things.

Around us everywhere matter exists in an infinity of forms, organic and inorganic. Materials building up organs adapted to perform functions ministering to the life and well-being of animals, or perchance supporting and maintaining vegetable organism. Materials wherewith minerals and precious gems are constructed, or elementary constituents out of which the chemist transforms substances applicable to the arts and sciences. In other words, the raw material of all useful and ornamental products, such as food, clothing, and microscopic art; also earthy substances of whatsoever kind, and intimate structures and organs of all vegetables and animals, recent and fossil. Indeed, everything likely to be examined by the microscope or retained as a preparation or interesting object in a histological collection, reduces itself within limits, when the elementary composition is unravelled. This limit is its primary textural component parts; and therefore this forms one kind of basis of classification to start from.

As, however, the simple elementary parts reconstruct themselves in a variety of ways into tissues and organs, and these tissues and organs again develop themselves so as to constitute systems; and as the systems are exponent in a great measure of the types of construction of vegetables and animals, we have in these so-called systems, normal and abnormal, a gauge wherein to measure and compare structural differences of two of the kingdoms of nature.

The third or inorganic kingdom comports itself more as to chemical composition, or the manner in which the masses are aggregated; so that instead of systems of organs, we have groups of substances to deal with.

Lastly, microscopic optical apparatus, art-microcosms, &c., are associated according to use, or whenever the subject agrees in bearing.

I mentioned that naturally the primary elementary tissues lead to a basic series, whereon a classification might be taken to start

from. These then would either precede or form the main source of divisions. On such grounds, most collectors of microscopic objects and writers on Histology form or dwell on many subdivisions of minute textural anatomy. Particularly was this the case with the late Professor Quekett, who dealt largely and classified extensively, according to the gradation of the elementary tissues.

Time-honoured and sanctioned on good authority as is the case, I nevertheless disagree with the custom of an elementary series preceding the more regular natural arrangement of objects in cabinets, except under certain conditions. I grant that introductory chapters on histological elements in manuals and text-books are not only useful, but a necessary prelude to the thorough understanding of the composition of organs forming bodies treated on. The circumstances of the case, however, are somewhat different, namely, between a treatise on histology generally and a cabinet of preserved specimens adapted for consultation and reference, where it is supposed the primary elements are to some extent mastered.

My reasons for divergence of opinion in the above point are:—

1. That a separate elementary series causes an unnecessary duplication of specimens; the same object being found in several places in the collection.

2. It mars the general harmony and sequence of the grouping.

3. The elementary tissues are in reality part and parcel of the main subdivision of organs and systems, and hence ought to be looked for therein.

4. The exception would be when a collection was used for teaching the rudiments of histology. In such a case probably nothing could be better devised than what has been done by Quekett in the first volume of his histological catalogue, or by Kölliker and other Continental authorities in their hand-books. Membranes, fibres, woody tissues, cells, cartilages, bones, pigments, silicious spicula and shell structures, are part and parcel of some organ. Wherefore disjoin them from their respective vegetable or animal organism, and render duplicates necessary when simplicity is the mother of order?

I am aware objections may be raised against departure from the ordinary routine, and the plea urged is that it is so convenient to have the elementary textures apart for separate reference. I am quite willing to acquiesce in this view if those who use it limit the application to small private collections where necessarily little more than types of fibre cell, &c., are present. But it becomes an entirely different thing its being followed out with that wonderful detail manifest in the great Quekett collection. There every variety of primary vegetable and animal tissue—cellular, vascular, and sclerous —is most elaborately represented, again to be repeated in a succeeding part of the collection.

In fine, for useful purposes, in a small as well as large collection, it seems to me a moderate illustration of elementary tissues is sufficient to be kept apart. These need not include inorganic substances, vegetable and animal components together in a series; but under each kingdom a few typical slides will convey all that is wanted concerning the material which builds up organs, &c. As to their proper position, they may precede, be dovetailed as a subsection in the midst, or terminate large groups. But I reiterate that ostensibly to anyone fairly acquaint with structural morphology, cellular, fibrous tissues, and such like, can nearly always find a convenient corner along with the group of organs they help to build up.

The microscope rigidly defines the structures differentiating the mineral, vegetable, and animal kingdoms; these groups, practically speaking, are thus sufficiently characteristic to form a groundwork of classification. They therefore serve as primary natural divisions. Proceeding from the simple to the more complex, the mineral kingdom first engages attention.

The classification of minerals and rocks is dependent either on their chemical composition or the peculiar form of their crystal—so micro-chemicals precede micro-minerals. In the structure of chemical elements and compounds, or simple and complex mineral substances, I am not aware of any gradation of minute form upon which a microscopic classification could with propriety rest. The subdivisions in either case therefore must be few, and follow conventionally the recognized systems employed in text-books.*

Amongst others, polarizing objects may form a convenient subsection. When many specimens of a single substance obtain—take, as a sample, coal sections—then a division geographically as well as structurally is valuable. Especially in such a substance as that mentioned is it legitimate, because there is no very clear gradal series to classify them with texturally. I differ from those who form a microscopic geology. Microzoal deposits or structures, whatever their nature, truly belong to the series of micro-biological specimens, if the term is applicable.

Above all, it must be remembered that the variety of purpose of a collection, its limited nature or miscellaneous character, render equivalence or diversity in grouping convenient or otherwise. But I would press my conviction, that there should always be appended to each natural division a series illustrating application to the arts and manufactures, whereby utility to every-day life, as it were, is recognized; and whereby comparison of objects as respects their purity and genuineness can freely be tested.

* I must not omit allusion to a paper by Mr. Davis Forbes, "The Microscope in Geology," 'Popular Science Review,' Oct., 1867, whose classification I adopt in my concluding section.

When the vegetable kingdom is reached, the natural orders of plants, or the divisions and subdivisions used by botanists as exhibiting natural affinities, are those which I believe should certainly regulate the arrangement of a histological collection. It may here be observed, and I should wish to lay particular stress on the circumstance, that whilst the main arrangement is a botanical one founded on natural affinities, the subsidiary divisions moreover ought to be of a physiological character. In this way, not only the differentiation of elementary structures is grouped progressively, but the separation and complexity of organic function is specially defined.

An essential difficulty in rigidly carrying out such a natural and structural classification of vegetable organisms or botanico-physiological arrangements, is the fact, that the lower forms can be viewed in their completeness in a single slide; while the higher forms, both on account of their size and segregation of organs, require many subseries to illustrate their anatomy.

This difficulty of arrangement, which equally applies to the animal kingdom, makes it essential that the lower forms, almost up to the ferns, should primarily follow their natural generic affinities, but their subsidiary divisions nevertheless be subject to physiological principles. On the contrary, the higher forms, Monocotyledons and Dicotyledons, should primarily be divided physiologically, and secondarily botanically. Further exemplified; the Root, Stem, Flower, Fruit, or organs of nutrition—skeleton, reproduction, &c., could not be arranged separately in every individual order or genera of Dicotyledons without producing a conflicting confusion in the classification.

Among the Diatomaceæ, for example, many genera may occupy only one slide. In such a case the most numerous forms or fewer leading characteristic ones, point where ought to be the true place of the slide. Again, it may so happen that a set of slides are mounted for the express purpose of demonstrating points connected with a particular locality. It would be a loss rather than a gain, therefore, to break up and distribute such a series, unless means (of which I shall hereafter mention) be taken to preserve their recognition as belonging to the original set in question. Diatomaceous earths grouped geographically or according to their geological position, stratigraphically, will teach their distribution and range in time.

Parasitical forms, such as Fungi, are usefully and naturally grouped according to the situations they are found in. But in all cases series of the genera and species should take precedence.

The changes coincident with development should form a series and subsection under the different groups.

In the division of Monocotyledons, Dicotyledons, &c., as above stated, the several organs, or apparatus, circulatory, respiratory, and such like, form the chief feature of arrangement. But fossil

forms here come to play an important part. These, when feasible, should be set under the organ and order they belong to. In the case, for instance, of stems, whether of recent or fossil woods, when the genus of the plant is unknown, a geographical division or fitness for manufacturing purposes may judiciously guide the arrangement. Fossil specimens may even be separated into a subsection where doubt exists as to their exact affinities. A series illustrating peculiarities in development, to wit, abnormalities in growth, should succeed the physiological. Vegetable fabrics, adulterations, &c., ought to terminate the vegetable kingdom.

The principles guiding the division of the animal kingdom are in many respects identical with those regulating vegetables. The ascending series is the most rational. From simplicity to complexity and the differentiation of organs, thus most naturally and justifiably are followed.

In the lower animal forms, as in the vegetable world, a slide may contain the entire organism. Here, therefore, a zoological arrangement into orders, genera, and species is sufficient. In higher forms the systems of organs, physiologically or anatomically considered, take the precedence of orders, families, genera, and species. Accessory organs connected with or subsidiary to a function follow the organ itself.

With the great bulk of Microscopic objects, generally speaking, it is not difficult to decide where each ought to be placed in a series arranged in the manner herein proposed. Nevertheless, numerous instances may occur in which it is not so easy at once to assign their position. By due consideration, however, of what special topic the slide is meant to illustrate, one may arrive at a conclusion where its nearest allies are to be found in the cabinet; and by balancing its minor relations we ultimately find a suitable situation for it.

At first sight a matter such as this may seem of trivial importance. But as the whole aim of classifying specimens is, that each should fall into its proper section, where they can be consulted almost without a moment's reflection or loss of time, it behoves that once for all a definite place should be assigned each object, where it may afterwards be readily found when wanted.

The question has been asked me under what heading ought this, that, and the other thing to be put.

Now, as I cannot predicate all the possible contingencies which are likely to arise, I must confine myself to a few examples; leaving specialities to be dealt with in a similar manner according as they may arise. For instance, where is the most fitting situation for such objects as egg-shell, coprolites, calculi, pearls, insects in amber, works of art, micrometers, and test-objects?

Treating these *seriatim*, I would say that egg-shell, although composed of the salts of lime, is virtually an animal product, and therefore to be excluded from the inorganic division. As to its physiological place, it may take rank among component elementary bodies, supposing there is such a group, either form part of an embryological section, or come under a division accessory to the generative organs of birds. Its most natural position is illustrating the textural character and peculiarities of the enveloping masses of the young of vertebrates, and therefore subsidiary to development near to the ovum.

As respects coprolites, these might either be looked upon as examples of earthy substances or as peculiar fossil forms. But if we bear in mind that coprolites are preserved as microscopical objects almost always to demonstrate the remnants of minute fossil bony structure, reptilian or otherwise, then there is no difficulty in seeing that their true position is amongst osseous structures, and along with the section which agrees in the constituent bony particles.

Calculi might with equal justice be classed with chemical substances or pathological products, and when no special series of the latter exist, the former would be quite suitable to their nature. As, however, urinary deposits commonly form a subsection of the urino-generative organs, calculi may with equal propriety be inserted after them.

Ought pearls to go along with shells or as examples of disease? This, again, depends somewhat upon the nature of the collection. Strictly speaking, pearls are but a morbid condition of the nacreous material of shells, and hence are true examples of disease. They therefore, according to principle, should be ranged among pathological textures. In a miscellaneous series, however, where pathology forms either an unimportant factor or is not meant to be exemplified at all, then pearls necessarily come under the heading Mollusca, or shell; and in a subseries either together or after the representative genus to which the shell belongs.

Insects in amber, as a rule, are kept not as typical of the structure of the amber itself, but as exemplifying a peculiar nidus wherein an insect may become imbedded. I would incline therefore to place such an object among the groups of insects. Here either close to the genus which the insect represented, or in a separate and subsidiary heading devoted to "Insects imbedded in foreign substances," at the end of insect structure.

Works of art belong to the division of "Miscellaneous objects;" micrometers to the subsidiary heading of same devoted to "Apparatus, &c." Test-objects may either be kept along with apparatus, or as I conceive quite as philosophically, with their affined group of organisms when diatoms.

I might quote many other doubtful objects, but I presume the above are sufficient to support the principle that the most pronounced feature of a specimen is that which ought to rule its position in the cabinet.

5. *Cabinets.*—On a choice of these so much depends on the intention and tastes of the individual, and the nature of the collection itself, that no rule or recommendation on my part can be given which would meet the conceptions and wishes of every amateur microscopist or professed histologist. I shall discuss the subject, nevertheless, in its broader aspect; that is to say, on such grounds as may be of general interest, or lead others to make suggestions on what at present there is no very definite standard or agreement upon.

The dimensions of a cabinet is a matter concerning economy, convenience of space, and the conception the collector intends to fulfil. If the numbers of slides are likely to be or already are very extensive, then large-sized cabinets are in some respects most advantageous. At the same time they are not free from serious drawbacks.

Among the advantages are:—

1. Stability when well made.
2. Easiness of access and reference to groups of specimens.
3. Less danger of the specimens being injured from shifting about.
4. General roominess.

Objections to them include:—

1. Expensiveness.
2. Requirements of space.
3. Awkwardness and difficulty (from weight, &c.) in their being shifted about.
4. The labour of rearrangement is great when one department becomes overstocked.

Thus it follows that according as each of these qualities has weight with those who pronounce judgment, so will the balance in favour of or the reverse be arrived at.

Smaller-sized cabinets, while deficient in solidity, roominess, &c., have still several points of recommendation.

1. They are, *cæteris paribus*, less expensive.
2. Their size is convenient, inasmuch as space and change of place or plan is involved.
3. They are more readily arranged.
4. They admit of their number being augmented agreeably to the increase of the collection.

5. If made to a uniform size and of a cubical form, they can be piled up one above the other, and so built together as to command all the advantages and none of the drawbacks of an immense single cabinet.

I cannot offer a better instance, forcing conviction of the last-mentioned proposition, than by reference to the Botanical Department and Insect Room of the British Museum. There (besides old wall-cabinets) they have a set of cubes identical in measurements, each devoted to a group or subsidiary division, numbered and labelled accordingly, and so arranged that to all intents and purposes they represent but one vast cabinet.

For study and reference they are uncommonly handy, as they can be brought down to the table, and in fact shifted about at pleasure without the slightest injury to contents.

Such is my *beau idéal* of a microscopic cabinet, compound yet harmoniously single; adapted to meet the wants of a limited, a moderate, or a numerous series; expansion being in the ratio of increment of slides. But furthermore, as I shall presently mention, the same principle is applicable to very modest microscopical collections; such, indeed, as even the amateur or those of limited means may aspire to. As a closing sentence to this clause, I may even make bold to say that, like other fashions and hobbies, that of cabinets is an infectious one: a handsome piece of furniture is attractive. Would that the zest for a thorough mastery of the contents was as powerful a stimulant.

I do not propose giving a lengthened dissertation and criticism upon every sort of cabinet, but by allusion to a few vindicate in passing the more desirable features pertaining to economy, easy access, and desirability for classific purposes.

A.—As regards space and cheapness, the common boxes with racks, sold by all microscopic object makers, are undoubtedly very handy. They are subject, however, to three great faults. 1. Many specimens, particularly those in fluid, are liable to spoil in them. 2. Reference to individual slides is awkward, from their being tilted in position. 3. Numbers and names cannot easily be read, unless by picking up one and then another, in guess-like fashion.

As an example of a considerable collection kept in the ordinary rack-boxes, I may mention Dr. Greville's specimens of Diatomaceæ in the British Museum, of which there are 3637 in all. His method of numbering and cataloguing, to which Mr. Carruthers kindly called my attention, I shall again make reference to.

B.—Dr. Müller, assistant to Prof. Hoffmeister, of Heidelberg, a few years ago kindly favoured me with a sight of their Histologico-Educational Collection. They were then adding a series of

sections illustrative of medicinal woods. In lieu of an expensive cabinet, they had adopted the following economical arrangement, whereby the slides lay flat and were easily got at.

A piece of stout millboard 7¼ × 9½ inches, and covered with coloured paper, had forty-two holes punched out. The holes drilled at equal distances, ran in parallel lines 1 inch apart. Through these an elastic cord was passed, down one hole and up the next; continuing along one line of holes and returning the next. Having reached the farther one from the point of starting, the cord is reversed and brought back hole by hole to the latter, where a knot joins the extremities of the cord. Slides are then introduced beneath the cords, which retain them in place. Each corner of the millboard, above and below, has a wedge-shaped piece glued on to it, and there is a narrow strip introduced in the middle. The trays filled above and below with slides are then piled one over the other on a shelf, or in boxes, open in front, with labels attached to each consecutive group.

I mention this inexpensive mode of forming a cabinet rather as a curiosity than to recommend it. There is one other point also to be borne in mind, *viz.* the German glass slips are shorter than the English ones, thence *multum in parvo.*

C.—Dr. Carpenter warmly commends a form of book-box as excellently adapted in lieu of an ordinary microscopical cabinet. As he says, a large histological collection can be stowed away in the library shelves, among the other books, and consulted with the greatest ease. My friend, Mr. David Forbes, has a few of such boxes slightly altered from Dr. Carpenter's plan, so as to suit the different shape of slide used by himself. The construction otherwise is similar, so that one description may suffice for both.

Each case is about 10¼ inches high, 8¼ long, and 4 inches thick in outside measurements. It opens only from behind, and has a fixed shelf across its middle. Trays of light cardboard, to the number of eighteen above and as many below, are piled on the top of each other. A small tack serves as a handle to each tray or *quasi* drawer. Mr. Forbes uses slides each 1½ inch square, so that eight of these occupy a drawer; the number or name, according to circumstances, being towards the knob end of the drawer. Dr. Carpenter's slides are generally those in common use, 3 × 1 inch, and these to the same number, eight, lie athwart.

Doubtless these book-boxes are neat, and in many instances a very excellent substitute for a large cabinet. The different sets of objects are most readily classified, and the title placed on the back, apportioned to the contents. The great fault, however, lies in the one tray being so placed above the other, that to consult those which happen to be below, all the trays above must be taken out. The depth of each tray, besides, does not well admit of labelling, so that

like the rack-boxes, it is a case of trouble in searching for an object. It would be an improvement if each tray slid in on fillets, so that one might be taken out without disturbing the others, and by deepening the face labels could be placed outside. To do this would, however, spoil the compactness, and in part materially cripple their intended utility.

D.—Piper's original Portable Horizontal Slide Cabinet, as described by himself,* is composed of any number of flat cardboard trays, divided into six or more compartments, each holding a single slide in a horizontal position. The trays are enclosed in a strong millboard box, the front of which is made to fall down, so as to permit the trays to be readily withdrawn. When closed, an elastic band renders the whole firm and secure.

"It may be made of any desired capacity. Specimens are placed on the table capable of receiving from six to two hundred and fifty slides. The smallest is well adapted to contain a 'half-dozen series' of anatomical or other subjects; and its great strength, combined with lightness, makes it peculiarly available for transmission through the post.

"The one figured above (*l. c.*) is, however, that to which I would more particularly call your attention, being of a convenient size, and suitable for carrying in the pocket. It contains six trays, and will therefore hold three dozen slides.

"Among the advantages which may be derived from the cabinets, I will mention the convenience of displaying, at one view, the entire collection of slides, and the facility thus afforded for the selection of any required specimen, without the troublesome search and difficulty of removal frequently experienced with the old form of box, in which the slides are dropped (out of sight) into perpendicular grooves. It also prevents the possibility of the covers becoming detached by shaking about in transit, which is important when it is to convey a rare or valuable collection.

"The trays, being all of uniform size, may be transferred from one cabinet to another of larger or smaller dimensions, without necessitating the disturbance of the slides. In addition to its portability, it possesses the merit of cheapness, durability, and neatness of appearance."

The advantage of Piper's horizontal cases is marred by the trays resting on each other, and hence is only applicable to a very limited series of objects. The Eulenstein† collection of the British Museum, containing one hundred types of diatoms, is contained in a case after Piper's pattern, but larger, and for this purpose it answers very well.

* 'Trans. Micros. Soc. and Journ.,' 1867, vol. xv., 2nd Ser., p. 16.

† 'Diatomacerum Species Typicæ, Studiis,' Th. Eulenstein. Cent I. Stuttgartiæ, 1867.

E.—Mr. Henry George* brought before the notice of this Society a few years ago an inexpensive, compact form of store-box, wherein considerable ingenuity was displayed. The merit of his plan of store-box, or, indeed, small portable cabinet, lies in its being composed entirely of tin (japanned or otherwise), therefore of small compass, light, and not liable to warp; in the slides lying flat; and in a simple arrangement whereby the slides are kept in place without chance of overriding each other.

Each box is made to hold three or six dozen slides, or by increase of capacity to hold proportionally more. In that which contains seventy-two the outer casing is of oblong figure, 6½ inches long, 3¼ wide, and about 2½ inches deep. The lid is unhinged, and of ordinary form. The four sides of the box are each incised by a wide, deep semilune, so that the trays can readily be extracted. Each tray is a simple sheet of tin, out of which a large, long, oval piece has been cut, to ensure facility in taking up each slide. At the two farther extremities the tin is turned on edge, and forms a rest to the tray which lies above it. The opposite sides of the tray have their edges curvilinearly bent in, so that the slides slipping beneath are held firmly in place, alongside of each other. The slides thus lie secure, transversely to the long diameter of the box or tray, six in a row; and when one or more is wanted, by a tilting motion of the finger below the glass through the open space, extraction is easily effected.

The defects in this otherwise capital little case apply equally to those of Mr. Piper and the book-boxes, *viz.* if a specimen is wanted from the bottom row, all above have first to be removed. Again, while the labelling of each slide is readily seen on being raised, yet unless the entire contents are known, every tray has to be gone over before the thing wanted is to be found.

F.—I may refer *en passant* to Mr. Furze's zinc cases, the chief recommendation to which is their being of metal. Thus there is no liability to warp, as is also the case in the material used by Mr. George. These certainly have advantages over wood, which, unless mahogany, and that well seasoned, is so liable to warp, and render drawers stiff and troublesome to open.

G.—As a modification of the rack principle, Mr. Sorby (according to my friend Mr. David Forbes) uses a small form of box wherein the slides are ranged in rack, but instead of their lying tilted, each is placed horizontally.

A further extension of the same principle, and what are really most excellent capacious store-boxes (or to those who are satisfied with a moderate thing, a compact cabinet), cheap, portable, and

* The address of this gentleman is 65, Castle Street, Oxford Street, W. His invention has not found its way into the microscopic trade generally, but Mr. Beck, of Cornhill, showed me one specimen among his stock in trade.

each specimen of easy access, will be found in Mr. Norman's* adaptation. In a mahogany box, 7 inches high and 4½ broad, 150 slides lay in racks horizontally, and by a marginal number are easily got at and referred to. There is a diaphragm across the middle of the box, and another running up the centre longways. This gives four compartments, so many in each. A folding door at each end provides easy access, and, with a handle on the top, the box can be carried about anywhere.

H.—Mr. James Smith† has described and figured what he terms a microscopical cabinet, wherein the slides are arranged after the mode of some entomological collections. Shaped like a back-hinged book-box, on its being opened back the contained slides are all seen at a glance, being each retained in place by a double elastic band. If I am correctly informed, Prof. Hughes Bennett, of Edinburgh, uses a form of box or serial cabinet similar to the above. Other objections might be offered, but that the slides all rest vertically is against its frequent adoption.

I.—Lastly, as everything depends upon individual requirements, some wishing a small, others a larger case, it is hard to recommend one form of cabinet that will do for all. Piper's, George's, and Norman's, are each good in their way for small series, but a larger-sized cabinet of square form, with trays coming out separately, is the most preferable article. Mr. Beck sells a cheap plain kind, made of polished deal wood, wherein the trays are cardboard. In this, as in more expensive sorts, every specimen is of easy access. The latter lie flat, the numbers and name facing the observer; and labelling is provided for outside the drawer. A number of such boxes can be piled above one another, and by degrees an extensive cabinet ultimately attained.

It is now universally admitted that objects preserved in a moist medium are retained in a sound state longer and better when laid down flat. This is easily understood, for the finest and firmest cement is not always a safe protection when the slide is tilted edge upwards.

In the case of a cabinet with drawers, these are better not too deep; although some drawers of sufficient depth to admit easily the large-sized deep-celled slides are an essential desideratum. Unless in collections devoted to special subjects, deep drawers for the reception of larger objects need not be distributed throughout the cabinet; it is sufficient if they are confined to the lowermost tiers. In this way heavy specimens can be kept together, especially if exceeding in thickness the ordinary drawer's depth. The appropriate position of such objects in the general series can be replaced by a

* Manufacturer of microscopic objects, &c., 178, City Road, London, E.C.

† 'Trans. Micros. Soc. and Journ.,' 1860, vol. viii., 2nd Ser., p. 202.

dummy, or blank slide, numbered *seriatim*, and with a reference where its real counterpart is to be found. The latter, meanwhile, also bears its consecutive number in the series.

Entire sets of deep drawers having an additional shelf let in from the top I consider objectionable. These may, indeed, come in handy where a cabinet with a set of deep drawers, originally intended for another purpose, is converted into a microscopic receptacle. The microscopic cabinet of our Royal Microscopical Society has been altered in this fashion, and the available area of the double tiers consequently can contain twice the number of specimens they originally did in the deep drawers. It behoves, however, that a sufficient proportion of deep ones be retained.

The compartments of the drawers should admit of the slides lying with the narrow ends, or long diameter, fore and aft. In this way there is no chance of the slides overriding and, when the drawer is suddenly drawn out, injuring each other. In the event of a specimen, not too deep for the drawer, occupying a greater surface than the usual 3 inches by 1, provided it is not over 3 inches square, it may readily be placed transversely to the tray or drawer's direction. If it should be above 3 inches in diameter, the transverse bar or partition can be cut in such a manner that the slide shall occupy a double interspace. The end of the divided bar will prevent it moving sidewards. This simple plan, and such like trifling mechanical contrivances, are very useful in preserving unanimity in a series. They keep specimens in their proper classified position, instead of being scattered to a distance.

III.—*On Bichromatic Vision.*

By J. W. Stephenson, F.R.A.S., Treas. R.M.S., and Actuary to the Equitable Life Assurance Society.

(*Read before the* Royal Microscopical Society, *April* 3, 1872.)

It is probably well known to every Fellow of the Royal Microscopical Society that, by the aid of a double-image prism and a film of selenite, two images may be shown in the field of the microscope, the colours of which will be complementary the one to the other, and that when these images overlap, the resulting image will be, as far as the overlapping extends, of white light; but it is not, I think, so well known that when, by a suitable arrangement, different colours are made to occupy the two fields of a binocular, the resultant is a combination of such colours, and that if these are complementary to one another, the sensation of colour induced in the brain by the retina of one eye, is neutralized by that which reaches it through the instrumentality of the other, and that by the combination of the two the sensation of colour is entirely lost.

The purport of the present communication is to show how this phenomenon may readily be observed.

On the stage of the microscope before you, which I described at our last meeting, is a selenite film transmitting the green light of the third wave; this, in the normal condition of the instrument, of course fills both fields, but as the object on the present occasion is to produce different colours in the two tubes, I have introduced between the analyzing plate and *one* of the binocular prisms a film of mica of such a thickness that it will, according to the direction of its axis, accelerate or retard the transmitted ray half an undulation. By the use of this film of mica I have raised the original colour in the right-hand tube half a wave, and thus, whilst the original fine green of the third wave is transmitted to the left eye, the right is filled with the bright red higher up in the same scale. These colours are very nearly complementary, and it will be seen, on looking through the instrument with both eyes, that the field is practically white.

It will further be found that if, after looking at this colourless circle for a time, either eye be closed, a faint tint will gradually appear in the other eye, but very inferior in intensity to that which was originally experienced in looking down a single tube; and it will be observed that the diminution in intensity takes place through that eye which has continuously transmitted the same colour; although the quenching of the colour might be presumed to have ceased, when, by dropping the eyelid, the cause of its primary extinction no longer exists, this is not so; but must, to a certain extent at least, be consequent on the complementary tint having fallen on the retina of the closed eye.

The phenomenon which I have attempted to describe is not explained by the ordinary theory of stereoscopic vision, which, as given by an eminent writer, is, "that if the two images of the right and left aspects of a solid body be made to fall on the retinæ of the two eyes in such a way as to coalesce into a common image, they are judged by the mind to proceed from the single body, which alone, under ordinary circumstances, is competent to produce them."

In the case of two complementary tints viewed simultaneously through the tubes of the binocular, there is certainly a coalition of the fields into a common image; but it is clear that as no colour has been visible, the mind can have formed no *judgment* which can have conduced to such a result as we have seen is thus produced. May it not arise from the impression conveyed by the two optic nerves reaching a common nervous centre, so that the impressions from the two eyes are blended?

If over the upper half of the selenite above described a piece of mica of exactly the same thickness as that interposed between the prism and analyzing plate be cemented, but with its tension in an opposite direction, the colour of the light transmitted through the covered portion will be lowered from the green of the third to the red of the second wave, and as the upper and lower pieces of mica will (as far as they are superimposed) neutralize each other, the colours in the two tubes will be reversed; but with this difference, that whereas in the first instance we had the green and red of the third wave, we have now green of the third and red of the second, which are as before, very nearly complementary.

On examining the slide thus prepared, we shall pass, on moving it towards the observer, from fields entirely green and red, to others which are bisected by these colours and their complementary tints, rendering them respectively half green and red, and half red and green; these, again, will change on passing the slide onwards until the field which was originally green has become red, and that which was originally red has become green, but as far as colour is concerned, the brain will still remain totally insensible to the changes which have taken place.

I just now mentioned that if, under the circumstances then detailed, one eye be closed, the colour returns faintly; but it is a noteworthy fact that on doing so with party-coloured fields the colours are instantly reproduced, and, as it appears to me, with all their original intensity.

Numerous other experiments might be mentioned in which objects of divers colours appear as merely varying shades of black and white or grey when viewed in a similar manner; but to detail these would be practically a repetition of those already mentioned. Enough has, I think, been said to show how this phenomenon of "colour-blindness," if I may use the expression, may be produced.

IV.—*The supposed Fungus on Coleus Leaves; and also Notes on Podisoma fuscum and P. juniperi.*

By Henry J. Slack, F.G.S., Sec. R.M.S.

(*Read before the* Royal Microscopical Society, *April* 3, 1872.)

Plate XVIII.

A short time ago Mr. Reeves showed me a slide exhibiting a supposed fungus on a leaf of a Coleus plant, and said to grow *inside* its tissues. A brief examination led to a doubt whether the supposed fungoid objects were inside and not on the surface; and having a stock of the plants of various colours, an examination was commenced with a view to see how far the statements and opinions of two letters, written by Mr. Howse to the 'Journal of Botany,'* could be established.

It will be seen that Mr. Howse speaks of boiling the leaves in potash, and this will account for his results differing in every important particular from those described in this paper, and which were arrived at by a careful avoidance of any process that would change the character or apparent position of the leaf structure, or the supposed fungoid bodies.

In the first place a number of leaves were taken from Coleus plants of various colours, and carefully examined in their natural state, both by transmitted and reflected light. It became apparent that every leaf, whatever its age or tint, exhibited, chiefly if not entirely on the under surface, a number of globular bodies of a beautiful yellow colour, highly translucent and refractive, most of them marked with a cross like that impressed upon the well-known cross bun. These bodies differed in hue from any yellow of the leaf, and they were distributed pretty uniformly without any regard to the variegations of the leaf-colouring matter. From damaged specimens it was obvious they were the bodies alluded to by Mr. Howse. The colours of these bodies, when looking healthy, and well filled with their refractive matter, varied from rich topaz to a pale sherry tint, and they glittered like jewels when well lit up. Empty cells had a rude resemblance to a mushroom in form, with a stout stem and a round head marked with the cross, but the texture did not look in the least fungoid, nor could any mycelium be discerned in or on the leaves.

It was also found that leaves of last year's growth and those of this year had the same objects upon their under surfaces, and pretty much in the same state, except slight varieties of colour or occasional appearances of having withered. In no case out of dozens of leaves taken from many distinct plants, could anything

* Jan. 7, 1872, p. 23, and March, 1872, p. 72.

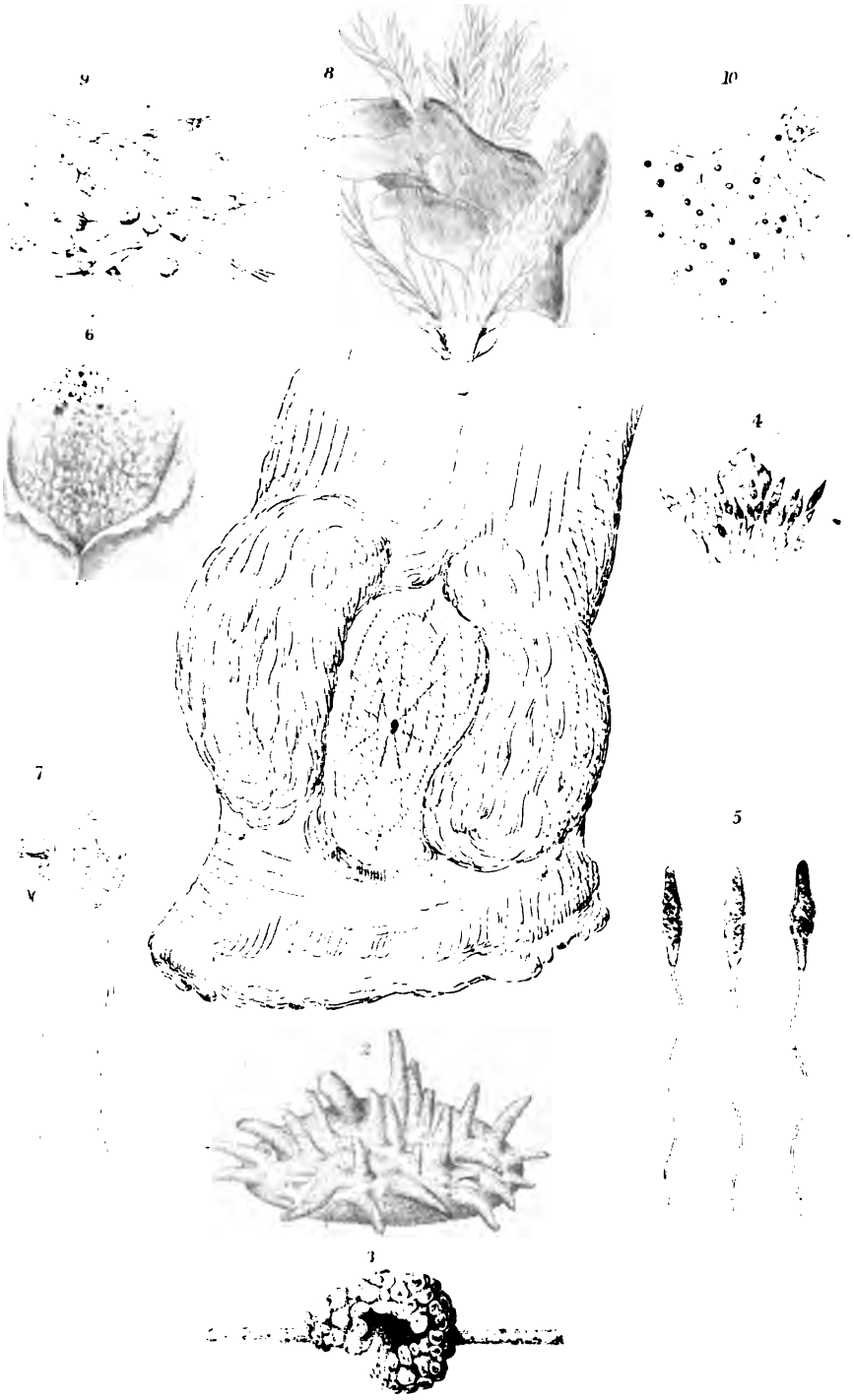
be discerned like spore formation or mycelium growth *inside* the tissues, though in one case something of the kind occurred outside.

On the 15th March a very young leaf, 5—8″ long and 3—8″ wide, was examined. This leaf was green with a dark centre. The under side was thickly set with the yellow bodies, which were distributed as freely on the green as on the dark part. The cells of the dark part when viewed by reflected light were found on the whole rounder and more prominent than those of the pale green parts. It was also noticed that the yellow bodies were not distributed at random, but ranged in something like a definite pattern with certain intervals between them. All of them were well filled, and nearly, if not quite, sessile. No stalked appearance was distinct in any leaf with the yellow bodies thoroughly filled and looking healthy.

On the 18th March, two fresh-grown young leaves were taken from another plant, the smallest being about 1—8″ long. On its under surface the yellow bodies abounded, quite as advanced in development as similar bodies on old leaves of last year's growth. Some of these contained distinct granules not unlike spores, and spore-like bodies were found on parts of the leaf. The healthy bodies were well filled and almost sessile. Dead ones showed the footstalk very plainly. On this leaf there was an appearance of mycelium, but not at all distinct.

The next observation was made on the 24th, the subject being a young leaf that had grown rapidly, and which was not quite old enough to open completely. This was carefully flattened out in a compressorium, care being taken to avoid injuring it by the pressure. Viewed by reflected light this leaf offered a most beautiful spectacle. It was full of colouring matter and fluids, and very hairy on its under surface, the hairs being developed more in proportion than the general tissue of the leaf. Amongst these hairs, glittering with fluids, some rich violet, and some white, were numbers of the yellow bodies, brimful of their coloured contents, many exhibiting the cross, and many in which it was not noticeable. They were extremely brilliant, of a fine topaz tint and high refractive power. They varied somewhat in size, a good plump one being about 1—800″, but some were smaller and some larger.

It seemed very unlikely that a quickly-grown and newly-opened leaf should have a fungoid parasite developed as well as every old leaf that was examined; and the regular arrangement of these bodies assisted the idea that they might be glandular structures belonging to the plants in a healthy and natural state. Somewhat similar bodies, but white, were found on the under surface of a leaf of garden sage; Mr. Reeves showed me others (white) on *Mentha viridis*, or garden mint; and unless it can be shown that the



Norman del. et Polyanthus
Hexadactylus Colens Lavate

yellow bodies on Coleus leaves go through some of the life processes belonging to fungi, it may be concluded that they are normal portions of the leaf structure, and not in any way parasitical. This will be the more apparent, as a note in 'Pereira's Materia Medica,' under the head of the Mint Family, speaks of similar bodies being recognized in many plants by German authorities.

If Coleus leaves are dried under moderate pressure, as between the leaves of a book, the yellow bodies shrink considerably in size, and lose some of their fine colour, but remain—at any rate for some weeks, the duration at present tested—sufficiently near their natural state to be distinctly recognized, and for their position outside the leaf to be in no doubt.

When Mr. Berkeley saw a slide prepared by Mr. Howse, he thought the bodies in question might be fungoid, and belonging to the genus *Synchytrium*. He remarked, however, that he did not know any fungus in which "the endochrome was so neatly divided." No such division could be seen in any of the leaves I examined, and it was probably caused by the potash treatment. In a recent letter to me, Mr. Berkeley expressed a doubt as to the fungoid character of these bodies; and as Coleus plants are very common, and grow vigorously in summer weather, or greenhouse temperature, Fellows of this Society can easily satisfy themselves on the subject.

Some Notes on Podisoma.—Having several specimens of the Carpet Juniper (*J. squamata*) and of the Irish Juniper—one annually infested with *P. fuscum* and the other with *P. juniperi*—observations on these peculiar fungi have been facilitated. On former occasions I have not been lucky in seeing the first form of these fungi, or what from this year's experience I take to be such. The stems they infest look gouty, with cracks in the bark, and somewhat suddenly, about April, masses of protospores crop out, yellowish brown in tint, darker in the case of *P. fuscum* and lighter in *P. juniperi communis*. *P. fuscum* forms broader masses, at first separate, but soon coalescing; while *P. juniperi* forms slender masses that swell but do not coalesce—at least that is their behaviour in the specimens to which these notes refer.

For a general account of this genus Podisoma, with the bibliography relating thereto, reference may be made to a valuable paper by Mr. Cooke in the 'Journal of the Quekett Club' (Jan., 1872). In this he says:—"If we take a portion of the orange substance which constitutes the fungoid parasite of the common juniper during the spring, and place it in a drop of water under the microscope, we shall observe that it consists of a multitude of brown bilocular spores, or spore-like bodies, with very long, transparent peduncles. These spores, for so they were long regarded, bear a striking resemblance to those of some species of Puccinia, with this difference, that they are imbedded in gelatine, whereas in Puccinia the clusters

of spores burst through the cuticle of the plant on which they grow, free of each other, and when more compact than usual not held together by a gelatinous medium."

This year, being on the look-out for the first outburst of Podisoma, it was noticed on the 29th February, on a Carpet Juniper not in a particularly warm situation, but the temperature was, as is well known, above the average of that month. A note made at the time says:—"Immense numbers of red or red-brown protospores burst through the bark in patches, each spore detached; no gelatinous enveloping mass. With 2—3″ objective looked like a multitude of garnet beads; with 1—5″ the two-celled character of each protospore and its granular contents became distinct." The protospores varied in size, and many of them differed somewhat from the typical form. Some were more elongated, others more squat; some had little nipples at the top, and others had smoothly-rounded tops. Amongst them were found a few trilocular cells, and one delicate crescentic spore-case with about twelve divisions.

Portions of this Podisoma, with the bark of the wood on which it grew, were placed on parchment paper and moistened. In a few days some jelly was formed, but further development did not occur, and it was soon covered with a white mould.

On the 2nd March the Juniper Podisoma was first seen on Irish juniper trees, without any gelatine. The outcrop was in much smaller patches than that of *P. fuscum* on the Carpet Juniper, and had the aspect and feel of little tufts of dry sponge. The biggest of this date were not more than 1—8″ long. Viewed as an opaque object, these tufts looked like aggregations of brown spun sugar threads; those on the surface projecting, each one distinct from its neighbour, the rest adhering close together, but no sign of jelly. With sufficient magnification the structure was seen, a two-celled protospore growing at the end of each thread. Portions were squeezed in a compressorium, and a multitudinous entanglement of mycelium threads disclosed, on which the protospores grew. The appearance of *P. juniperi* in this stage was not at all like the garnet-bead aspect of *P. fuscum*. Some multilocular cells were mixed with the characteristic cells of this fungus. Many of the protospores at this stage differed more or less from the normal forms.

By the 18th March *P. fuscum* had formed a good deal of gelatinous matter on Carpet Juniper, and a little on the Irish Juniper was formed by *P. juniperi*. By the 29th the fungi on Carpet Juniper had passed the stage of distinct development of each gelatinous mass; adjacent masses had become confluent, and all were in a soft, pappy state. Portions of this jelly showed under the microscope that great numbers of the protospores had germinated, and this had taken place with more irregularity of detail

than last year. Some protospores had four tubular projections, two from each side, each one springing from its own side of a cell division, and not always from exactly the same part. Some had only put forth little roundish vesicles, and many only developed tubes on one side, some only a single tube, the rest being abortive.

At this date (April 1) the Irish junipers show their fungi in a much less developed stage.

Mr. Reeves mentioned to me a case of *P. juniperi* that had formed large warty excrescences on a tree belonging to Mr. Wollaston, of Chiselhurst, and that gentleman has kindly forwarded me a sketch. It appears that the juniper on which this fungus grows had two leaders, and Mr. Wollaston cut one close off, leaving the usual cicatrix. On each side of this cicatrix the fungus grows, the gelatinous tufts springing annually from two bumps which the fungus has formed. Can this be regarded as an approximation to the habit of the American *P. macropus*, which makes peculiar galls, that surround the stems it affects, and which have pitted facets from which the spore masses spring? The occurrence of *P. fuscum* and *P. juniperi communis* in a non-gelatinous stage assimilates them to *Puccinia gymnosporangium*, which Mr. Berkeley informs me is separated from Podisoma by the gelatinous element of the latter.

EXPLANATION OF PLATE XVIII.

FIG. 1.—*Juniperus communis*; the swellings on each side towards the base produced by *Podisoma juniperi communis*. (Mr. Wollaston.)

„ 2.—*Podisoma juniperi communis* growing out of the swellings in Fig. 1.

„ 3.—Kind of gall produced by the American *Podisoma macropus*. The fungus grows out of the facets, as in Fig. 3—n. s.

„ 4.—Group of protospores of *Podisoma juniperi communis* bursting through cuticle, before formation of gelatinous matter × 135.

„ 5.—Protospores of ditto × 240, with mycelium stalks.

„ 6.—Group of protospores of *Podisoma fuscum* bursting through cuticle, before formation of gelatinous matter × 135.

„ 7.—Protospores, ditto × 240, with mycelium stalks.

„ 8.—*P. fuscum*—n. s.

„ 9.—Portion of very young Coleus leaf, with hairs and glands (yellow) having a cross mark upon them × 135.

„ 10.—Lobe of larger leaf, showing the arrangement of the yellow bodies × 60.

V.—*Optical Curiosities of Literature.*

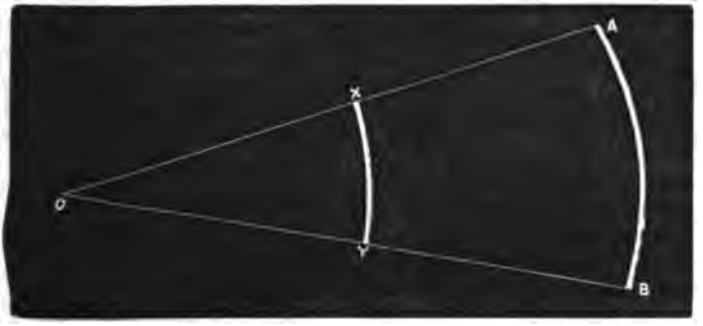
By the Rev. S. Leslie Brakey, M.A.

There is no class of artists which receives so large a share of respect and admiration from their employers as the professional makers of microscopes. Those who use these instruments are, above all men, interested in the excellence of the performance of which they are capable. They are incessantly examining and testing them, comparing their glasses with their neighbours' glasses, or the glasses of one maker with those of another maker. But the qualities they are in search of depend upon conditions which are, for the most part, entirely out of their reach. They can recognize the finest shades of performance when they see them, but are wholly ignorant of what they depend upon. As a necessary consequence the makers, who hold in their hands the secrets of these things, are looked up to with that peculiar awe which always attaches to the mysterious and unknown. With many workers this pleasurable emotion lasts to the end. I confess that, when fresh to the work, I have to some extent shared this feeling; but, less fortunate than some others, have experienced the painful sensation of having my eyes opened by degrees, and opened at last very widely indeed. Having been drawn on, as all workers are who really love their work, to follow out some special lines of investigation, and so to become dissatisfied with my apparatus, I used to make occasional visits to London for the purpose of having it modified. In course of this work I visited a great many different "houses." Acutely conscious of ignorance on various points concerning the structure of object-glasses, on which neither published books nor mathematical calculation could throw light, I endeavoured to "improve" these occasions by conversation, on the chance of picking up something which might throw light on the secrets of the business. On what curves depended flatness of field,—what was the difference in effect of single and triple fronts,—what was the connection between "actual" focus and angle of aperture,—these and such like were the things I sought to find out.

It was this spirit of inquiry which led to the disenchantment I have spoken of. Things occurred occasionally which began to raise my suspicions that opticians did not always themselves know what they were doing. Of my earlier experiences here is an example. Having some alteration to be made in a stand-condenser, an optician, well known to fame, incidentally observed that the glass was wrongly made. It was double-convex, whereas such a condenser "ought always to be plano-convex." It was quite essential; and he spoke with much severity of the carelessness of makers who used the wrong construction. I assented, in theory at least; and as he

offered to change the glass, a plano-convex was then and there fitted on. Presently observing, to my surprise, that in trying something, he had turned the round side to the object, I asked if he had any reason for preferring that side to the other. To this he replied that—yes—it certainly would work with the other side just as well; either side would do; there was no difference in that way, only it *must* be a plano-convex.

It was something of a shock to me to find that an optician of no obscure name did not know the meaning of a plano-convex lens; but this was only a beginning. After some more incidents of the same kind, I was destined to receive another shock very much more severe. In conversation with the head of another house, I happened to remark on the smallness of the mirror in a microscope that was standing near. The optician replied that—yes—it *was* small, unusually small perhaps, but—it came to the same thing in the end. The fact is, he proceeded to inform me confidentially, there was a great misapprehension about the size of mirrors: few—*very* few, were aware of a fact of which he had satisfied himself by demonstration, that the illumination was exactly the same, whether the mirror was smaller or larger. I expressed anxiety to know something more of it; and, calling for pen and paper, he drew for me the following figure:—



"Now," he said, "you see here is the large mirror A B sending in a pencil of light to the point O. Very well; now suppose a smaller mirror placed in this way, as X Y, you see how it comes to just the same thing; stands in exactly the same angle of light, and so sends in the same pencil."

With this the last remnants of my faith passed away, and I ceased to be surprised at anything. I might add some more curiosities of literature from my experience to these two, which were the first and the last; but their interest would depend on the names of the opticians concerned, which, for obvious reasons, could not be given. Those concerned in these cases were names perfectly well

known and familiar, though not in the very first rank. For, I ought to add, none of these incidents occurred in connection with any of the three historic great houses of which England is so justly proud, with all of which, so far as my experience has enabled me to judge, optics means something more than mechanical work.

The anomaly which so much perplexed me when new to the work, I believe that I can now perfectly account for. Optics, in a special sense, involves both principle and practice, the combination of the two being necessary at almost every step, so much so, that this department of knowledge cannot with propriety be called either an art or a science. But in certain cases the scientific part is reducible to fixed measurement and figures, and in this form can be handed over to the workshop. *Some* opticians have in this way acquired not the Science but the Art, working under others from whom they have received the figures; afterwards working the same figures on their own account. Their glasses are thus made *to formulæ*, the makers remaining in total ignorance of the "why" and the "wherefore" in which the formulæ originated. They work to these, and within certain narrow limits work well. It is only when something new is required, some modification of existing designs, that the deficiency is made apparent, and they are, in a literal sense, thrown out of their reckoning.

The curiosities I have been speaking of are not, however, by any means confined to the literature of conversation. Feats of learning which may well bear comparison with them are to be found in published works—works which have a name and a certain measure of authority. I will take an example from the treatise of Dr. Lardner in the 'Museum of Art and Science.' In vol. vi. the question of Nobert's lines is taken up, in connection with the reports of the juries of the Exhibition of 1851. The juries, it seems, had reported that while certain moderate powers sufficed to resolve the lower bands, to resolve the upper ones powers not only higher, but very much higher, were found to be necessary. Here the writer's calculating power discovers "a mistake." He counts the bands so as to find the proportion which the closeness of one band bears to the closeness of another. The higher band is, suppose, five times as close; therefore, a magnifying power exactly five times greater must, he says, resolve the higher band. For is it not plain, demonstrable mathematically, that this will present the lines so as to subtend the same angle which in the first band was found sufficient for their separation? And in fact, says the author, proceeding to generalize, we never need consult our microscopes at all to know what amplification will separate a given band. Ask how many lines it has to the inch, take the proportion of this to any band already resolved, and you have the required amplification at once; a simple sum in Rule of Three. Finally, the writer expresses him-

self as painfully surprised that the jury should have overlooked this short and easy method, and so fallen into the mistake; and is the more anxious to call attention to it at once, as he finds that unhappily the "mistake" has got copied into some microscopical treatises.

It would be hard to find now even a beginner who could read this passage without smiling. No doubt we are more familiar with these famous lines than observers were then; but allowing for this, and passing over any amount of contempt for the eyes of the jury, it still seems unaccountable that to one familiar with scientific methods, the possibility never suggested itself that the different conditions of microscopical from ordinary vision *might* introduce something which should modify the abstract calculation. But the possibility, if it ever suggested itself, seemed so remote as not to make it worth while even to take down the microscope to look.

Practical errors, however, are not the only errors in this treatise. There are others, which if less amusing are more discreditable, being things of pure science. Take, for example, the passage in vol. viii., where the author—perhaps someone else, and not Dr. Lardner himself—undertakes to show the *rationale* of the magnifying property of a simple lens. He premises the discussion by laying it down as a thing of primary importance, and specially so because there is "no subject on which more inexact and erroneous notions prevail." Having sounded this flourish, he proceeds himself to explain it, and explains it—*wrong*. An object brought nearer subtends a larger angle, and the lens enables the eye to see it when brought nearer, nearer than the customary ten inches;—this is what the explanation comes to. If the writer had ever happened to take out a pocket lens while reading and held it before the book, he might have observed that the print was magnified, although the distance remained the same; and might thus have been led to avoid the fate of himself increasing the list of "inexact and erroneous notions." Some more examples of the same kind may be found in vol. ix.

Not to go farther away, a number of remarkable flowers might be culled from the pages of the 'Monthly Microscopical Journal,' even at this early period of its youth. One of them will be found in a paper by Mr. Mayall on immersion objectives, in the number for February, 1869. Mr. M., who appears to have had an unusually extensive acquaintance with glasses of this kind, observes, as it were in passing, that in consequence of the refractive power of water an immersion glass never "has to do" with rays of greater obliquity than 48°. This observation, which is made with the easy air of feeling perfectly at home in the subject, has evidently been a source of much disquietude to readers of the article in after-times. Some correspondents manifestly had become convinced that there must be a mistake somewhere, though not able to say exactly where or

why. It was not perhaps to be much wondered at that an amateur worker should himself have made a mistake like this, or even that he should assert it in such a self-complacent tone. The unaccountable thing is that he should have passed over the startling discrepancy between his own doctrine and the professions of the makers he was eulogizing. For in the lists of Merz, Hartnach, and Gundlach, with all of which he is on familiar terms, the immersion apertures are given rising by degrees to 175°. But whatever may have been the origin or account of his error, Mr. Mayall has himself preserved ever after a judicious and most commendable reticence, following in this the well-known wisdom of the "leading journal," which, once committed to a misstatement, neither withdraws nor explains, leaving its readers to explain it for themselves as best they can.

But the choicest of the flowers by many degrees is that which was presented by Mr. Tolles, of Boston. The episode is too recent to have been forgotten, but, the discussion being ended, it may be regarded as now in some sort appertaining to history. It was on this wise. The original controversy about angular aperture having some time before come to a close, Mr. Tolles who, unknown to everyone, had been studying it, appears unexpectedly with a contribution which seemed to open up an entirely new phase in the question. Putting theory aside, he simply tests the fact by an experiment. And he finds that the disputed aperture *can* exceed the famous 82°, because in fact it does exceed it, being 110°. This certainly seemed startling, for facts are proverbially hard things to reason against, and Mr. Tolles, a practical worker, might be supposed to know a fact when he saw it. It appeared, however, that this was just what he did not know. Mr. Wenham instantly pointed out the fallacy; he had made a mistake in arranging the saddles for his horses, or, not to use metaphor, had *measured the wrong ray*.

So far the oversight was not unnatural, although one which a "professional" might have been expected to look at twice before committing his reputation by printing it. But a greater thing was coming. Having again reflected for some time, Mr. Tolles again appears, and this time also on a new ground. The experimental line having been found not to work, he now elects to win with Theory. Cementing to the front of his object-glass an imaginary hemisphere to match it, he forms an imaginary sphere, and then proceeds to reason upon it. As thus:—The rays can pass into this sphere below, and out of it above at every angle. And there is no difference if the hemispheres are connected, not immediately, but with the slide and immersion water between them. As before, the rays will pass in and pass out at every angle, and so the aperture has no limits at all.

This is the flower I have spoken of as being, all things considered, the most remarkable yet seen. You cannot, says the proverb,

have it in meal and also in malt. Mr. Tolles, however, having taken a good while to think of it, imagined that he could. The front of his object-glass he uses to pass the rays straight on, the refraction being neutralized with fluid. Then, oblivious of having used it up, he all the same expects the benefit of its action the other way, as a refracting surface collecting the pencil for the eye-piece.

I think that this may fairly pair off with the discovery of the man with the diagram of the great and small mirrors.

Mr. T.'s ingenious idea was disposed of by Mr. Wenham in a single line, by asking the question, "Do you expect to collect your whole aperture with the back combinations?" And after this it might have been supposed that Mr. T., in silence and meditation, had learned at length to avoid the pitfalls of this question. But from an incident which has since occurred I gather, with regret, that he is still in difficulties. A letter addressed by me to Col. Woodward, under the signature "B," concerning the aperture of his $\frac{1}{5}$, Mr. Stodder volunteered to answer (Jan., 1872). He replies for the four cases, immersed and dry, uncover'd and cover'd. In three of the cases the figures are no doubt correct; in the remaining one—the object immersed, uncover'd—prompted by Mr. Tolles he reports the aperture as 140°!!! Col. Woodward, who also courteously replied, also answers for the four cases; but, better instructed than his countrymen, on coming to the critical case he leaves out the figure, restricting himself to saying that, after a certain turn of the screw, the glass may now be used for objects immersed uncover'd.

I pass to notice some other passages remarkable in different ways, but not for the same kind of reasons as these. I insert them here for convenience only, and in no sense intending to class them with those already given.

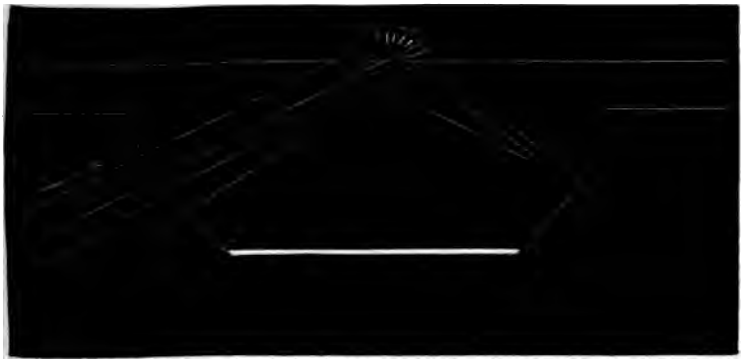
In a paper by the late President, the Rev. J. B. Reade, a sentence occurs which seems difficult to account for. Experimenting with his prism, he announces* that when it is turned so as to reflect light to the tube from its upper plane face the beam will be found to be polarized. This he gives as a fact so novel and startling, that he even anticipates it will be received with incredulity—"as a fable." Can it be that he was till then not aware that at a certain angle light is polarized by reflexion? He was before my time; but I have always understood that his scientific attainments, more especially in optics, were of a very high order. Yet in this passage he speaks precisely as Malus might have spoken on that evening when, sixty years ago, to his own astonishment he "vanished" the sun's reflexion from the windows of the Luxembourg with his doubly-refracting prism. It may be, however, and I should be glad if it can be shown, that the passage was meant to bear some other meaning, and that the mistake is mine.

* Vol. ii., p. 83.

In August, 1871, a communication appears from M. Mouchet on the thickness of glass covers. He there describes his method of measuring it, *viz.* by focussing for the upper and then for the lower surface, and observing how far the tube has been lowered, ascertainable by the index of the fine-adjustment screw. This method of taking soundings, as it might be called, is, I believe, discovered independently and used on all occasions by almost everyone who works intelligently with the microscope. It suggests itself, indeed, or rather forces itself on our notice every time we begin to work the screw. It is not, however, for this reason that I notice it, but to point out that M. Mouchet has by an oversight recommended an erroneous measurement with it. The distance traversed by the microscope is not the thickness of the glass. We must in every case correct this distance by increasing it in the proportion of three to two, due to the index of the glass. Of course so long as we only want a correction for the screw-collar the error is of no consequence; the proportion of this adjustment to the index of the slow-motion screw remains the same. But if we go outside this, applying it *e. g.* to the measurement given on some slides by the preparers, in absolute fractions of an inch, then the error will come out: as also when we use it, like M. Mouchet, to measure the absolute thickness of diatoms in balsam.

In the report of the proceedings of the Royal Microscopical Society, in the number for December, 1871, I notice observations of Mr. Slack and Mr. Brooke on the new lens introduced by Mr. Wenham, which do not correctly state its principle of action. There are two modes of action of which this lens is capable, and these in their nature and conditions are entirely distinct. In the observations referred to (p. 294) these two are mixed up together, so that the meaning of the illumination is lost. This may possibly be due to the necessary compression of the report, the observations having been made in conversation. Taking them, however, as they appear printed, they are not correct, and as they remain still uncorrected I notice them; principally, however, for the purpose of calling attention to the action, as yet it would seem so little known, of this most peculiar illumination. The persevering efforts which for some time have been made, as yet with no sign of success, to make out disputed structures with very high powers, are forcing us to pause and review our resources. We have in fact come nearly to a standstill, and the indications are, I think, becoming very plain that we can scarcely hope to "force the game," if I may so express it, by increase of power, or even by fineness of corrections alone. For in cases where, the definition being good, the magnification appears amply sufficient, the structure still refuses to reveal itself, and we come to a point where increased amplification yields no increase of knowledge. I have been for some time becoming more and more convinced that we must turn

our attention very much more than has yet been done to illumination *and its interpretation.* Things seen with high powers and large apertures are seen under conditions essentially differing from those of ordinary vision; so that in these realms it ceases to be true that seeing is believing. And when the best has been done for our glasses, we must still, I fear, supplement the discernment of our eyes very largely by the discernment of our reason And this of course involves chiefly the question of illumination and its illusions. At present the methods we possess are three—opaque, direct-transmitted, and dark-field. The lens in question has now added another which in nature and principle differs from all these, and supplies us with what I may call a new analysis of the objects as seen. But to use it so as to make it an analysis it is even more necessary than in the other methods that we understand the principle on which it works. I therefore append a diagram, which may



help to effect this by showing the action reduced to its most simple form. The lens and slide are here represented as forming a single piece, which optically they do (from the intervening oil). The object rests on the slide, touching it here only at a single point. The cover is omitted intentionally, having nothing at all to do with the work. It may no doubt be placed over the object, it is of no consequence at all whether or not; provided always that if placed no fluid is added between, and the object must still be, as here, on the slide. For simplicity therefore I omit it entirely. All the light which enters the slide is lost by total reflexion, except the ray which reaches the single point of contact. This ray is transmitted by entering within the object itself, and is there dissipated, more or less, according to its structure. This method therefore is distinct in nature from those before in use. Mr. Wenham has given no name to it. I have been accustomed to call it *Internal Illumination.* It is seen at once that it gives an analysis of the contact of the object with the slide. It might be thought the illumination would be symme-

tric, the field equably lighted; but this is not altogether so. It is so, however, to a greater degree than in any other illumination; and I have used it for the Binocular with an $\frac{1}{8}$th of large aperture with perfect stereoscopic effect. This, however, is not at all times very easy, the direction of the light sometimes making itself very evident; and I confess that I cannot as yet always control its action so as to reproduce effects before obtained. Moreover, in applying it to very fine objects, like Podura scales, where there is in parts almost, though not absolutely, contact with the glass, difficulties begin to appear, of theory too as well as of practice. The limits of the dissipation of the inner light have to be considered; and indications show themselves of conditions limiting the absolute inflexibility of the law of total reflexion, which require for their elucidation more knowledge of the laws of light than any student of optics, I fear, as yet possesses.

The other action of this lens I mention only to exclude it. It works with media only; the upper surface of the cover then reflecting down the light. It is not therefore new in its nature, but only an opaque illumination by a special method. It is also, to my experience, very hard to manage; and I do not expect much can be got from it. At any rate it has nothing to do with the diagram given above, or the remarks I have made upon it.

The preceding paper was written under the impression that the discussion in which Mr. Tolles was concerned had ended. From the March number of the Journal, received this morning, I find that this supposition was premature. Not dismayed by his two falls, he puts in yet another appearance, or rather two appearances. If Mr. Tolles has any enemies, which I hope he has not, they must rejoice that their enemy has written, not indeed a book, but four scientific papers. In these last two he has, as reviewers say of the latest work of a novelist, surpassed himself. It is in truth scarcely possible to discuss seriously the absurdities of these articles. That Mr. Tolles meant them seriously, there can, I think, be no doubt; but if he had meant them as a jest, a more prosperous one was never sent out. Changing his front once more, he thinks he now really at last has caught his aperture. In his own language he has designs, two at least, for its "procurement." The back combinations having failed him, he now has remedied the defect (on paper). His objects he will mount in the insides of little glass balls or *pillulæ*, which shall be quite separate from the object-glass. Then when the rays come out as before at every angle, he will look into these little balls with a complete object-glass, a glass with its own front, a glass of "three systems"—and so the thing is, as he says, "ended." We shall see. Suppose—it is an impossibility, a twofold

impossibility, but suppositions cost nothing—suppose a high-power glass could be made to reach the centres of the *pillulæ*, what becomes of the immersion? For to command the aperture, the glass must work *dry*. And if, changing it yet again, he designs another remedy by applying what he calls the "flowed-in-liquid," what becomes of the aperture? for as soon as the liquid is "flowed-in" the aperture is flowed out, and the inexorable 82° is upon him again.

The rest is in keeping. The best thing is where he quotes the experiment with the water-tank against itself. A $\frac{1}{12}$th in the tank showed 100°; and this being admitted he cannot for his life think why it does not end the case; for, as with touching simplicity he observes, 100° is surely larger than 82°. So it seems that after having had just half a year to think over the experiment, Mr. Tolles has never found out that in this case the object is in water as well as the objective; and that the same law which admits the higher limit for water, necessitates for the very same reason the lower limit for balsam.

In his new curves which form additional designs he is equally at fault. His diagrams are falsely drawn, and the lines do not give the refractions they are indicated as giving—a grave fault at any time in a professional workman, but altogether inexcusable where the very point at issue turns on the magnitude of the angles in question. Into this I need not enter, however, in detail; for though in one sense public property, it is in the first place the business of Mr. Wenham, who is the most competent to show its merits. Whether he will choose to do so is of course another question. Having twice set Mr. Tolles on his feet, he may not feel himself called on to repeat the process indefinitely. In any case I should not, for the present at least, have noticed it, but for the accidental coincidence of its coming in connection with the paper just written; supplying as it does a new and unexpected illustration of the extent to which skill of a high order in manual work may coexist with absolute ignorance of the principles on which the work depends.

PROGRESS OF MICROSCOPICAL SCIENCE.

The Subject of Binocular Vision is partly a microscopic one. We therefore notice a paper which we had intended to have reproduced, but which we now for various reasons are compelled to give up. It is by Mr. Joseph Le-Conte, Professor of Natural History in the University of California, and is really a very interesting communication. It enters upon a discussion of M. Pictet's views, which it disputes, and the author supports his views by some very ingenious experiments. The paper is of considerable length, and has been published in two or more numbers of 'Silliman's American Journal of Science,' to which we must refer our readers for further particulars. —*Vide Silliman's American Journal*, December.

The Development of Microzymas.—A claim to priority of discovery of the development of these organisms has been made to the French Academy.* M. Bechamp asserts, it is alleged, that the mycrozymes or Bacteria unite together, and thus form a cell. Now M. de Seynes asserts that this fact was pointed out by M. Pineau in 1845. In the number of 'Comptes Rendus' referred to, M. Bechamp says that he never asserted that the bacterias or mycrozymes unite to form a cell; on the contrary, he pointed out where they may become converted into Bacteria. He admits that Henle's Anatomy (1843) does contain some ideas like his, but they were put forward as mere speculations.

Spontaneous Generation.—On this subject, which has of late, through Dr. Bastian's publications, received so much attention, two lectures have been delivered in New York before the College of Physicians and Surgeons, by Professor J. C. Dalton, M.D. These give a useful summary of the various experiments that have been tried in the several European countries and in America during the last two hundred years. The lectures are full of interest, and extend over forty pages of the 'New York Medical Journal.'† We therefore commend its consideration to our readers. The author seems more opposed to spontaneous generation than in favour of it, as may be seen by the following concluding remarks:—"Thus," he says, "we find that now, as always, the idea of the spontaneous generation of living beings is confined to organisms of which we know the least. Exactly where our definite knowledge fails, owing either to the minute size or the imperfect organization of these bodies, there commences the obscurity which hangs around their origin. It is very justly said, in support of their spontaneous generation, that, if this mode of production exists at all, it is precisely in the case of the simplest and most imperfect organisms that we should expect it. We might imagine a bacterium or a monad to originate in this way, but not an eagle or an elephant. On the other hand, it is alleged that the imperfect organization of these minute forms is only apparent, and depends on the imperfection in our means of observation. When our microscopes and other aids to investigation have been still further improved, we shall find, it is said, that the bacterium and the vibrio possess an organization of their own, not less essential and complete in its way than that which we

* 'Comptes Rendus,' Feb. 19, 1872. † February, 1872.

now know belongs to the ciliated infusoria. There is every evidence that at least their regular and normal mode of production is from germs disseminated in the atmosphere; and they themselves, as we have already seen, are embryonic or transitional forms in the development of a distinct vegetable growth. They are consequently to be regarded as an integral part of the cryptogamic vegetable organizations; and, notwithstanding the apparent simplicity of their structure, they no doubt, like other plants and animals, have their definite place in the organic world."

NOTES AND MEMORANDA.

Papers "standing over."—Owing to the pressure on our space, we regret being compelled to allow papers by the following authors to stand over:—Dr. Lionel Beale; Dr. Braithwaite; Dr. R. H. Ward; Captain Hutton; Mr. Isaac Roberts; and Mr. P. Braham.

"Pattern Lead" for making Cells.—Mr. White, the secretary of the Quekett Club, and one of the members of our Council, stated at a recent meeting of the club that for many years he had been in the habit of using cells made of a thin kind of lead known as "pattern lead," which was used by dentists for taking patterns for their gold plates. It would be found to answer the purpose very well, and had none of the objectionable qualities mentioned by Dr. Matthews (in his observations at same meeting), since the slide might be made almost red hot without melting the cells, and the cells were very easily stuck on with marine glue. For shallow cells a simple ring of gold size, and gum dammar put on thickly and allowed to get hard, answered the purpose very well, and if Bastian's cement were used instead, the cell could easily be built up higher by adding layers upon those which had become dry. Another way was to use the zinc cells, which would stand any amount of heat; acid, however, would affect these, but vulcanite cells would resist acids. In making cells for mounting in fluid, it would be found of great advantage to set up some standard size, and keep to it, as this would enable the worker in a short time to estimate correctly the exact amount of fluid required for filling—a matter of very much importance.

Colonel Woodward's Paper in the April Number on Tolles' Objective $\frac{1}{18}$th Immersion.—Colonel Woodward has requested us to add the following observation to his last paper, which, however, has already appeared in print:—"Since writing the above paper I have made photographs of the two frustules shown by the Wales' lens, with a lens of Mr. Tolles' (a $\frac{1}{18}$th immersion), which I think fully equal to that by the Wales' objective. I may add that any of these objectives, including the Becks' $\frac{1}{10}$th, will resolve *Amphipleura pellucida* in balsam, as, in fact, was done by Count Castracane with objectives by Hartnack and Nachet.* Count Castracane made photographs of the balsam-mounted specimen on Möller's type plate, and counted the striæ."

* See this Journal, April, 1871, p. 176.

CORRESPONDENCE.

The 'American Naturalist' and Mr. Wenham.

To the Editor of the 'Monthly Microscopical Journal.'

Sir,—Seeing in page 178 of last Journal your notice quoted from the 'American Naturalist,' I ask for a brief reply—not in the way of controversy, for the insignificance of the case does not call for this, and the writer, C. S., may remain under cover of initials, but merely to correct a misstatement, that I wrote "a paper in reply to one of Mr. Bicknell's." I did not commit myself to such an extent; all that I said in reference to him was included in *one paragraph of fifteen lines* * as a protest at what I called an uncourteous remark against Colonel Dr. Woodward, whose name is now familiar to microscopists for his valuable and practical discoveries in a most important and interesting department of the science.

Accusing Colonel Woodward of wilful deception, and Messrs. Powell and Lealand inclusive, I think is generally to be admitted as something more than uncourteous, and so near to an *unwarrantable personal insult*, that it would have come with a far better grace if they (C. S. and Mr. Bicknell) had given some plea or atonement for expressions, if hastily or inconsiderately written, than to expect *me* to "apologize" for what they term "practically defending the imposition"!! No one knows better than myself the difficulty of adopting a nomenclature by the different makers that shall exactly denote the power of all the highest object-glasses sent out. Every user must do this for himself, and so it will ever remain; and as one witness on behalf of at least the most respectable portion of the body, and holding in scorn the abuse of some whose opinion is evidently not worth caring for, I finally venture to affirm that no imposition is intended.

Yours faithfully,

F. H. Wenham.

PROCEEDINGS OF SOCIETIES.†

Royal Microscopical Society.

King's College, *April* 3, 1872.

Dr. Millar in the chair.

The minutes of the previous meeting were read and confirmed.

A list of donations to the library and cabinet was read, and a vote of thanks passed to the respective donors.

Mr. Stephenson read a paper "On Bichromatic Vision."

Dr. Pigott was much delighted with Mr. Stephenson's description

* *Vide* page 292, 'M. M. J.,' Dec., 1871.

† Secretaries of Societies will greatly oblige us by writing their report legibly—especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. 'M. M. J.'

of this very subtle process of regulating the thickness of the waves of light as applied to the binocular. He thought Mr. Stephenson had very perspicuously explained the action of the coloured fields which he employed. He (Dr. Pigott) had not tried any experiments of the kind, but the ideas set forth by Mr. Stephenson were very suggestive.

After referring to the effect produced when one is looking through a pair of spectacles, and the two rims are noticed or not, Dr. Pigott continued: Suppose that the two glasses were of different colours, what would be the effect upon the eye and brain? He imagined that if they were complementary colours, they would be united by the brain, and that if we were looking at snow we ought to see it tolerably white. He thought that Mr. Stephenson had touched upon a new point in optical science, which might be developed with very great utility. The ingenuity of the whole thing struck him with admiration. Looking at the very complicated manner in which rays of light are refracted and reflected by internal reflexion, and according to the position of the prism, it was wonderful that such good effects were obtained.

Mr. Slack said there was one point in Mr. Stephenson's paper which seemed to require explanation. He referred to the portion of it which treated the effects described as having a character distinct from stereoscopic binocular vision, which it really resembled very closely. Mr. Stephenson explained that the remark alluded to was not his own, but a quotation. Mr. Slack proceeded to point out the resemblance between the combination of two perspective views, and two complementary colours. In both cases it seemed that the perceptive portions of the brain acted upon the combined images.

The Chairman said the interesting point in Mr. Stephenson's paper was that in which he referred to the fact that in each retina there was a different colour, and yet it was seen combined into one colour. He did not remember in any work the subject being treated in the same way as Mr. Stephenson had treated it.

Mr. Slack said he should like to suggest an experiment, *viz.* to try with all sorts of persons. It was known that there were very few people who had two eyes exactly focussing alike, and probably it would be rare to find a pair of eyes in which the chromatic corrections were the same. It would be very interesting to notice what would be the different effects the same images produced upon persons whose eyes had different corrections.

Dr. Pigott said he had met with the case of a gentleman who, looking through his microscope, said he could never distinguish a *red* colour in the instrument. It always appeared to him more like black and white. He never saw anything *red*, but objects that appeared *red* to other persons were black to him.

Mr. Ingpen referred to an experiment tried with the stereoscope, in which tinted glasses were used, having complementary colours. The result was sometimes combination of colours, and not unfrequently alternation of colours. The alternation might be attributed to the fact that the eyes got alternately fatigued by each colour.

Mr. Slack read a paper "On the Supposed Fungus on *Coleus* leaves," and "Notes on *Podisoma fuscum* and *P. juniperi*."

Mr. Howse said, having re-examined many specimens, he was now quite of Mr. Slack's opinion in reference to the supposed fungus.

Mr. Slack said he had suggested that probably the treatment with potash was the reason that there was the fungoid appearance. The best way to view such things is in their natural state.

Mr. Howse had treated them with potash that he might see whether the bodies were outside or inside; they seemed below the cuticle.

Mr. Slack said the little globes are above the cuticle in its natural state, but if the cuticle was swollen by chemical means they might appear inside.

Dr. Braithwaite thought that Mr. Slack was quite correct in the inference he had drawn. The Coleus plants expelled a fœtid odour something like the Lamium, and the yellow glands might evolve it.

Mr. Slack said in reference to Podisoma he had noticed a curious appearance in one of his slides, in which he had put by a portion of the gelatinous matter that he had alluded to without any preparation whatever, just to keep it free from dust in a perfectly natural state; when it began soon to develop one of the moulds. The same form of mould grew upon a specimen which he had kept upon a parchment paper and rice paste. It exhibited little black globes at the tips of white threads. When the black globes were squeezed they broke up into a number of little spherules which acted as spherical lenses. When aggregated together, these highly-refractive bodies presented an appearance like a black pin's head.

The meeting then adjourned to the 1st of May.

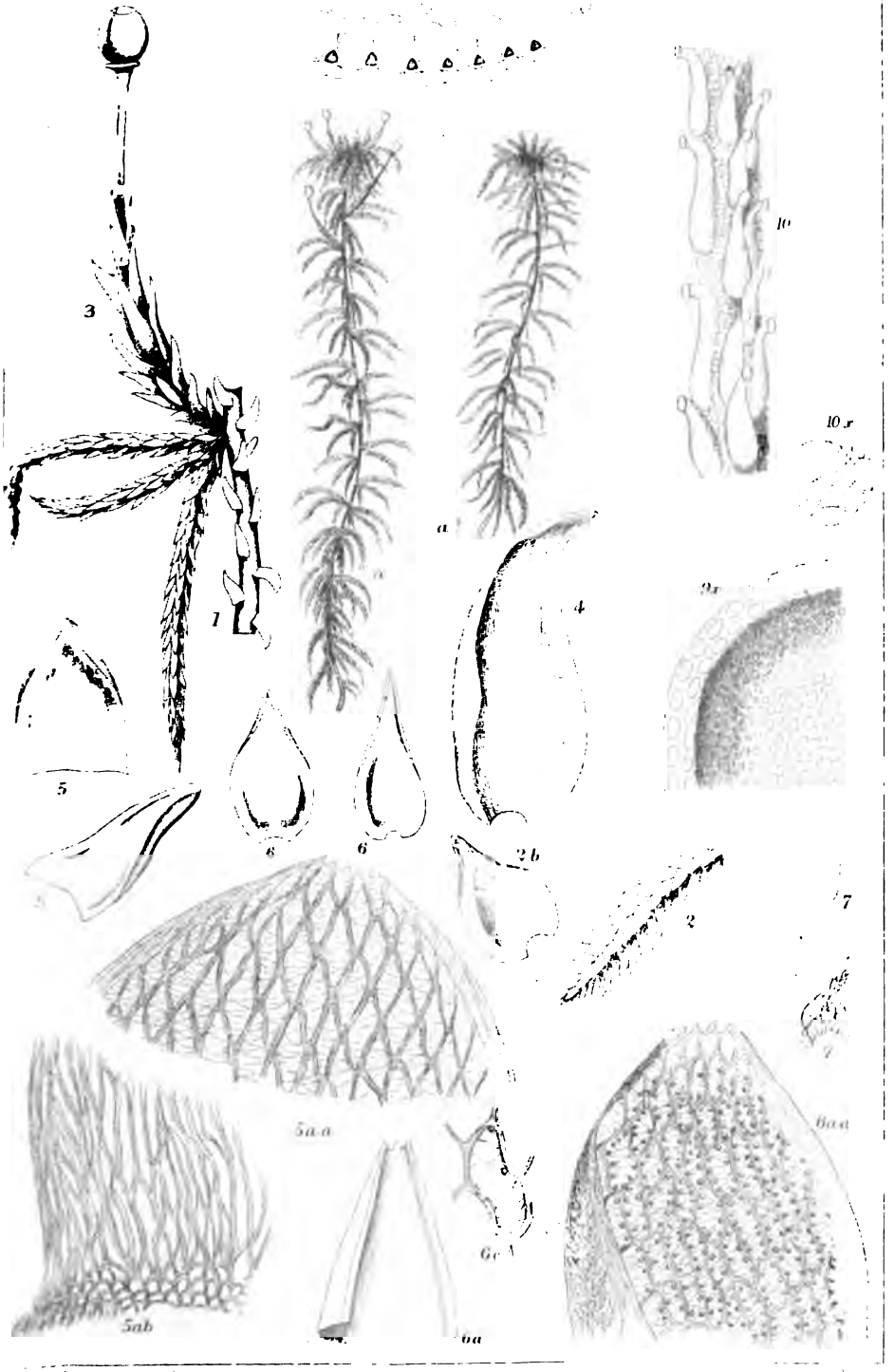
Donations to the Library and Cabinet, from March 6th to April 3rd, 1872:—

	From
Land and Water. Weekly	*The Editor.*
Nature. Weekly	*Ditto.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal. Weekly	*The Society.*
Journal of the Quekett Club, No. 18	*The Club.*
Journal of the London Institution, No. 13	*The Institution.*
Two Slides of Battledore Scales	*Dr. J. Anthony.*
Twelve Slides of Podura Scales	*S. J. McIntire.*

The following gentlemen were elected Fellows of the Society:—

B. D. Jackson, Esq.
Francis Fowke, Esq.

WALTER W. REEVES,
Assist.-Secretary.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THE

MONTHLY MICROSCOPICAL JOURNAL.

JUNE 1, 1872.

I.—*On an Improved Reflex Illuminator for the Highest Powers of the Microscope.* By F. H. WENHAM.

(*Read before the* ROYAL MICROSCOPICAL SOCIETY, *May* 1, 1872.)

SOME retrospect may be needed to explain my reappearance before you on a subject which by this time may perhaps be considered threadbare and exhausted, but the very singular effects of the improved arrangement will, I trust, exonerate me from all undue intrusion.

It is now sixteen years since I described in a paper read before this Society (March 26, 1856) a mode of illuminating objects under the highest powers of the microscope. As then explained, this was strictly an opaque illumination, intended only for such objects as were mounted in balsam or fluid, as the intermedium permitted the slide and cover to be treated as an entire refractive plate, the upper plane of which, or cover, could thus be used like a speculum by throwing light down again from it as a total reflecting surface on to the objects beneath.

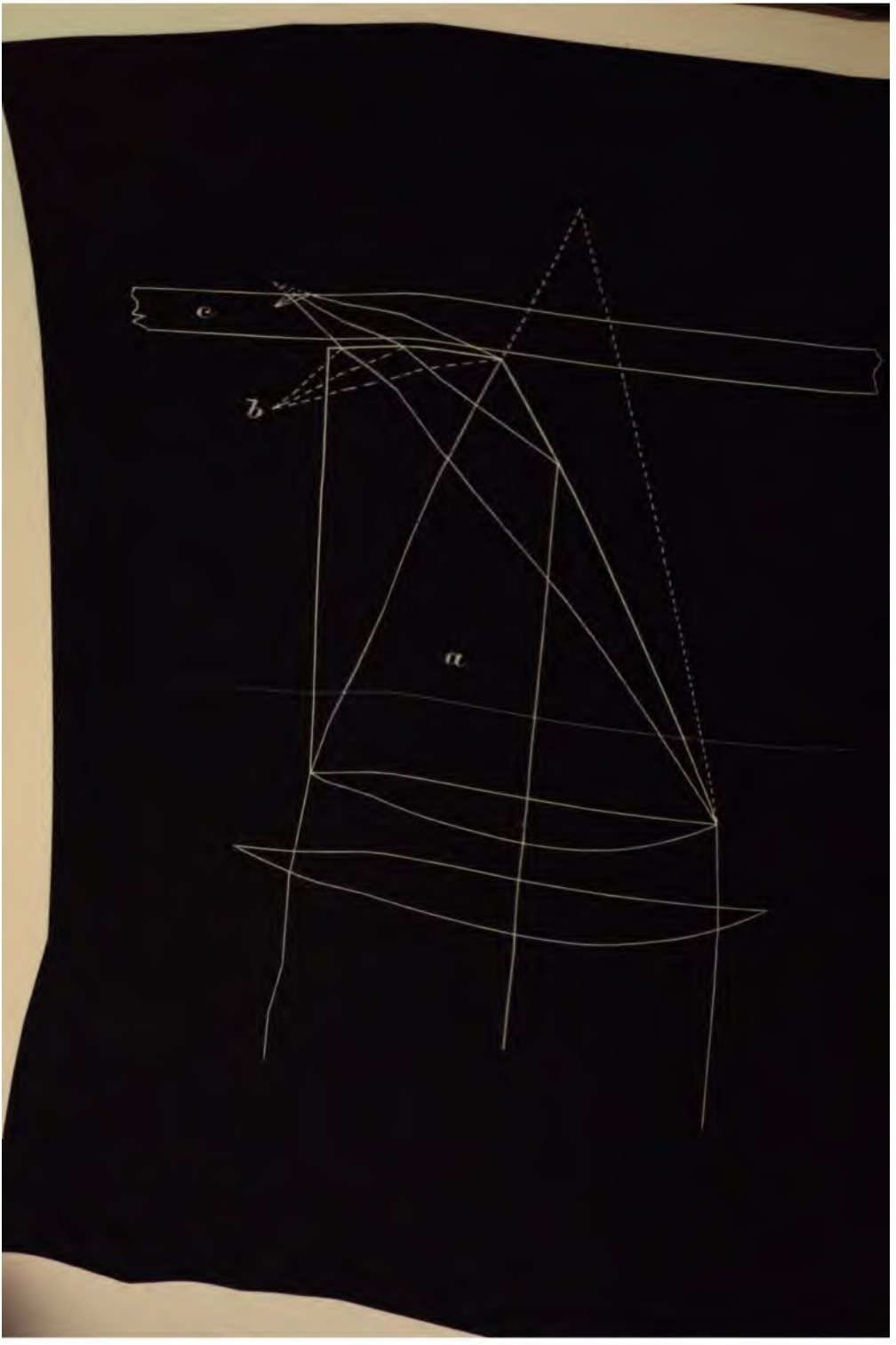
Several methods were figured and shown at the meeting for allowing rays above the angle of 41° to pass direct to the cover, through which of course they could not escape, but by the law of total reflexion would be thrown back again and intercepted by underlying particles or objects.

One plan was to patch on a right-angled prism by its long side with water, to the under surface of the slide, and then condense the light through either of the faces by lens adaptations. Another arrangement was by means of a solid glass parabola with a flat top, on which the slide was laid with water. This is now revived under the name of the "immersion paraboloid." The one that I then made I possess still, and employ occasionally. But the plan then preferred and in use now, without the slightest modification, is the combination of the ordinary parabola and the truncated lens, as this gave far more light than any of the others. The method has not been much used, as the number of objects in fluid or balsam capable of intercepting light as strictly opaque objects were remarkably

limited, and of course all structures that could not be seen in balsam, such as many butterfly scales, were quite excluded. I may remark also that immersion object-glasses (which, however, were not known or employed at that time) were not available, as the water between the cover and lens would prevent the reflexion, and the object could not be seen opaquely. I was not then aware of the possibility of viewing objects in a dark field with the largest apertures when mounted *dry*. This was an accidental discovery made a few years afterwards. Happening by mistake to take up my *Podura* slide and place it on the immersion parabola, I was surprised to perceive a few of the very numerous scales that I knew were there, shining out with great brilliancy, and found that these were the ones in contact with the slide itself; all those on the cover were invisible. It thus became evident that the effect was due to the interruption of total reflexion from the slide, the mere contact or presence of the object thereon attracting the light, or allowing it a passage through. That the object should under these circumstances collect light enough to be seen under high magnifying powers, could not have been anticipated; but so great is the intensity, that Col. Dr. Woodward has obtained some fine photographs of objects illumined this way. Except to friends at hand, I gave no special description of this method. The first notification of it appeared in one of my discussional communications to the 'Monthly Microscopical Journal' for July 1, 1869. Being usually either too indolent or indifferent to make public demonstrations in order to disseminate my notions, I merely publish them and leave them to be appreciated by the public or not according to their merits. Perceiving that many of our most eminent observers now use the truncated lens and parabola for this curious principle of illumination, I have exerted myself to improve upon the original plan, now sixteen years old, and which I must confess is in many respects a most clumsy and unhandy combination, and so difficult to manipulate, that experienced microscopists sometimes fail to show satisfactory results. In the first place it is difficult to patch on the truncated lens in the exact spot under the object you wish to observe. You have to shift it about several times before you can do this. After much trouble having got everything right for one object, you wish to have a look at a neighbouring one, you have to traverse the slide—lens and all—which will thus be set excentrically both in the parabola and relative to the object, and then to get a passable view you are compelled to effect an imperfect compromise by shifting either the mirror, the outstanding bull's-eye lens (which is always required for parallel rays), or by working the parabola up and down, and worst of all, if particles of dirt or *diatoms* are thickly strewn over the slide, they all become so brilliantly luminous, that the whole field is flooded with blue and white blurs, which greatly injure the definition by

impairing the sensitiveness of the eye. There is no possibility of *confining the light exclusively to the object to be viewed*, which is an important point. In this last respect my immersion paraboloid is not better, and also requires the bull's-eye for parallel rays.

The diagram, five times the size of the original (see p. 240), illustrates the plan that I have arranged to overcome these defects. *a* is a cylinder of glass half an inch long and four-tenths in diameter, the lower convex surface of which is polished to a radius of four-tenths. The top is flat and polished. Starting from the bottom edge, the cylinder is worked off to a polished face at an angle of 64°: close beneath the cylinder is set a plano-convex lens of 1¼ focus. Parallel rays sent through the lens, after leaving the lower convex surface of the cylinder, would be refracted to the point shown by the dotted lines if continued in solid glass, but by impinging on the inclined polished surface (which is far within the angle of total reflexion) they are thrown on the flat segmental top; here they would be totally reflected and beaten down again to the point, *b*, outside the cylinder; but if an object-slide, *c*, be laid over the flat top with an intervening film of water, the rays proceed on till the focal point reaches the upper surface or is slightly beyond it; here total reflexion now takes place; all the light is concentrated to a minute spot in the centre of the field of view of the microscope, and most of the rays are available for any object brought there by traversing the slides over the water top of the illuminator, which must be kept full without allowing any to run down the reflecting surface. It will be seen, in order to get the focal point in the centre of the microscope, that the lens centre must be excentric, but this does not involve the slightest inconvenience, as the excentricity only amounts to a little over two-tenths of an inch, and is so small that the same adjustment of the mirror serves during an entire revolution. The apparatus rotates on the focus as a centre. The management of this illuminator is very easy and simple; its fitting goes into the ordinary sub-stage, and has an independent rotary movement of its own, like that of Nachet's prism. The cylinder is brought up nearly level with the stage. The centre of rotation is set true by a dot on the fitting seen with a 1½ object-glass. A drop of water is then placed on the top, upon which the slide is laid. The required objects *on the slide* are found by a low power, and may be distinguished by their brilliant appearance, while those on the cover are nearly invisible. The light is thrown up by either the plane or concave mirror. The former is generally the best and most controllable. The lamp should not be placed much beyond the stage, else its direct rays will get underneath and mar the blackness of the field. Having got the best effect, say on a *diatom*, or insect scale, by tilting the mirror, we now proceed to rotate the illuminator. During this the most exquisite unfolding of structure


c
b
a

takes place, opening out as it were into detail the form of bosses or ribbings. On that superb test, the *Podura*, for example, when the light is thrown from the apex to the quill, the whole scale is dotted over with bright blue spots laying in a zigzag direction; these are the most prominent parts or the club-end of the markings, which are nearest in contact with the glass. As the light continues to revolve, these gradually arrange themselves in the usual note of exclamation figures. In the reverse direction, or from quill to point, the blue spots are rather less distinct, and the object is most brilliant when the light is transverse to the scale. By making the light as oblique as possible in a direction from the point to the quill, I have for the first time been able to get a satisfactory view of the surface tissue of the *Podura;* it appears of a pale brown colour in waves or transverse undulations which reflect light visibly, and follow the zigzag of the markings. I need not, however, at present describe effects that will soon be known, further than to remark that *Amphipleura pellucida* assumed a substantial appearance, not seen in any other way, and at once displayed its striæ with an $\frac{1}{8}$th that had never resolved them before. As partly accounting for these effects, the black field has a material influence, and it has been long known that the resolution of very difficult striæ depends upon excessive obliquity of light. Now the most oblique that we can obtain by ordinary means, cannot strike the object at an angle greater than 41°, if it is either in balsam or on the slide, but on this principle we are dealing with rays entirely beyond this angle.

I will venture to anticipate a few objections that may be made to the arrangement. One might be that it is not achromatic. This would increase the expense, and I do not think will have the smallest advantage. The light spot is now so definite that its margin can be brought half across an *Angulatum* scale, at which there is a chromatic effect produced. This is by no means detrimental—quite the reverse, as some fine appearances are produced by it.

It may be said that the rays reflected from the lower end of the facet are just without the angle of total reflexion, and might enter true, and I had intended to stop off a small segment of the lens at this place, but found it so desirable, in many objects, to admit a little light, that I preferred it without alteration. It is easy to get a black field in all cases by mere mirror adjustment.

Also it might be objected that the light is always one-sided, which in many objects is well known to produce a host of false appearances; but this does not apply in this principle of illumination. It seems sufficient to get light into the object alone, whether sideways or not, so as to make it appear as a self-luminous body; and there is none of that coma thrown off like an overhanging veil, or

ghost object, long known as the "diffraction spectrum." I will, however, try another combination to act just opposite, and so make a double-barrelled affair of it, though I do not expect to gain much except in increase of light, and this is scarcely needed. Finally, it might be urged that we do not secure near the same average amount of obliquity of the rays as with the parabolic condenser. This is correct in appearance but not in effect, as there is a reason why the most oblique rays on this system of illumination are scarcely available. It is those nearest to 41° that are the most serviceable.

In my paper of 1856 I gave a diagram, showing that the use of water limited the angle to 160°. In order to secure the extra 20°, I recommended the use of oil of cassia or cloves for patching on the truncated lens; but the Rev. S. L. Brakey, who has made investigations on this principle of illumination, informed me that he found that it caused not the slightest difference whether water or a more refractive oil was used. Instead of waxing wroth at what seemed to demolish my pretty bit of theory, I accepted it as a fact to be reasoned upon, particularly as it allowed some latitude for improvements, and soon found that for very oblique rays the objects to appear luminous must be in absolute, or, indeed, in strong adhesive contact with the glass, otherwise these rays would not enter them; and as we now deal so considerably in the principle of total internal reflexion in microscopes and their appliances, I will finally submit a few remarks on the theory for further consideration.

When a ray of light falls on an internal surface of crown glass at an angle of near 41°, it can no longer be refracted outwards; its passage coming parallel with the plane surface, the whole is reflected back again at an angle equal to that of the incident ray. For monochromatic light, the limiting line between total reflexion, and transmission, or refraction, is extremely definite, but for ordinary light not so, as the coloured rays are refracted and pass in the order of their refrangibility. At about 40° a colour bow is seen across the prism, caused by the reflexion of a portion of the successive rays of compound light, but even then the boundary is very narrow, and comprised within an incident angle of one degree five minutes, consequently in all total reflecting arrangements the incident angles of the rays must exceed 40°. It is a fact well known to all those conversant with optical apparatus, that total reflecting surfaces must be kept clean, like those for direct transmission; any particles of dust, &c., on the former will abstract a portion of light from internal reflexion. It is exactly on this principle that the method of illumination described in this paper is based, but it appears to involve some conditions which do not hitherto seem to have been thoroughly investigated. I must call attention to an error which has been handed down in most optical

works up to the present time, which state generally that when a ray of light proceeds from a denser to a rarer medium, such as glass to *air*, beyond angles given, total internal reflexion will occur. But the fact is, that *air* has nothing to do with the phenomenon; its refractive power is so very small that it can only be discerned by dealing with enormous masses, and has been accurately determined by the difference between the real and apparent position of celestial bodies seen through the whole stratum of our atmosphere. Any portion of the back of a total reflecting prism that has the air withdrawn, by being set on a vacuum tube, will not show any difference at the spot where the air is absent; and if the whole prism is placed under the receiver of an air-pump, total reflexion will continue precisely as before: and as also bearing on the question before us, I may add that "Newton's rings," or the coloured bands seen between contiguous surfaces of glass are not caused by films of air, they are the mere effect of interval upon the colour undulations of light, considered upon the wave theory. When all air is withdrawn the colours are still there. I allude to this as not being altogether foreign to the present question, and to show that in what are deemed total reflecting surfaces there is some influence on light extending beyond that surface, and that external objects placed thereon *do not require to be in absolute contact* in order to abstract light. For very oblique incidences they must be much nearer, as I have found that the rays within a few degrees of 90° will not touch objects such as *diatoms*, but pass beneath them. At 41° the objects may be seen when they lie quite loose on the surface, and hence the reason why the least oblique rays are the most available. Sir Isaac Newton leaves it to be inferred that his colour rings are caused by air, and also that the same element performs its part in total reflexion. After he ends the experiments in the first part of his Optics, fourth edition, so accurately conducted and clearly expressed, towards the end in Book III. (which consists mainly of queries) there is the following remarkable passage in question 29, page 346, relating to total reflexion, which I will venture to quote:—"The rays of light in going out of glass into a *vacuum* are bent towards the glass, and if they fall too obliquely on the *vacuum*, they are bent backwards into the glass and totally reflected; and the reflexion cannot be ascribed to the resistance of an absolute *vacuum*, but must be caused by the power of the glass attracting the rays at their going out of it into the *vacuum* and bringing them back, for if the further surface of the glass be moistened with water, or clear oil, or liquid and clear honey, the rays which would otherwise be reflected, will go into the water, oil, or honey, and therefore are not reflected *before* they arrive at the farther surface of the glass, and begin to go out of it. If they go out of it into the water, oil, or honey, they go on because the attraction of the glass

is almost balanced and rendered ineffectual by the contrary attraction of the liquor; but if they go out of it into a *vacuum*, which has no attraction to balance that of the glass, the attraction of the glass either bends and refracts them, or brings them back and reflects them."

In this sentence, the word "*vacuum*" is in italics, as if the author doubted the influence of air, and both here, and subsequently, speaks more definitely of bodies in close proximity to the reflecting surface "attracting" the ray of light.

I must ask to be excused for this episode, on the plea of a desire to consider and discuss any obscure point in the principle under present attention.

Addendum.

Wishing to ascertain the distance at which objects on a total reflecting surface were rendered visible by light transmitted in consequence of their presence or contact thereon, I yesterday tried the simple expedient of Newton, and pressed a lens of long radius, on the back of a right-angled prism, in order to measure the distance by the colour rings. These were distinctly shown at all degrees below the angle of total reflexion; but as soon as the angle exceeded 40°, and the bow that marks the boundary had passed, the colour rings vanished, and the so-termed "black spot" alone remained, showing with peculiar distinctness. This is strictly a transparent circle through which all light is admitted and none reflected.

Under all incidences within the angle of total reflexion no colour appeared round the margin of the spot, except a very faint band seen by polarized light; but this was entirely due to the strain and compression of the glass, and had nothing to do with light transmission. The black spot was at its maximum size just after it had passed the boundary into the field of total reflexion; then, as the obliquity of the light was increased, the diameter of the spot diminished and became of less intensity, till at an incidence of about 5° it was almost invisible.

From this it may be inferred that no colour effects or decomposition of light can be produced in abstracting it from a total reflecting surface, for the reason that the object withdrawing that light must be brought into such close contact as to be within the distance of any undulation that can produce colour. This would be less than the ten millionth part of an inch; any distance beyond this will not affect the total reflecting surface. Now this distance is so small, that the question may be raised whether even the minutest atoms in the form of *diatoms* can lay sufficiently close, on account of their irregularities, to abstract the light, and whether their visibility, and also that of insect scales, must not depend

entirely upon adhesive contact caused by moisture or an oily exudation from the scale? If so, the effect would be enhanced by previously pouring over the slide a thin solution of Canada balsam in ether, so as to leave a mere trace, or sufficient only to ensure contact without the risk of spoiling the object by percolating its structure.

In English-mounted slides of *diatoms* and insect scales, some are always found detached from the cover and on the slide, and are shown well; but such is not the case with Möller's and others; they raise their covers off the slides by a ring of cement, and in many of them I have searched in vain for a solitary specimen that has become detached.

II.—*On a Silvered Prism for the Successive Polarization of Light.*

By J. W. Stephenson, F.R.A.S., Treasurer R.M.S., and Actuary to the Equitable Life Assurance Society.

(*Taken as read before the* Royal Microscopical Society, *May* 1, 1872.)

A paper on the "Successive Polarization of Light," by Sir Charles Wheatstone, was read before the Royal Society last year, wherein the author made known another means of producing successive polarization by the reflexion of plane polarized light from a plate of polished silver, and Mr. Spottiswoode delivered a lecture at the Royal Institution on these experiments, in which he stated that "if a ray of plane polarized light fall upon a metallic reflector, it is divided into two, whose vibrations are respectively parallel and perpendicular to the reflector, and the latter is retarded behind the former by a difference of phase depending upon the angle of incidence. If the plane of vibration of the incident ray be inclined at an angle of 45° to the plane of incidence, the two rays into which it is divided have nearly the same intensity. At an angle nearly 45°, which varies with the metal employed, but which is perfectly definite, the intensities become accurately equal. And further, if the angle of incidence have a particular value dependent upon the nature of the metal (for silver 72°), the retardation will amount to a quarter of a wave length. These two rays, on leaving the reflector, will re-combine, and in the last-mentioned circumstances become a circularly-polarized ray. Lastly, the direction of motion in this circular ray will depend upon the side on which the original plane of vibration is inclined to the plane of incidence; if, when it is inclined on one side, the circular ray becomes right-handed, then when it is inclined on the other, it becomes left-handed."

The purport of the present communication is to show how, by instrumental means, this method of producing successive polarization can be made applicable to the microscope.

The first essential condition of such an arrangement is that the ordinary polarizer should remain in its usual position beneath the stage of the microscope, and another is that the silver plate, from which the plane polarized ray is to be reflected, should have a flat and polished surface, and be, if possible, protected from oxidation.

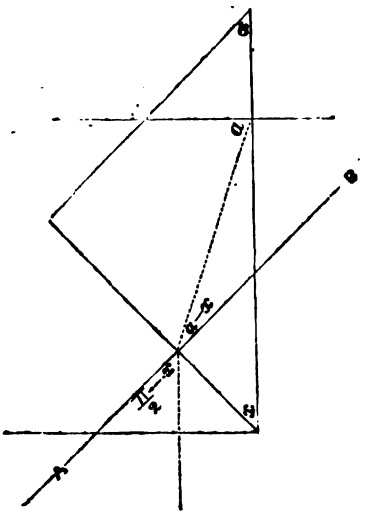
These conditions are fulfilled, by placing between the polarizer and the stage, a truncated glass prism, having two similar acute angles of such a magnitude that the plane polarized ray shall, on entering, be refracted from its course, and thus become incident on the hypothenusal side of the prism, previously silvered by the sugar of milk process, at an angle of 72°, whence after reflexion it will on emergence again be refracted, and resume the original straight line on which it was previously moving in the axis of the instrument.

It will be seen not only that the requisite angle of reflexion from the silver plate has been secured, but that the pure silver thrown down on the highly-polished glass presents a brilliant and even surface, which is effectually preserved from oxidation by the glass on which it is deposited.

The angles and dimensions of such a prism are easily determined if the index of refraction of the glass employed in its construction be given.

This will be readily perceived on inspection of the Figure No. 1, in which the dotted line represents a ray of parallel light, incident on the lower surface of a prism, to which the line A B is a normal. Let a be the circularly-polarizing angle of the metal employed, n, as usual, the index of refraction, and x the angle sought; then, as the angle of incidence will be equal to the complement of the angle

FIG. 1.



of the prism $\left(\frac{\pi}{2} - x\right)$, and the angle of refraction will be equal to the circularly-polarizing angle of the metal minus the angle of the prism $(a - x)$, we at once obtain the following relation:—

$$\cos. x = n \sin. (a - x).$$

From this equation by an easy transformation, which it is unnecessary to give, we obtain

$$\tan. x = \tan. a - \frac{1}{n} \sec. a.$$

As in the case which we have been considering the metal employed is silver, and the index of the glass used by Mr. Browning in

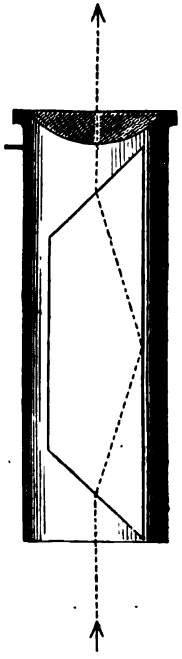
making the prism is 1·526, we find the value of α, representing the angles of the prism, to be 43¾°.

The formula being general, is applicable to glass of any density, or to any metal which can be thrown down on the glass, by simply substituting for a and n their respective values, but silver being more brilliant than any other metal, it is probable that, for microscopical purposes, no better can be employed.

Having thus determined the angles, we have now only to ascertain the *size* of the prism, by which I mean its length relative to its thickness. When the reflecting metal is silver, it depends exclusively on the value of n, and when this equals 1·526 the ratio of the thickness to the length of the prism will be as 1 to 4·122, and for all practical purposes this is sufficient; if greater accuracy is required, which is hardly possible, the new measure can readily be ascertained by the ratio of the sines to the sides of the triangles.

Generally speaking, a prism whose angles are 43¾° should be about 1½ inch long and three-eighths of an inch in both width and thickness; but this must of course depend in a great measure on the size of the Nicol which is used as the polarizer.

Fig. 2.



In the instrument before you, the prism—which is 2 1/16th long and ½ an inch thick—is inserted in a brass tube, Fig. 2, on the upper end of which a small condensing lens of short focal length has been screwed for the purpose of condensing the light, and it being desirable at all times to know in what plane the reflecting surface is placed, a small point of german silver is made to project from the tube at right angles thereto; and still further, as a matter of convenience, for the same purpose, the outside of the tube is blackened for its whole length and half its diameter. Thus prepared, nothing is required but to place the tube containing the prism in what is called the selenite fitting of Beck's polarizer, by which it can be placed in any azimuth.

On the stage of the microscope is a film of selenite of uniform thickness, which has been divided in a line midway between the two neutral axes, the cut portion being brought together after inverting one of them, by which means a compound film has been formed, which exhibits simultaneously, as pointed out by Sir Charles Wheatstone, both right-handed and left-handed successive polarization—when the polarizer is turned, the tints of one portion ascend, while those of the other descend, and when it is at 45° and 135°, they

exhibit complementary colours—the film being that producing a green, the successive colours will be, on one side, G, B, P, V, R, O, Y, and on the other, in the reverse order.

If the selenite film be placed between the polarizer and the silvered prism it will be found, as explained in the paper previously mentioned, that on rotating the analyzing prism we shall have all the phenomena of successive or circular polarization, but, with the selenite between the analyzing plate or prism, when the polarizer is at 0 the polarized ray is reflected unaltered by the silver plate, "but when the polarizer is turned to 45°, 135°, 225°, or 315°, the plane of polarization of the ray falls 45° on one side of the plane of reflexion of the silver plate, and the ray is resolved into two others, polarized respectively in the plane of reflexion and the perpendicular plane, one of which is retarded on the other by a quarter of an undulation, and consequently gives rise to a circular ray;" when the polarizer is turned, "so as to place the plane of polarization in any intermediate position between those producing rectilinear and circular light, elliptical light is obtained."

To say more on the subject would be to detail the various experiments described in the 'Proceedings of the Royal Society,' in which Sir Charles Wheatstone's paper appears, and from which I have already quoted; but I may add that by this trifling addition to our instrumental means we are enabled to determine which is the thicker of two films of the same crystalline substance, to examine the coloured rings of crystals by light circularly or elliptically polarized, to produce the succession of colours with selenite or other plates as previously mentioned, as well as other phenomena which are "numerous and varied."

III.—*Structure of Battledore Scales.*

By J. Anthony, M.D., F.R.M.S.

(*Read before the* Royal Microscopical Society, *May* 1, 1872.)

I venture to make some further observations on the structure of Battledore scales, inasmuch as, through the kindness of Mr. McIntire and Mr. Wonfor, I have been enabled to examine carefully a number of species of the Lycænidæ, to which these scales are peculiar; and more particularly do I bring this matter forward, because in making such examination I employed a mode of illumination which I found so very useful, that I would call the attention of the Society to it, as a valuable addition to our present means, for the investigation of structure under the microscope.

I will take this illumination first, as on it will partly hinge the correctness of what I have to describe with respect to the scales. In looking then at the various forms of these Battledore scales, it of course was desirable to make out clearly the structure on the surface by "reflected" light, in order to check the suspected fallacious appearances given under "transmitted" light. Now a $\frac{1}{8}$th objective entailed such an amount of obliquity of light with respect to the plane of the object, that shadows, *quasi* rugosities, and possible extraneous matter on the surface of the scale became unduly prominent, so that very small bodies, such as the tubercular elevations I was seeking, were lost in all these exaggerations of light and shadow. Under these circumstances I bethought me of applying a device, often resorted to in practical astronomy, in the examination of close doubled stars, where the extreme contrast of brilliant light against a dark ground produces much confusion of vision, the said device for getting rid of this consisting in illuminating the field of the telescope artificially just to such an extent as to *reduce the violent contrast, without impairing the brightness of the image.* My adaptation of this principle to the microscope was accomplished in the following way: I illuminated the surface of the scale vividly from the side, by means of the usual plano-convex lens, and then, reflecting some light (by my mirror below the stage) through the condenser, I gradually brought that condensed light up to just so much brightness, as to very faintly illuminate the field and the object. The effect was all I could desire; the exaggerated shadows were done away with, the dominant illumination was still reflected from the object, and a careful balancing of these lights gave me the Battledore scales with a beauty, and the markings on their surfaces with a reality, which I had hardly ventured to expect. I suppose I may call this device "double illumination," and I take the employment of it for the microscope to be a novelty, as I have never heard of, or read of, its being so employed; however, remembering the Solomonian proverb that "There is nothing new under the sun," I dare say somebody will start up and say he has used this mode of

examination I do not know how many times! However that may be,—and I am sure I do not want to plagiarise—I can honestly advocate the use of this "double illumination," in the examination by "reflected" light of semi-diaphanous objects, which I take to comprise the majority of objects examined by the aid of the microscope.

To proceed to the Battledore scales. I am glad to say that I have found no reason to alter my opinion with respect to the tubercular bodies I described in a former paper as existing on the surface; they were plainly enough seen in *Ly. Alexis* by reflected light, but they could not be so readily made out in other species; this was unsatisfactory, as opposed to the usual harmony of the structure which is found to obtain in allied species; and moreover, as these *quasi* tubercles were more easily seen under transmitted light, it was suggested to me by an acute and intelligent observer, that these tubercles were really inside the scale, and formed part of a framework, and so held up, or pushed up the inner membrane of the scale in such a manner as to show on the surface as so many "lumps" or "bumps." I confess that in my inability to see the tubercles on the majority of the Battledores under reflected light, other than as shadowy, slight elevations, I was inclined to think that this "framework" view of the structure must be correct; but then the rows of distinct tubercles with stems and rounded heads on *Ly. Alexis* were so *very real*, and on a foreign specimen kindly sent to me by Mr. McIntire were absolutely so "staring" by reflected light, that I felt it necessary to re-examine more vigorously the whole series of scales, and to modify if possible that exaggeration of light and shadow which only dazzled and confused me. Hence the employment of the "double illumination" which I have described, and which made it at once evident that there was really no want of harmony of structure, inasmuch as I was now able to make out, so soon as the secondary light had removed the too great intensity of the shadows, that the tubercles were visible on *all* the species of Battledore scales, but that they varied in size in almost every one of the said species, being sometimes so small as easily to escape observation. I found the largest tubercles by far on the foreign specimen sent to me by Mr. McIntire, which scale I have sketched carefully.* The tubercles measure the $\frac{1}{8000}$th of an inch in length; in Alexis they are from $\frac{1}{12000}$th to $\frac{1}{10000}$th of an inch, while in many other species, as "Long-tailed Blue," I did not make them more than $\frac{1}{14000}$th of an inch.

Repeated observations have led me to recognize that the tubercles are in all cases placed upon a distinctly-beaded but narrow rib; the base into which the little column seems to fit,

* Dr. Anthony forwarded the sketch named, and the appearance is substantially the same as in the engraving illustrating his former paper. (See the number for January, 1872.)

looking broad and flattened—I use the term "fit," because not unfrequently I have seen the appearance, the tubercle displaced from the base to which it has apparently belonged, and lying on its side, a round hole in the flattened base strongly suggesting the original locality of the small peg-like tubercle. In one specimen, to which I can always refer by means of Maltwood's finder, a scale has received some crushing, and several of these little "pegs" lie about, looking as if they were knocked out of place; two of them appear reclining on the surface of the scale, and several more are seen on the glass by its side.

Although the Battledore scale is not strictly what would be called an object for the polariscope, there is no doubt parts of its structure, and particularly the tubercles and the ribs, do partially *de*-polarize the light, and therefore polarized light becomes valuable in examining the structure, and under a proper management of the analyzer and the sized pencil of light transmitted, the tubercles show with *extreme* brilliancy and reality, and moreover they seem to be placed as I have represented, in shallow pits in the surface of the scale in some specimens, while in others the ends of the pits seem semi-continuous, as if a row of tubercles stood in a groove, the sides of which were constricted at intervals. These pits may really be elevations, and due to "inversions of the image," but they do not look so, and I have put them down just as I saw them. I am so afraid of being thought guilty of exaggeration, that I have quite understated the effects which can be got in these tubercles by aid of polarized light. To employ this light to advantage, I prefer daylight, with tourmaline and Nicol's prism only, the latter behind the objective. For lamplight a pale-green ground from an appropriate selenite film seems an advantage.

A word in concluding, to those who may wish to repeat these observations on the markings of Battledore scales, or of kindred structures. Scales are best seen by "transmitted" *ordinary* light, when a "pin-hole" stop is placed like a small cap on the usual wide-angled condenser, and by being very particular that both flame of the lamp and object are in focus, or very nearly so, at the same time. When scales are looked at by "reflected" light, then I saw them at their best by bringing up a little transmitted light at the same time, such light being quite subordinate, and only for the purpose of rendering the black shadows transparent. A similar effect of course can be got by using a second lamp and "bull's-eye" at the other side of the microscope, or even in a minor degree by a bit of white card, placed in the stage beneath the object, so as to reflect light, but on the whole I prefer the first form of "double illumination" as equally satisfactory and far less troublesome.

April 27th, 1872.

IV.—*Beale's Nerve Researches.*

Dr. Beale in Reply to Dr. Klein.

(*Read before the* Royal Microscopical Society, *May* 1, 1872.)

With reference to the criticisms of Dr. Klein, published in the April number of the Journal, I beg to offer the following remarks:—

1. The network described by Remak was not the same sort of network that was to be seen in my specimens.

2. Neither Schaafhausen nor Remak have given figures like those of the specimens published by me in the 'Phil. Trans.'

3. The network described by Kölliker was different from that seen and described by me. See, for example, my figure of the mucous membrane of the epiglottis, fig. 70, p. 168, 'Die Structur der Einfachen Gewebe,' translated by Victor Carus, Leipzig, 1862.

4. It is not possible to decide as to priority in such matters unless the figures given are compared. To say that So-and-so "described" a network or plexus before somebody else, goes for very little unless the exact sort of network or plexus is exhibited in drawings and placed before the observer. Neither ought the mere "assertions" of one observer to be brought forward against the observations of another.

5. My observations of the distribution of nerves to the bladder of the frog were made before those of Klebs, and perfectly independently of him.

6. That intermuscular networks are not "accepted" does not prove that intermuscular networks do not exist. I can show finer "intermuscular" fibres than the authorities cited by Klein. It is curious that the specimens of "intramuscular" nerves do not "keep," while mine of "intermuscular" nerves have been preserved for years.

7. I have, contrary to the statement of Dr. Klein, examined isolated muscular fibres in the frog, chameleon, and many lizards and snakes, as well as in many mammalia.

8. As regards the distribution of fine nerve fibres amongst epithelial cells, I have often tried to demonstrate them, but so far have failed to do so. I do not say that there are no nerves in the cuticle and in the conjunctival epithelium, but only that I have not yet seen any specimens that demonstrate them to my satisfaction. It is remarkable that those who believe in the distribution of nerves amongst epithelial cells do not agree whether they terminate in ends or form networks. While of all the observers who admit inter-epithelial nerve fibres, it is strange that not one has attempted to explain how they got there.

9. Dr. Klein fancies he convicts me of shifting my ground with regard to the constitution of the finest nerve fibres seen by me. Speaking of me, he says, "He admits (December, 1871) that his delicate fibres are compound, that they are fibrillar." He will find in my paper * that it is distinctly stated that "*each cord* (of the network)

* 'Phil. Trans.,' 1862.

is compound, and composed of numerous fibres which never anastomose." Again, in my Croonian Lecture, 1865, "Every fibre of this network is *compound*; so that perhaps the term 'plexus' more truly describes the arrangement."* My "previously-expressed opinions," therefore, do not clash with my "new views," as Dr. Klein wishes his readers to believe. Not only have I very distinctly stated that the fine nerve fibres I described were composed of still finer fibres, but I have given many figures of fine nerve fibres in many situations showing that they were so, and have published a diagram to show what I believe to be the arrangement, see figs. 399 and 400, 'How to Work with the Microscope,' third edition. Neither do I "abandon" my "former views," though I readily admit that "it is quite possible that there may be still finer fibres than any that I have been able to demonstrate." Dr. Klein's criticisms would be dissipated at once by reference to many drawings published by me during the last ten years. But Dr. Klein brings forward a quotation from p. 332 of the fourth edition, of 'How to Work with the Microscope,' and then remarks that I now admit that the nerve fibres are "compound," that they are "fibrillar," &c. Instead of commenting upon this, I will refer your readers to the very same page from which Dr. Klein quotes, and to the previous page, and to the plates illustrating my remarks, and ask them if Dr. Klein is justified in asserting that I have withdrawn from my previously-expressed opinions.

10. Dr. Klein seems to consider that the "nucleus" belongs to the "sheath," and asserts that the fibrillæ which come off from a non-medullated nerve fibre, either singly or in bundles, have absolutely nothing to do with any nucleus whatever. This may be true as applied to his own specimens, but it is absolutely incorrect as regards mine. There are nuclei to be demonstrated in hosts of fine nerve ramifications which come off from non-medullated fibres, and in every tissue in which nerves ramify.

11. There are other most positive assertions of Dr. Klein's which might also be refuted by specimens. Dr. Klein says that he has ascertained on one of my own preparations of the cornea that my view "by no means agrees with the facts"! Thus it would appear that in the course of less than a minute he had been able to discover what I had failed to make out after examining the same specimen for hours at different times during the last seven or eight years. However, one may feel content that one is not held up as a person perfectly blind, or as a melancholy example of perverted visual sense.

12. In England original observers in this department labour under great disadvantages. We are very few in number, and our work excites comparatively little interest. We have to condense our remarks into the smallest possible space, because English readers are provoked by long and exhaustive dissertations. I have myself always endeavoured to write briefly on anatomical questions, but to render my drawings as careful and accurate as possible, and at the same time to preserve the specimens from which the drawings have been

* 'Proceedings R. S.,' 1865, p. 237.

taken. If, therefore, a hostile critic merely refers to my description, he places me at a disadvantage, for no fair comparison can be made between contemporary anatomical labours unless the drawings are placed side by side. Again, an English minute anatomist, if he live long, may undoubtedly succeed in convincing his countrymen that he has really demonstrated some new facts; but the predisposition of many writers in England is against English anatomical work, and so strongly in favour of German observations, that the latter are sure not only of finding a prominent place in many of our journals, but of being favourably received and criticized. Observations made at home are often passed over.

It would be difficult to find a question of greater interest in its bearing upon the nature of important physiological and pathological changes than the distribution of nerves to capillary vessels. I am indebted to Dr. Klein for drawing the attention of his readers to the fact that these nerve fibres were first described and figured by me. My observations were made nearly ten years ago, and I believe Dr. Hughlings Jackson and Dr. Klein are the only writers who have yet drawn attention to the subject, which is so very closely connected with general pathology that it is hardly possible to discuss the nature of the nutritive changes in disease without taking into consideration the arrangement and action of the nerves distributed to capillary vessels. I hope that Dr. Klein's researches will render this and kindred inquiries more popular in this country, and that they will ere long receive that consideration from physiologists and physicians which they unquestionably deserve.

In conclusion, I may be permitted to remark that a scientific observer ought to consider himself fortunate if his researches have been allowed to pass for a few years without hostile criticism, and still more fortunate if, at the conclusion of a decade, observations made by him at its commencement have not been altogether forgotten and ignored; for few among anatomists have been allowed to work on for several years without having their "errors" very freely exposed and rendered too "gross" by a little.

LIONEL S. BEALE.

April 9th, 1872.

V.—*On Bog Mosses.* By R. Braithwaite, M.D., F.L.S.

Part IV.

Plate XIX.

Group B. Subsecunda. — Plants soft, laxly tufted, commonly growing with other species. Branches terete, their leaves erecto-patent or somewhat imbricated, usually subsecund, broadly ovate, truncate at apex, canaliculate-concave, the edges involute in the upper half, broadly margined. Dioicous.

2. Sph. tenellum Ehrhart.

Herbar. Petropol. (1795) teste Lindberg.

Plate XIX.

Syn.—*Sph. tenellum* Persoon MS. Bridel, Mantissa p. 1 (1819). Bryol. Univ. I, p. 4 (1826). Lindb. Torfm. No. 13 (1862). Hartm. Skand. Fl. ed. 9, II. p. 83 (1864).—Non *Sph. tenellum* of Bryol. Germ. nor of Funck, Taschenb.—*Sph. cymbifolium* β *tenellum* Bridel, Musc. rec. II, p. 24 (1798).—*Sph. obtusifolium* β *tenellum* Weber & Mohr, Bot. Tasch. p. 72 (1807).—*Sph. molluscum* Bruch, Regensb. Fl. 1825, p. 633. Bridel, Bryol. Univ. I, p. 753 (1826). C. Müller, Synop. Musc. I, p. 95 (1849). Wilson, Bry. Brit. p. 19, T. LX (1855). Hartm. Skand. Fl. ed. 6, p. 435 (1854). Schimper, Torf. p. 71, T. 21 (1858). Synop. p. 682 (1860). Berkeley, Handbook, p. 306, Pl. 2, fig. 3 (1863).

Dioicous. Plants *delicate, extremely fragile*, 2–6 inches high, but attaining a foot or more when floating, *densely clustered in soft tufts of a pale greenish yellow colour*. Stem simple or bipartite, straw coloured, covered with a *double non-porose cortical layer*. Ramuli abbreviated, single, binate or ternate, either all patulous or 1–2 deflexed, pale red, lax-leaved, not pointed; cortical cells very unequal, smallest quadrate, *largest flask-shaped, numerous, with the apex recurved and projecting, perforated, yellowish at the mouth*. Cauline leaves erecto-patent, reflexed, rather large, ovate-oblong, slightly narrowed toward the apex, margin incurved, with a broadish border, hyaline cells densely fibrose, sparingly porose, but toward the base free from both threads and pores.

Ramuline leaves patent or laxly incumbent, sometimes secund,

EXPLANATION OF PLATE XIX.

Sphagnum tenellum.

a.—Female plant. *a* ♂.—Male plant.
1.—Part of stem with a branch fascicle.
2.—Catkin of male flowers. 2 *b*. Bract from same.
3.—Fruit with its peduncle. 4.—Peduncular leaf.
5.—Stem leaves. 5 *a a*.—Areolation of apex of same. 5 *a b*.—Ditto of base.
6.—Leaves from middle of a divergent branch. 6 *x*.—Transverse section. 6 *p*.—Point of same. 6 *a a*.—Areolation of upper part. 6 *c*.—Single cell from middle × 200.
7.—Intermediate leaves from base of a divergent branch.
9 ×.—Part of section of stem. 10.—Branch denuded of leaves. 10 ×.—Section of ditto.

broadly ovate and elongato-lanceolate, three toothed at apex, *incurved at edges in the upper half*, margined; hyaline cells strongly reticulato-fibrose, on the inner side of the leaf very prominent, confluent and perforated with pores, on the back separated from each other by the interposed chlorophyll cells, not perforated. Male plants in distinct tufts, rarely intermixed with the female, but resembling them; amentula small, orange-coloured. Fruit from the capitulum or from the side of the stem, on a more or less elongated pseudopodium; peduncular leaves large, imbricated, outer oblongo-lanceolate, inner ligulate, densely fibrose from the middle to the apex; capsule small, thin, ochreous-brown; spores large, sulphur-coloured.

Var. β *fluitans* Schimper, Torf. p. 72, T. XII. fig. 6. Very long and slender; pendent branches none, all the leaves distant, pseudopodia very long, with distant leaves.

Var. γ *longifolium* Lindberg M.S. Resembling the typical form; branches more attenuated at points, with longer leaves; cauline leaves with the hyaline cells all fibrose.

Hab. Spongy places on heaths, and wet hollows in hilly places. Not common. Fr. May. Occurs throughout Europe, but most frequent in the Scandinavian regions. In Britain it has been found in Lancashire, York, Sussex, Kent, &c., also in Scotland and Ireland. The specimens figured were collected on Keston Common, in Kent, by my friend Mr. Reeves. Var. β is stated by Schimper to have been found by Lesquereux at Marais les Ponts in the Swiss Jura. Var. γ was collected by Prof. Lindberg at Helsingfors, Finland.

A word as to the alteration of name. In Phœnogamic Botany, Entomology, and other departments of Natural History, the adoption of the first name by which a species has been described (dating from the establishment of the binomial nomenclature by Linnæus) is considered imperative; yet the synonymy of mosses is wofully confused, for Hedwig and others gave a new specific name as often as they changed the genus, a rule not sanctioned by the best authorities. Others may object with greater reason that the brief descriptions of the older authors are not sufficient to identify the species with certainty, yet it must be remembered that the actual specimens of very many of them are in existence, and their examination by a competent authority in most cases settles the question. Prof. Lindberg, who has worked so indefatigably at this unattractive department of botanical literature, has shown in his 'Rev. Crit. Ic. Musc. Fl. Dan.,' that this species is in the St. Petersburg Herbarium named *tenellum* in Ehrhart's own handwriting; this, however, without description, might not be allowed to stand, but the same species received the same name from Persoon, as proved by a specimen from him, preserved in Swartz's herbarium, and a description is given by Bridel in his 'Mantissa Musc.' (1819), the

leaves indeed being described as recurved at the point, which might perhaps refer to them in a dry state. Bridel also admits *S. molluscum* into the Bryol. Univ., but he only copied the description of Bruch (1825) without having seen a specimen. I have no hesitation therefore in adopting the name first given to the species.

VI.—*Crystallization of Metals by Electricity.*

By Philip Braham.

Plate XX.

The accompanying illustration represents the result of a series of experiments, and shows their most distinctive appearance as seen with a 2-inch objective.

Gold and copper are nodular in their formation, copper having the larger and more definite spherules.

Silver is peculiarly fine, and leafy, and has its main line of crystallization distinctly marked.

Tin may be considered the most beautiful object; its invariably rectangular crystal, straight direction, and rectangular deviation, are exceedingly characteristic.

Lead may be likened to silver enlarged; but the breadth and taper of the leaves, and its want of centre line, are very distinctive.

Zinc grows feathery, with a crystalline, fungoid appearance.

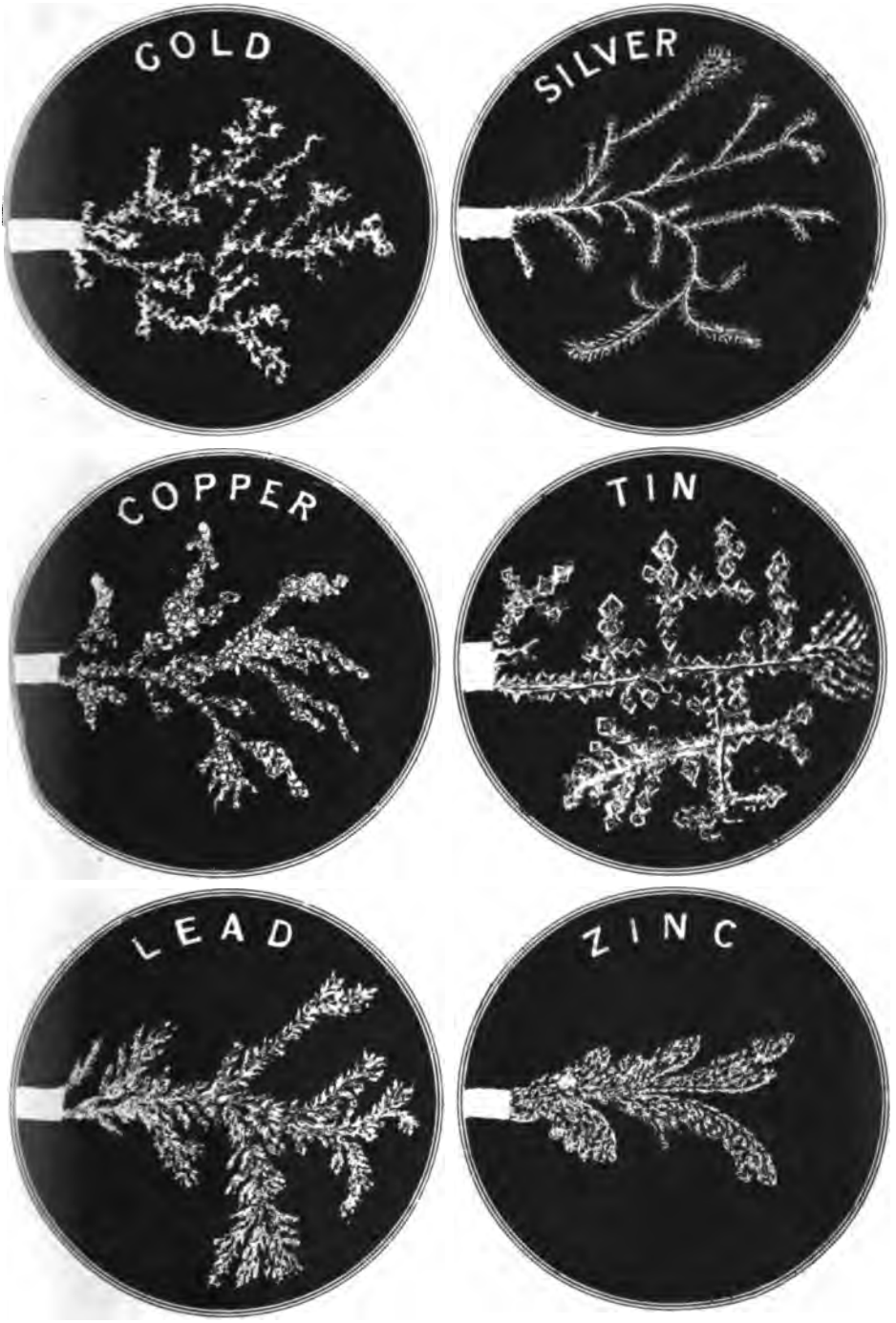
There are apparently other forms, which make their appearance with the same metals; but on careful investigation they are found to be only modifications of those illustrated.

It is worthy of notice that the two yellow-coloured metals have decidedly spherical formation, and that gold leaf transmits a blue light, and the salts of copper are beautifully blue. The size of the crystals depends on the regulation of the battery; the more rapidly they are formed the smaller they are.

I have tried the experiments away from the microscope, and have been generally disappointed with the result; the difficulty of manipulating away from it may be easily conceived by considering the solution to be a resistance to the electricity, and as the metallic crystals form that resistance is diminished, and a rush of crystals spoils the experiments.

I have succeeded in obtaining crystalline appearances from platinum, palladium, iron, bismuth, and antimony, but not of a very definite character.

The delicacy of some of the crystals will not allow of their being kept, and the beauty of their appearance is lost by drying. The more oxidable metals tarnish immediately, so that no ade-



Metals crystallized by electricity

West & Cº lith

quate idea of the beauty of the experiments can be gleaned from mounted specimens, or sketches; they must be seen in the act of growth.

The formation of metals in their ores is strikingly similar, and there is every reason to believe their existence in the metallic state is due to the action of electricity, but whether thermal or magnetic is an investigation that could be profitably pursued; it would be of value if the direction of metallic veins could be determined as to their poles, and their position with respect to the surrounding minerals.

VII.—*On the means of distinguishing the Fibres of New Zealand Flax from those of Manilla or Sizal, by the Microscope.*

By Captain Hutton.

To an experienced eye, the difference between Manilla fibre and New Zealand flax, when in any quantity, is so great that I do not suppose that any mistake would occur in distinguishing between them; but when a small portion only is available for examination, as when Manilla rope is sparingly adulterated with *Phormium*, the case is different, and unless the easily-recognized marginal or mid-ribs of *Phormium* are present, the microscope will have to be resorted to before a satisfactory determination of the nature of the fibre can be made. Sizal, again, is still more difficult to distinguish by the unassisted eye from *Phormium*, than the latter is from Manilla, and I therefore propose to give the characteristics of each fibre, when examined with a microscope:—

In *Manilla*, the fibrous bundles are oval, nearly opaque, and surrounded by a considerable quantity of dried-up cellular tissue, composed of rectangular cells. The bundles are smooth, very few partly-detached ultimate fibres are seen, and no spiral tissue.

In *Sizal*, the fibrous bundles are also oval, and surrounded with cellular tissue. They are also smooth, and very few ultimate fibres project from the bundles. They are, however, more translucent than Manilla, and can always be recognized by the large quantity of spiral fibres mixed up in the bundles.

In *Machine-dressed Phormium* the bundles are translucent, and irregularly covered with tissue, being in places quite free from it. Spiral fibres can also be detected among the bundles, but not in the same quantity that is seen in Sizal. Many more ultimate fibres stick out from the bundles, which are flat instead of oval. In those places where the bundles are entirely freed from tissue, they are generally divided longitudinally into two or more smaller bundles, or *fasciculi*, and in these places the number of half-

detached ultimate fibres is greatly increased; these are, however, rarely broken, most of them having the end perfect. Spiral fibres are here absent.

In *Maori-prepared Phormium* the bundles are almost entirely free from tissue, and quite so from spiral fibres. They are always broken up into many fasciculi, which average, perhaps, some twelve or fifteen ultimate fibres in each fasciculus. Many ultimate fibres are semi-detached, and they are much more broken than in machine-prepared fibre.

If an examination of the fibrous bundles fails to give a satisfactory result, resource must be had to the size of the ultimate fibres which compose the bundles, and this will always give a certain proof of the nature of the fibre under examination, provided enough measurements are made to strike an average, and this can always be done, as one fibrous bundle a couple of inches long will yield an ample number of ultimate fibres.

In order to accomplish this, the fibre must be first boiled for two or three hours in a weak solution of potash, by which means it will be decomposed. It will then be found possible to isolate individual ultimate fibres from the rest of the bundle by careful dissection with needles under a microscope, the decomposed bundle being placed on a glass slide, in a drop of water. When thus prepared, it will be found that the ultimate fibres of Sizal will separate easily, those of *Phormium* with more difficulty; while it will require great care to prevent breaking those of Manilla when endeavouring to detach them. In fact, Manilla requires four or five hours' boiling in a tolerably strong solution of potash, before the ultimate fibres can be detached readily.

The following Table gives the average dimensions of the different ultimate fibres, made from a considerable number of measurements of each kind:—

	Length of ultimate Fibres in inches.			Diameter of ultimate Fibres in inches.			Thickness of Cell Wall in inches.
	Max.	Min.	Mean.	Max.	Min.	Mean.	
Sizal	·25	·20	·21	·00140	·00098	·00112	·00028
Manilla	·25	·17	·21	·00098	·00060	·00083	·00024
Phormium	·80	·13	·39	·00070	·00035	·00045	·00015

It will be thus seen that the average length of the ultimate fibre of *Phormium* is nearly twice that of either of the others, while the average diameter is not much more than half that of Manilla, which again is much less than Sizal. The cell wall also of *Phormium* is also much thinner than that of either of the other two.

NEW BOOKS, WITH SHORT NOTICES.

The Lens: a Quarterly Journal of Microscopy and the Allied Natural Sciences, with the Transactions of the State Microscopical Society of Illinois. Edited by S. A. Briggs. Chicago, U.S.A. 1872. —Our readers were made aware of the existence of this journal through several quotations which we made from it in a late number. We have now, in a few words, the pleasure of introducing it to them as a valuable and carefully-edited report, not only of the Society whose proceedings it is made the medium of communicating to the scientific world, but of the whole trans-Atlantic progress which is being made in the various branches of science connected with the microscope. Its first number promises well, for a moment's glance shows that its editor understands his business, and does not mean to allow any one department to get a too prominent position at the expense of any other. We therefore find that he has taken care to have every subject well represented, and we must, in the first instance, acknowledge the compliment he has paid us of selecting as his sole illustrated article, a paper which originally appeared in these pages, from the pen of Mr. M. Johnson, M.R.C.S. But, besides this, there are several able articles, more especially the "Conspectus of the Families and Genera of the Diatomaceæ," by Professor H. L. Smith; "The Preparation of Soft Tissues," by Dr. Danforth; "Microscopical Memoranda," by Col. Dr. Woodward; and "A New Method of Illuminating Opaque Objects under High Powers," by Dr. H. A. Johnson. Altogether, we are well pleased with the new Chicago venture, and we heartily wish it every success it so well and so thoroughly merits.

De la Microcythémie, par MM. Vanlair et Masius, Professeurs à l'Université de Liége. Bruxelles. Henri Manceaux. 1871.—We call attention to this work, not because we have any faith in the conclusion at which the authors have arrived, but because the theory they urge is a most important one if proved, and in order that those who have given attention to the subject may consider this theory. The writers are men very well qualified to make observations in microscopy and to write upon them; but we fancy in the present instance they are entirely mistaken, and that therefore their view is absolutely a false one. They give the term *microcyte* to a blood corpuscle which appears to differ somewhat from the ordinary ones, and they term the condition one of *mycrocythemia* whenever the blood presents a considerable number of those *microcytes*. We fancy the authors have rushed rather hastily to a hypothesis, for we can assure them, from our own experience, that there is hardly a human being in existence who does not present less or more of the characters they associate with *mycrocythemia*. At least, the writer's own observations, and they have been considerable, have led him to that conclusion. However, MM. Vanlair and Masius hold quite a different opinion, and they have expressed it very fully and supported it somewhat by

observation in the work under notice. The plate which accompanies the book is certainly, so far as we can see, opposed to the theory laid down in the volume. Still, our readers will, we trust, judge the book on its own merits.

PROGRESS OF MICROSCOPICAL SCIENCE.

Uniformity of Nomenclature in Objectives.—On this subject Dr. R. H. Ward has a paper in the 'American Naturalist' for March, which is of interest, and which we shall probably reproduce in a future number of this Journal. He thinks that the following questions still remain open:—Should the standard one-inch objective be characterized by magnifying ten diameters as used in the compound microscope, or should it be compared to a simple lens of actually measured focus or foci? Should the objective be named by its equivalent focal length, or by its amplifying power, or both? Should our standard distance of measurement be changed from ten inches [254 mm.] to nine and five-sixths inches [250 mm.]? From what point in the objective shall the distance to the scale be measured? At what point of screw-collar adjustment shall the objective be placed for rating its angular aperture and amplifying power? Should the name *ocular* be substituted for "eye-piece" in general use?

Micro-photography.—Mr. Charles Stodder has a semi-popular paper on this subject in the 'Boston Journal of Chemistry.' It seems, says Mr. Stodder, that the history and advantages of microscopical photography are well given, though no reference is made to the corresponding disadvantages, such as the unequal applicability of the process to objects of different colours, and the necessity of representing a single focal plane or section of the object, while the different varieties of delineation by hand-work enable the artist, if sufficiently expert to know what he sees, and sufficiently candid to draw what he sees and not what he thinks he ought to see, to reconstruct to some extent the object, and represent at a single view the knowledge gained by many slight changes of focus. Unfortunately for their value as tests in this case, the so-called test-objects seem to be particularly suitable for photographic illustration. Of the Woodward photographs familiar to the writer, those of the test-objects are (probably necessarily) more faultless than those of the tissues, and are therefore tests of the corrections of the objectives and of the perfection of the illumination rather than of the general applicability of the photographic process. Of this latter question, but little understood as yet, the researches of Dr. Woodward and others give promise of an early solution.

The Structure of the Bloom or Wax of Plants.—Professor De Bary's paper on this subject in the Botanical Zeitung is thus abstracted

by a writer in the 'American Naturalist' (March). The original memoir contains thirty beautiful illustrations. The wax does not appear to be a simple coating over the surface, as though it might have been laid on liquid with a brush, forming a continuous layer. It is seen to be rather a dense forest of minute hairs of wax; each one sitting with one end upon the epidermis and the other either rising up straight or rolled and curled among its neighbours. This matting of waxen hairs often becomes so dense that when examined from the surface it presents to the microscope the appearance of a continuous layer, while a carefully-made section of the leaf, or skin of the fruit, shows its true structure. The question from what part of the epidermis or sub-epidermal tissue does the wax come, is most beautifully and clearly answered. Professor De Bary says that in the cell-contents there cannot be discovered the slightest trace of wax, and the statement that the chlorophyll is partly made of wax is totally erroneous. The locality in which it can be first detected is the cuticle and the cuticularized elements of the epidermis cells.

The Odontoblasts of Teeth.—Mr. T. C. White, the secretary to the Quekett Club, lately read before that Society a very interesting paper on the above subject. Mr. White agrees most generally with the views already laid down, and he showed the Society his method of examining the teeth, which is certainly of importance. His remarks on the subject of the odontoblasts are of interest. He says that about the seventh month of fœtal life the ossification of the tooth commences, and the dentine is represented by a cup-shaped scale capping the crown, and ultimately extending down the sides and embracing the whole of the upper surface of the pulp. It is at this period of their growth that the odontoblasts are most active, for they have the development of the dentine before them, and deriving a plentiful supply of nutrition from the plexus of blood-vessels beneath them, dentine is formed through their agency from without inwards, till the pulp being reduced to the size at which we generally see it by the gradual formation of the dentine, the odontoblasts become dormant, but capable of awaking to activity under the influence of certain circumstances of irritation; thus if caries attacks a tooth at a particular spot the tubuli in the dentine, through the fibrillæ in them, become consolidated at an equal distance from the point of attack all round it, and a barrier seems to be thus thrown up against the inroads of the advancing enemy; but unless such a remedial measure as the careful excavation of the carious portion of the tooth and subsequent plugging of the cavity be adopted, barrier after barrier may be thrown up but to be overcome. Even then the odontoblasts of the pulp resist by forming new dentine in its very substance, and it is only when inflammation and suppuration destroy the odontoblasts that this reparative process is annihilated. Mr. White's mode of preparing the tooth should also be read.—*Vide Quekett Club Journal*, April.

The Penis of the Flea.—Mr. W. H. Furlonge has a paper in the last number of the 'Journal of the Quekett Club,' in which he enters very fully on the anatomy of the flea. Most of his researches have

been done years before by a French zoologist; but there are some points of novelty in his paper. Particularly interesting is his description of the penis of this animal. Looking down, he says, upon the extremity of the extruded sheath, when expanded, a minute orifice may be observed through which the penis is projected. It is a dark-coloured, wire-like organ—presumably chitinous—which is capable of protrusion for about half the length of its sheath; but of its structure he is able to give no further description; in fact, he has only seen the extrusion of the organ in some four or five instances out of the dozens of male fleas he has had under observation, and on these occasions it was protruded and retracted with such rapidity that the eyes could hardly follow it. He has no doubt, however, that it constitutes one of the coiled rods or chitinous fibres, and that it is projected by the uncoiling, and withdrawn by the release of the spring-like coil. With regard to the copulation of this insect he says that when young and transparent specimens of the insect are selected this important process can be seen with remarkable clearness, and the animals being so closely locked together may be manipulated with great facility. Even when placed between the glasses of the compressor they will endure an amount of compression quite sufficient to render the abdomen of each insect perfectly transparent without the interruption of the copula. When thus examined the extremity of the sheath of the penis may be seen to be continually opening out and closing up, and by this action the spinous processes attached to the extremity of the sheath appear, as it were, to *grasp* the ovarian clusters which he has described; but he has not been able to observe the protrusion of the penis itself during the copula, nor at any time to distinguish spermatozoa in the female.

Description of certain Lepidodendroid Stems.—The following account of certain stems recently examined by Professor Williamson has been sent by him to the Royal Society, and is published in the 'Proceedings of the Royal Society,' No. 133. The examples vary from the very youngest, half-developed twigs, not more than $\frac{1}{12}$th of an inch in diameter, to arborescent stems having a circumference of from two to three feet. The youngest twigs are composed of ordinary parenchyma, and the imperfectly-developed leaves which clothe them externally have the same structure. In the interior of the twig there is a single bundle, consisting of a limited number of barred vessels. In the centre of the bundle there can always be detected a small amount of primitive cellular tissue, which is a rudimentary pith. As the twig expanded into a branch, this central pith enlarged by multiplication of its cells, and the vascular bundle in like manner increased in size through a corresponding increase in the number of its vessels. The latter structure thus became converted into the vascular cylinder, so common amongst Lepidodendroid plants, in transverse sections of which the vessels do not appear arranged in radiating series. Simultaneously with these changes the thick parenchymatous outer layer becomes differentiated. At first but two layers can be distinguished—a thin inner one, in which the cells have square ends, and are disposed in irregular vertical columns, and a thicker outer one consisting of

parenchyma, the same as the epidermal layer of the author's preceding memoir. In a short time a third layer was developed between these two. When the vascular cylinder had undergone a considerable increase in its size and in the number of its vessels, a new element made its appearance. An exogenous growth of vessels took place in a cambium layer, which invested the pre-existing vascular cylinder. The author distinguishes the latter as the vascular medullary cylinder, and the former as the ligneous zone. The newly-added vessels were arranged in radiating laminæ, separated from each other by small but very distinct medullary rays. At an earlier stage of growth traces of vascular bundles proceeding from the central cylinder to the leaves had been detected. These are now very clearly seen to leave the surface of the medullary vascular cylinder where it and the ligneous zone are in mutual contact; hence tangential sections of the former exhibit no traces of these bundles, but similar sections of the ligneous zone present them at regular intervals and in quincuncial order. Each bundle passes outwards through the ligneous zone, imbedded in a cellular mass, which corresponds, alike in its origin and in its direction, with the ordinary medullary rays, differing from them only in its larger dimensions. At this stage of growth the plant is obviously identical with the *Diploxylon* of Corda, with the *Anabathra* of Witham, and, so far as this internal axis is concerned, with the *Sigillaria elegans* of Brongniart.

The Modes of Origin of Infusoria.—One of the best and most highly suggestive papers that have for years appeared is that which Dr. Bastian, F.R.S., published in the 'Proceedings of the Royal Society' for April. It is so good that we envy the 'Proceedings' the possession of it. It is of considerable length, amply illustrated, and deals with the subject more simply and intelligently than we have seen in most of the writings on the subject. It certainly appears that the author is justified in the conclusions which he has drawn from a vast series of observations conducted during the past few years. This paper ends by stating that the phenomena which the author has described as taking place in the "proligerous pellicle" may be watched by all who are conversant with such methods of investigation. He does not require to call in the aid of the chemist; he need exercise no special precautions; the changes in the pellicle are of such a kind that they can be readily appreciated by any skilled microscopist. Just as he has supposed that living matter itself comes into being by virtue of combinations and rearrangements taking place amongst invisible colloidal molecules, so now does the study of the changes in the "pellicle" absolutely demonstrate the fact that the visible new-born units of living matter behave in the manner which he has attributed to the invisible colloidal molecules. The living units combine, they undergo molecular rearrangements, and the result of such a process of heterogenetic biocrasis is the appearance of larger and more complex organisms; just as the result of the combination and rearrangement between the colloidal molecules was the appearance of primordial aggregates of living matter. Living matter is formed, therefore, after a process which is essentially

similar to the mode by which higher organisms are derived from lower organisms in the pellicle on an organic infusion. All the steps in the latter process can be watched; it is one of synthesis—a merging of lower individualities into a higher individuality. And although such a process has been previously almost ignored in the world of living matter, it is no less real than when it takes place amongst the simpler elements of not-living matter. In both cases the phenomena are essentially dependent upon the "properties" or "inherent tendencies" of the matter which displays them.

Where should the Characeæ be placed?—Mr. Frederick Currey, M.A., F.R.S., in delivering an admirable Presidential address to the West Kent Natural History Society, made the following observations on this important subject. He referred particularly to the researches made by Dr. De Bary upon the genus *Chara*, which have been published in the monthly reports of the Berlin Academy for 1871. The mode of reproduction in the *Characeæ* has been studied by one of the ablest of German botanists, Dr. Pringsheim, and in his opinion the cell upon which the fertilizing action of the spermatozoa operates is not the cell which immediately produces the pro-embryo, but a cell preceding that one by several generations. If this were so, the formation of the fruit in *Chara* would be similar to what takes place in the mosses. De Bary asserts that Pringsheim's view is incorrect, and that the ovum-cell, or oogonium, is not impregnated until towards the close of its growth, and that the phenomena of impregnation in *Chara* are not the same as those which characterize the mosses, but are similar to the reproductive phenomena which occur in *Vaucheria*. The supposed identity in the development of the fruit of the mosses and the *Characeæ* caused the latter to be arranged systematically at the commencement of the series of the *Muscineæ*, or as a special group in the division of moss-like plants. De Bary, upon the assumption that this supposed identity has been disproved, maintains that the only peculiarity common to the mosses and the *Characeæ* and not met with in other groups, is the form of the spermatozoa, and that this is not of systematic value. He considers that the *Characeæ* ought not to be remitted to the Algæ, amongst which they were formerly placed, because that group, in the sense in which it embraced the *Characeæ*, cannot be considered as subsisting, it being impossible to arrange the latter in any of the well-defined groups of Algæ. The *Characeæ* ought (De Bary says) to be ranked as a special group, not as a link of transition from the mosses to the Algæ, but as an independent group near the mosses on the one side, and the *Florideæ* and *Fucaceæ* on the other, allied also to some of the oosporous *Confervæ*, such as *Vaucheria*.

The Embryological Development of Limulus.—In addition to the work done on this subject by Mr. Packard, we understand that Professor Van Beneden, the eminent Belgian embryologist, on the other hand, has published a paper in the 'Comptes Rendus de la Société Entomologique de Belge,' in which, from a study of the embryological development of Limulus, he arrives at the following conclusions:—
1. That the Limuli are not Crustaceans, as none of the characteristic

phases of the development of Crustacea can be distinguished; and that, on the other hand, their development shows the closest resemblance to that of the Scorpions and other Arachnida. 2. That the affinity between the Limuli and Trilobites cannot be doubted; and that the analogy between them is the greater in proportion as we examine them at a less advanced period of their development. 3. That the Trilobites, as well as the Eurypteridæ and Pœcilopoda must be separated from the class Crustacea, and must form, with the Arachnida, a distinct division.

NOTES AND MEMORANDA.

A Medical Man's Pocket-Case for Specimens, of an ingenious and simple description, is that described by Dr. I. N. Durnforth in the first number of the 'Lens.' It consists of a thick slab of plate glass, in which six deep cells are excavated. The cover consists of another plate of glass, of the same size, and the two are fastened together by a strong rubber band. By means of this simple device specimens of pus, of expectoration, of mucus, or of solid tissues, may be easily transferred without being either dried or soiled; and if a drachm vial of carmine solution be also carried along, and a drop or two be added when the specimen is first obtained, it will be stained ready for the examination by the time the physician reaches home. Wrappings of any kind of cloth or paper are totally unfit for the transportation of specimens, as every microscopist very well knows. The compound cell, which he has described, furnishes a cheap and simple, as well as compact and portable, means of carrying as many specimens as any physician would be likely to accumulate in a single day.

A Modification of Dr. Matthews' Turn-table has been suggested by the originator. Dr. Matthews said, at a late meeting of the Quekett Club, that most of the members would, no doubt, recollect that he had produced a self-centring turn-table, nearly two years ago (May 27th, 1870), and exhibited it at one of the meetings. It was then pronounced to be excellent, and it remained excellent for new slides, but in most cabinets there occurred a necessity for revarnishing old slides, and cells on these were not always found to be central. In such cases this turn-table would only correctly centre them, and thereby show their eccentricity, and its accuracy thus became a defect, although it was a defect consequent upon its perfection. He had, however, now devised a remedy for this by dividing the top of the table into two portions, so arranged that by sliding the upper part upon the surface of the lower, any required degree of eccentricity could be attained. This was accomplished very easily and simply, and he thought that the arrangement rendered the turn-table as perfect as could be desired; certainly he did not himself see what more could be done to it. One of the improved turn-tables was then

exhibited to the meeting, and its utility shown by centring a slide which had been eccentrically mounted for the purpose.—*Quekett Club Journal*, April, 1872.

An Improved Mode of Observing Capillary Circulation.—Mr. H. L. Smith says that if we grasp a frog in the hand and plunge it in water about as warm as can be conveniently borne, say about 120°, though he has never measured this, judging simply from the apparent warmth to the hand, we shall find that, in a few moments, the frog will become perfectly rigid; it may now be removed and laid upon a plate for dissection. Carefully opening and stretching the parts by pulling upon the fore limbs gently, or even cutting the bones if necessary, the heart may be displayed, showing the contraction and expansion beautifully; and if now the animal is placed in warm water, the lungs will immediately float out, and by a suitably contrived stage the circulation may be examined. "It is better, however, not to do this, but to draw out gently the large intestine by means of blunt forceps, and then spreading the mesentery on the glass of the frog-plate (I find it convenient to use a large one with an elevated glass, instead of one in the same plane, on which to spread the mesentery) we can observe the capillary circulation very nicely with a $\frac{1}{4}$th or $\frac{1}{5}$th inch objective, by dropping a bit of thin glass over the place or with a higher power 'immersion.' Of course the parts opened must be kept moist and covered with a cloth, and a few drops of tepid water added from time to time. If the experiment has been properly conducted, the animal will remain perfectly quiet and the circulation will continue for hours; I cannot say how long, for I have never known it to cease until long after I had finished all the exhibition I have ever had occasion to make. If the frog is a large one, the mesentery can be spread out so as to afford the most magnificent exhibition of capillary circulation, with a distinctness, and under an amplification which will excite the greatest admiration and astonishment in anyone who has only seen it hitherto in the web, or the tongue. The objectives of a high power ought to be more tapering at the end than our American makers usually furnish them. In this respect some of the foreign objectives are superior. It would be very little more trouble to make the higher powers at the object end but little larger than the front lens, and thus infinitely more convenient for work than with the large flat surface which most of them now present. In fact, with a $\frac{1}{8}$th or $\frac{1}{12}$th American objective, as ordinarily made, it would be impossible to approach sufficiently near to the mesentery to focus on the smaller capillaries without striking some of the larger blood-vessels. If nothing more could be done, the front set at least might be mounted in a little projecting tip or nose, and if those who are ordering objectives will insist upon this, I doubt not the opticians will do their part."

The New Erecting Arrangement.—A writer who signs himself C. S., writes as follows to the 'American Naturalist' (April):—In the January number of the 'Monthly Microscopical Journal,' Dr. Ward describes "a new erecting arrangement especially designed for use

with binocular microscopes." The arrangement proposed by Dr. Ward will undoubtedly work as he proposes, but *cui bono?* It is an axiom in microscopy, as well as in other pursuits, that the simplest means of accomplishing an end is the best. Dr. Ward's arrangement is complicated and troublesome, and unless all the lenses are well made and carefully centred, definition will be injured. Dr. Ward is correct in his observation that the "*erectors usually furnished* [italics are his] are not good, and the use, otherwise satisfactory, of a good objective as an erector has not yet afforded the advantage of binocular vision." The first clause is correct, because "the erectors usually furnished" reverse, counteract, or destroy all the corrections which the opticians have taken so much pains to introduce into the objectives. The second clause refers to binocular vision. This has been completely accomplished by Tolles' binocular eye-piece, which has been in use and before the public more than six years. Without any change from, or addition to, its regular construction or use, it gives an image *erect*, *binocular*, and *stereoscopic* with any objective, from a 4-inch to a $\frac{1}{15}$-inch; and of course it may be used for dissecting by transmitted or reflected light with any objective having "working distance" enough for manipulation—certainly with a $\frac{1}{2}$-inch. The only objection, if it is one, against the instrument for this use, is that the "power," the amplification, is necessarily higher than with other binoculars. But where a *very low* power is wanted, I believe a pair of spectacles set with magnifying periscopic lenses will prove to be better than any binocular dissecting microscope yet devised. But the objection to the "usual erector" for monocular instruments remains. This was remedied by Tolles years ago; so long ago that he has forgotten when. He made erectors that did not disturb any of the corrections of the objective, but preserved them, and gave as good effects as were obtained without an erector.

A Note on the above Remarks, which we suppose to be by Dr. Ward, and which is signed R. H. W., says that it can hardly be necessary to state that Tolles' binocular eye-piece, with which the writer has sometimes worked, was ignored in the paper referred to, simply because there was no occasion to mention it,—it being no novelty, but an article whose properties have been perfectly familiar to American (and foreign) microscopists for years. It is only fair to add that the new arrangement, which can be added to any microscope at a cost of two or three dollars, has been used for months by several microscopists, who consider it extremely simple and convenient. That an erector (or anything else), however perfect, can be added to the objective and ocular, and give "as good" optical effects as would be obtained without the additional refracting and dispersing surfaces, is much disputed, and surely cannot be considered a conceded point at the present time. If Mr. Tolles is prepared to supply the market with erectors radically superior to those generally used, microscopists will doubtless learn the fact when it is announced, as he, R. H. W., does not find it now, in the catalogue of the Boston Optical Works or in their advertisements in the 'American Naturalist' and other journals.

Death of M. Alphonse de Brebisson.—It is with much regret that we have to announce the death of the distinguished naturalist, M. Louis-Alphonse de Brebisson, which took place on the 26th of April last. M. de Brebisson was a most distinguished naturalist. Though his absolutely microscopic work was small in amount, his additions to natural history were both numerous and important. He had reached his seventy-fourth year.

Death of Mr. Hodgson, F.R.M.S.—We regret to have to record the death, on the 4th of the month (May), of one of the oldest Fellows of the R.M.S.—Richard Hodgson, Esq., F.R.A.S., &c., of Chingford, Essex.

CORRESPONDENCE.

Sir David Brewster, Dr. Royston-Pigott, and the Photographic Camera Obscura.

To the Editor of the 'Monthly Microscopical Journal.'

Sir,—When I read in your Journal for the month of February a statement made by Dr. Royston-Pigott, that an "optical principle" had been laid down by Sir David Brewster involving as it seemed to me heretical doctrines, my curiosity was excited, and, as you are aware, I troubled you with a few lines upon the subject.

Now that I am, by the favour of Dr. Pigott's reply, put in possession of the words of Sir David, I must confess to a considerable amount of surprise, and I will add, of regret. As Dr. Pigott is responsible for the revival of what was generally regarded as a defunct controversy, I will, with your permission, address these remarks more particularly to him.

The doctrine to which I object is, that "Lenses of four inches aperture and upwards, when used upon a minute object, yield nothing but a mere jumble of hideous images, proceeding from upwards of 200 different points of the compass," &c.

I object equally to the doctrine that, to get rid of the "distortion in photographic images caused by the stereoscopic effect of such a lens, it is necessary, or proper, to reduce its aperture to that of the pupil of the eye."

With regard to the aberrations ascribed to lenses of four inches aperture and upwards, it may be observed that whatever aberrations may be created in any lens by rays of light diverging 150° as in the microscope, it does not follow that where the rays are parallel, or virtually parallel, as in the camera obscura (in which the divergency never exceeds a single degree), the same aberrations will take place.

In support of that view I may appeal to the object-glasses at the Greenwich Observatory, in which no aberrations of the kind exist;

and which are supplied to the most minute objects with perfect success.

It would be unbecoming in me to question any of the optical opinions of Sir David Brewster, but I think I may venture to break a spear with Dr. Pigott, even under the ægis of so eminent an authority.

It will be admitted, I presume, that the arguments of Sir David Brewster, so far as they relate to photography (of which alone I am treating), rest entirely upon the analogy between the camera obscura and the human eye; and I agree that there could be no better foundation upon which to proceed; but in reasoning out the parallel between the two, Sir David, by a most unaccountable oversight, has entirely overlooked the principal factor in the question, namely, the *length* of the ocular focus.

If the ocular focus be assumed as 0·8 of an inch, represented by a; the aperture of the pupil 0·2 inch, represented by b; the focus of the camera obscura 12 inches, represented by c; then it is evident that, in the human eye, the aperture of the pupil is one-fourth of the ocular focus; and the proportionate aperture in the camera lens would be $\frac{12}{4} = 3$ inches, not $\frac{2}{10}$ of an inch, as assumed by mistake.

Or, substituting the real or any other measurements, and adopting the symbolic but accurate and comprehensive language of Dr. Pigott, the proportionate aperture would always be $\frac{b\,c}{a}$.

I do not suggest that a trifling oversight of this nature will in the slightest degree affect the scientific character of Sir D. Brewster; but it vitiates the whole reasoning of which it is the foundation, and throws some light upon the justice and propriety of certain "*unsparing* criticisms."

Finally, I feel entitled to call upon Dr. Pigott, either to point out the error in my calculation, or to accept it as true. In either case, I apologize beforehand for any expression I may have used which may have given him pain.

Your very obedient,

G. S. Cundell.

P.S. If not encroaching too much upon your space, now that I have cleared my stomach of controversy, I should like to say a few words in extenuation of the faults of certain unskilful practitioners in photographic portraiture, who have brought so much obloquy upon their whole fraternity: I must, however, inform Dr. Pigott that I have nothing to do with that beautiful and now very important art.

Everything may be overdone, and certainly everyone must occasionally have seen in photographic portraits an undue prominence given to stereoscopic effect. I am, however, by no means in favour of abolishing it altogether, by the use of lenses of $\frac{2}{10}$ of an inch aperture.

Do we not, to our great advantage, see everything stereoscopically? Painters, I believe, paint all that they can see, and, as they use both

eyes, they must see, and paint, stereoscopic effects. Did not one of the most able and learned of them all announce the true stereoscopic theory 400 years ago, and practise its teachings, as all great painters have done ever since? Why, then, seek to exclude it from photographic portraiture?

In thus advocating a certain amount of stereoscopic effect, I would limit it to that quantity under which people see one another; that is, to the quantity due to a lens of 2½ inches diameter, that being the average space between the pupils of our eyes, which I may be permitted to call their "stereoscopic aperture." With such a lens, a head would be reproduced as we see it in nature, without the hard, cutting, lines of monocular vision, which all painters deprecate, and avoid.

Dr. Pigott will excuse me passing over a variety of subjects introduced into his letter: I confine myself entirely to a small branch of the optics of photography.

G. S. C.

Object-glasses.

To the Editor of the 'Monthly Microscopical Journal.'

Padnal Hall, Chadwell, Essex, *March* 4, 1872.

Sir,—In No. XXXI. of this Journal, for July, 1871, page 36, there appeared a short notice from Mr. Tolles describing an experiment intended to prove that a pencil of rays of more than 82° can pass from a balsam-mounted object with a wet-front or immersion lens. This has nearly escaped my recollection, and perhaps is forgotten by most of your readers. In the succeeding Journal, while giving Mr. Tolles credit for truth and honesty in the measurement of the angle shown, I pointed out how the error had arisen from a refraction caused by the position of the under hemispherical lens. Knowing that it is no easy task to bring this within the comprehension of minds not familiar with optical science, I took the pains to show, by a direct experiment, that the angle was considerably diminished by water. Canada balsam might have been tried in the same way, but I objected to injure my object-glasses by immersing their fronts in a tank of balsam merely for the sake of demonstrating a result which I thought the most obtuse could foresee. In water a dry-lens aperture of 170° was reduced to 100°. Mr. Tolles seizes upon this to show that I am wrong by my own experiment on the point—that a greater aperture than 82° cannot pass from a balsam-mounted object. Had the experiment been repeated with the front immersed in a tank of balsam in lieu of water, it needs no demonstration to convince those conversant with the laws of refraction that the aperture would then have fallen to below 82°, and further, though the aperture is 100° on an object *in* the water, yet when that object is in balsam as usual, and under the water, that the rays would again be refracted outwards, reducing the angle from the object to within 82°. Mr. Tolles is unable to perceive this, and so let it remain. Mr. Tolles has accepted the only

condition under which the full aperture can be brought to bear on a balsam-mounted object, *viz.* that of the tiny hemispheres. I am glad of his announcement that he has succeeded in this, and should like to see the same thing done in this country, particularly with large-aperture glasses, say higher than $\frac{1}{8}$th. But to end the matter, I must positively decline any further reply to Mr. Tolles on the question of the passage of rays through refracting surfaces, as it is futile for him, and a waste of time for others. I have merely to refer to his Fig. 2, page 117 of the last Journal, as my apology for this. Starting from the focal point, and accepting his position that the refractive power of the covering glass, interposed medium (say balsam instead of water), and front lens are nearly the same, the rays may be taken as straight till they reach the outer convex surface: and here is a gross error permanently recorded in black and white, for at the angle at which they are made to reach that surface, instead of emerging in the direction he has shown, the refraction would be so small that they would proceed more nearly in a straight line, so that no possible back combination at present known could take them in, or in due course bring them to a focus behind.* The science of optics ranks amongst the most exact of all, and no fanciful direction of rays is admissible. In the object-glass question there have been imaginative diagrams *ad nauseam*, when all deductions should have been taken from drawings of the real thing. Mr. Tolles has made combinations that have done well in practice, and if he will give us the correct radii, diameters, distances, &c., of any one combination that has been actually constructed, and transmitting a full aperture in balsam, and show us the passage of the rays through to prove the result, then I will strike my colours, or if he will furnish the drawing, the rays shall be traced through for him. Nothing beyond what has been known before, has yet been learned by the correspondence, unless the fact that in America the *single front*, differing in thickness and slightly in radius, is now employed both for the dry and wet lens in their best object-glasses. I fairly claim to be the originator of this system. I made but one object-glass with a triple front (in the year 1850). I then found that the radii of the contact surfaces of this gave a large excess of over-correction or *negative* aberration for parallel rays, and under-correction or *positive*† aberration with divergent ones. This induced me to project the mean refractive rays through the entire combination drawn as a large diagram, which analyzed the cause, and led at once to the single front with a thickness proportioned to correct the aberrations. Some particulars of this are given in my paper "On the Construction of Object-glasses," commencing with this Journal. I am happy to find that this formula has now become almost universal, and that the triple front may be considered as

* Fig. 1 is still more erroneous. In the dotted-line lens of Mr. Tolles, shown within the copy of my front, it may be seen at a glance how his dotted ray will emerge nearly as a radius.

† These terms are so thoroughly known and descriptive of the conditions of the marginal rays both in chromatic and spherical corrections falling without or within the central ones, that I am not disposed to reverse them on any theoretical ground, or that they are "rule of thumb employed in the workshop."

obsolete. At the time the belief was slow, though in the few years following I had made a $\frac{1}{4}$th, two $\frac{1}{5}$ths, two $\frac{1}{12}$ths, one $\frac{1}{25}$th, and one $\frac{1}{50}$th, all on this system, and which were duly mentioned by the President of the Microscopical Society in his annual report at the time. I was always glad to challenge comparisons, in order to prove the superior advantage of this construction, yet it has only recently been adopted by some of our most eminent English makers; and however this may now, in some slightly-modified form, be announced as a "newly-discovered system," yet the principle remains the same, and like in all exact sciences there is but one plan of perfect construction.

I have reason to believe that I am now unfolding a rigid law in respect to the relative foci and correction of the front and back combinations, that applies equally to the $\frac{1}{2}$ inch and upwards in all adjusting glasses. It has the merit of simplicity to recommend it, and as soon as I can develop and define its conditions, I will give the data from which I anticipate that mathematicians can work with advantage to the science.

Yours very truly,

F. H. WENHAM.

DR. PIGOTT AND MR. HENNAH.

To the Editor of the 'Monthly Microscopical Journal.'

MILTON HOUSE, CLARENCE STREET, BRIGHTON, *April* 17, 1872.

DEAR SIR,—I cannot allow myself to be condemned by default, as would be the case if Dr. Pigott's note, at page 173 of the last number of the Journal, remains uncontradicted.

Will you kindly afford me space in your next issue to state that the photographs alluded to by Dr. Pigott are the same as those presented to the Society in March, 1871. They were the result of *original* experiments, and so far were they from being taken from Dr. Pigott's rods, or in any way in connection with him (as he ingeniously suggests in the note), that *I* arranged the rods and took the photographs, and then sent them to Mr. Curteis, who, for convenience of display at the March meeting of the Society, further mounted them in a chromatrope frame before he placed them in Mr. Hogg's hand.

I never heard of Dr. Pigott or anyone else having experimented in the same way, until *after* my experiments had been published in London; and I can prove that Dr. Pigott, when he wrote his note, was fully aware that the photographs were from rods mounted by *me*.

I am sorry so far to occupy your space, but as the photographs and rods were submitted to the Royal Microscopical Society, as *original* illustrations in proof of the difficulty of determining structure—as well as a caution against too readily accepting Dr. Pigott's statements, I must, in justice to the Society, as well as to my own position, distinctly assert the originality of experiments undertaken to ascertain

the value of the beaded appearances on which was based an attack on our observations and objectives, which is still, in my opinion, most insufficiently supported.

I will not now do more than express a hope that Dr. Pigott's record of personal observations is more precise than his statement of facts.

I am, dear Sir, faithfully yours,

JNO. H. HENNAH.

MR. ROSS'S OBJECTIVES.

To the Editor of the 'Monthly Microscopical Journal.'

GROVE HOUSE, NEWBOLD STREET, LEAMINGTON, *April* 4, 1872.

DEAR SIR,—To the numerous readers of the Journal who, like myself, look out for its successive appearances with expectation and pleasure, perhaps this note may not be without its interest. I have no doubt we all feel a degree of justifiable pride in the name of Ross, as historically connected with the achromatic microscope, and regret the loss of father and son, who kept so well to the front in the constant march of improvement. I am happy to say there is every probability of the old House keeping up its well-earned reputation. A few days ago I received a box of objectives from the present firm, which were so superior that I feel it to be only an act of justice to call attention to them. The $\frac{1}{10}$ths surpassed any other glasses of like focus I ever received from them, and we all know how T. Ross prided himself on this combination: the beauty of their performance was equalled only by their *facile* resolution of tests ever considered difficult for this power. Another glass of remarkable character and excellence was a "new" $\frac{1}{5}$th, which acts most beautifully as a dry lens, and by manipulating the adjustment, *without change of front*, performs admirably as an immersion lens. This double performance as dry and wet lens is shared, as far as I know, only by Powell and Lealand's beautiful $\frac{1}{5}$th. This $\frac{1}{5}$th resolves the *Cymatopleura elliptica* of Möller's Probe Platte. I would here remark that Möller having used such a thick slide to mount the diatoms on, prevents the proper illumination requisite to bring out the best results. I found by gently warming the slide I could remove the cell containing the diatoms. This I did, and remounted it on very thin glass, with the best result, and without disturbing in the least the beauty or perfection of the test-plate. All Ross's glasses, I understand, are now worked from new formulæ, which promise a still further point of excellence: they have a new $\frac{1}{20}$th under construction, from which much is expected, and no doubt justly so.

I am yours truly,

THOMAS BIRT, M.D.

PROCEEDINGS OF SOCIETIES.*

Royal Microscopical Society.

King's College, *May* 1, 1872.

W. Kitchen Parker, Esq., F.R.S., President, in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations was read, and a vote of thanks passed to the respective donors.

The Secretary called the attention of the meeting to a simple and inexpensive method brought forward by Mr. Richards, of adapting a second objective by its being screwed into a brass plate and placed on the stage, and centred by the stage movement after the plan proposed by Mr. R. H. Ward, M.A., for erecting the image when using the microscope for dissecting purposes. This plan was referred to in the January number of the Transactions.

A communication was read from Dr. Beale in reply to the criticisms of Dr. Klein upon his (Dr. Beale's) recent paper "On the Relation of Nerves to Pigment and other Cells or Elementary Parts."

Mr. Hogg said he thought Dr. Beale laboured under some misapprehension with regard to Dr. Klein's paper published in the Transactions. He believed Dr. Klein had no intention of claiming more for German physiologists than the discovery of the fine nerve fibre in connection with vessels and other structures contemporaneously with Dr. Beale, and it would appear therefore they had some claim to be regarded as discoverers in a field of research, which had gained not a little from Dr. Beale's investigations. Dr. Klein showed that Remak in 1843 first described "a terminal network of nerve fibres," and that investigations on nerve structure had been carried on from that time to the present by his countrymen. He also showed that the carmine process failed to bring out clearly the finer nerve fibres, while the chloride of gold process, now so much employed by the German school, demonstrated in a most satisfactory way the finer nerves of the epithelial layer of the cornea, as well as those permeating deeper seated structures; and, what was still more important, these finer nerves could be seen by the use of low powers. A power of 250 diameters would show even the varicosities of the minutest fibres. He would refer to Dr. Klein's mode of making his preparations, which he thought of some importance. He (Dr. Klein) first passed a fine silk thread through the cornea of the living frog, to set up a certain amount of inflammatory action, and as soon as possible after death the cornea was removed and steeped in the chloride of gold solution. In this way the solution permeated thoroughly every portion of the corneal structure. The light, subsequently acting upon the connective tissues, stained them a beautiful violet colour, while the nerves were left

* Secretaries of Societies will greatly oblige us by writing their report legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. 'M. M. J.'

quite black, and seemed to stand out in bold relief when examined with the binocular. He (Mr. Hogg) had had an opportunity of examining quietly Dr. Klein's specimens, and he must say they were very beautiful and wonderfully clear.

The President then read a paper "On the Difference between the Skull of the Thrush, as one of the highest classes of Birds, and the Skull of the Common Fowl."

Dr. Murie asked the President what was the difference between the structure of the bird's jaw, and that of the mammal, and especially that of the human being.

The President then described by means of a series of diagrams the mandibular arch, as it exists in the mammal, the bird, and the fish. He drew the attention of the Fellows to the unity of the plan of creation, exhibited by the metamorphoses which took place in the development of animal structures.

Mr. Slack said they could all highly appreciate the philosophy of such a paper as the President had just read, although but few might be able to follow the whole of the details. They could not help being very much struck by the way in which science has recently enlarged the ideas of men with regard to the unity of the plan of creation. He thought also that many would be disposed to see a striking confirmation of the truth of the views held by the Darwinian school. He begged to propose a vote of thanks to the President, which was carried unanimously.

The President, in acknowledging the vote, said he wished it to be distinctly understood that the whole of the work of which he had described the results was microscopic work. It was all the result of observation made through the microscope, although perhaps not with the highest powers.

The President asked Dr. Braithwaite whether he could not, as a botanist, confirm the value of the method he (the President) employed in his investigations.

Dr. Braithwaite replied he could most certainly do so. The botanist began with the lower forms of plants, and traced them up to the higher forms, carefully marking their affinities, and taking a proper view of the structure. As he ascended, he would notice that the cells of the lower plants underwent extraordinary metamorphoses, which, however much they might seem to indicate that the vessels found in the higher could not originate from simple cells; that they do so is nevertheless proved by the fact that in the embryos of the highest forms of plants nothing but cells are to be found.

Mr. Wenham then read a paper "On an Improved Reflex Illuminator for the Highest Powers of the Microscope."

Mr. Brooke said it seemed to him that Mr. Wenham had described a much more manageable plan of illuminating an object by rays obtained by total reflexion, than that which he had himself proposed many years ago, or even than that which Mr. Wenham had brought forward some time since. The plan in question was one by which reflected rays could be more readily brought to bear upon a minute object than anything else that had yet been introduced to the notice of the Society.

A vote of thanks was then given to Mr. Wenham.

A paper was read from Dr. Anthony, F.R.M.S., containing further observations on battledore scales of butterflies.

Mr. Wenham said some of the scales he had examined were remarkable for their thickness. They appeared to have an internal structure, which led him to conclude that there was something more than inflection which kept those scales in form. He should much like to know how they were situated. Sometimes they stood up like hairs on the surface of the scale.

Mr. McIntire said, in the scale which Dr. Anthony had figured, and which was then under the microscope, the little tubercular bodies were unmistakable. He was inclined to think that in these particular scales they lay between the membranes; but of their particular functions he could not possibly express any opinion. In one or two of the scales under notice, when pressure had caused the tubercles to lie on one side, the dome-shaped top of the tubercles and the base and the neck could be distinguished. On the other battledore scales he was certain they were there also, but whether inside or outside the membrane he could not be certain. In *Alexis* they seemed to be definite elevations on the inner surface of the scale (that is, on the membrane next the wing of the insect). He did not think any elevations were to be found on the exterior surface. He had never examined the battledore scales during the life of the insect, but had never experienced any difficulty in examining them *in situ* on a dried wing as opaque or transparent objects.

Mr. Slack said he had examined a great many of these battledore scales with which he had been supplied by Dr. Anthony and by Messrs. McIntire and Wonfor, and he was disposed to think that the view held by Mr. Wenham respecting their structure was substantially correct. He had tried the scales in all manner of ways, looking at them under glass, and again as uncovered objects, and under each condition they presented most vexatious difficulties. They seemed to him to be bags, and in dealing with them the ordinary modes of illumination utterly failed. With a great many objects that really presented elevations there was no difficulty in examining them by means of unilateral light, and ascertaining what the true structure was; but directly this mode of illumination was applied to these objects, their whole appearance became confused. If illumination was effected by a small central pencil of light, such a plan would doubtless facilitate getting certain effects, but the total absence of shadow left the observer in considerable doubt. It moreover placed him somewhat in the position of looking down upon a small object so as to produce much foreshortening. He had once tried some of the scales entirely uncovered, and he came to the conclusion that upon those were to be seen little bumps. If you focus down with the fine adjustment, upon these battledore scales the first thing seen was the outline of a bump on the upper surface of the scale, and he believed that on the membrane below was a similar bump. Mr. Wenham had noticed something like a columnar form, and if you imagined that these two membranes, the upper and lower, are separated by little hollow pillars, rounded

at the top and bottom, you got something like what Dr. Anthony figured, but inside instead of outside. Dr. Anthony says he had used polarized light. He (Mr. Slack) had tried polarized light, placing the polarizer under a $\frac{4}{5}$ condenser, and fixed the analyzer just over one of Powell and Lealand's immersion $\frac{1}{8}$ths. With that he got abundance of light. When the prisms were so arranged as to give a perfectly luminous field, the tops of the supposed pillars permitted the light to pass freely, and they looked white, with a little shading off at the edges. By turning the prisms so as to get a perfectly dark field, all the thick parts of the scale became brilliantly luminous. The margins or rims of the supposed columns were quite bright, and the centres dark. Hollow pillars covered with thin membranes might produce these effects. Among some peculiarities he had noticed in the most perfect scales series of ridges or ribs. One of the scales viewed as an uncovered object suggested the idea that the true form of the markings might be that the columnar pillar referred to was formed by bifurcations of the ribs, the ribs opening and the two halves uniting again so as to make hollow columns.

Mr. Hogg, referring to Mr. Wenham's remarks, said he thought Dr. Anthony looked upon these peculiar battledore scales as somewhat allied to glandular structures in animals. This species of butterfly was mostly found "on the banks where the wild thyme grows," and it was quite probable that they sucked the essential oil from the flowers of the plant, and stored it up in these scales, which occupied an intermediate position beneath the more brilliantly-coloured scales, and the membraneous structure of the wing. It was somewhat remarkable that these scales were only found on the wings of males. This would seem to favour the notion that the oily fluid stored up in them might in some way contribute to the greater brilliancy of the plumage of the males and cause them to be more attractive to their female companions. Mr. Hogg took exception to some of the terms employed by the various speakers, as those of "tubules," and "tubercular elevations," which did not convey the same meaning, although drawn exactly alike. Mr. Slack, he thought, was right in his representation of the structure in question, and in speaking of it as "a tuberculated membrane," that is, a raised part of the membraneous portion of the scale. Whereas, if Dr. Anthony's representations were correct, the structure consisted of a stalk or pedicle, terminated by a nodulated extremity projected out from the surface of the membrane. In such a case it could not be called *tuberculated* in the sense in which the term was usually employed.

Mr Stewart said he had lately examined some dry specimens of the battledore scales of *Polyommatus Alexis* and of other Lycænidæ. He quite agreed with Mr. Anthony as regarded the shape of the stud-like bodies, but thought they were situated between and not outside the membranes of the scale.

The battledore scales of *P. Alexis* were spoon-shaped, having the hollow of the bowl directed towards the membrane of the wing; they were inserted into the wing at an angle of 45°, close to the point of attachment, and underneath the larger scales.

In no position of the scale could he see the studs as upstanding processes, and in torn and also in uninjured scales which lay on their outer surface, the first thing seen on carefully focussing down was the thin and sometimes finely-beaded deeper membrane of the scale and the head of the stud; a slight alteration of focus would next bring into view the base of the stud, the coarse ribs, and the delicate superficial membrane. He consequently believed the studs to be buttresses or pillars, whose bases were attached to the superficial costate membrane, and whose rounded heads were fixed to the deeper membrane. If this interpretation be admitted as true, he thought that the structure in question might give strength to the scale. An analogous instance might be seen in the wing-case of many beetles (Dytiscus, &c.). In this case pillars connected the two membranes of the wing, apparently to give strength, and to afford a space in which the trachea, &c., might ramify; these pillars had been mistaken by some for modified crystals, but their real nature could readily be detected by examining a vertical section of the wing-case.

He fancied that the purpose served by the scales referred to might be to maintain at a favourable angle for catching the light the large iridescent scales; indeed, that they might be compared to the true tail feathers of the peacock, which are short, strong, and plainly coloured, being used to support the brightly-coloured feathers of the back, which constitute the false tail.

Donations to the Library and Cabinet from April 3rd to May 1st, 1872:—

	From
Land and Water. Weekly	*The Editor.*
Nature. Weekly	*Ditto.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal	*The Society.*
Journal of the London Institution, No. 14	*The Institution.*
Popular Science Review, No. 43	*Editor.*
President's Address and Report of the West Kent Natural History Society for 1871	*Society.*
Der Vogelschutz. Von G. R. V. Frauenfeld	*Author.*
Die unseren Kulturpflanzen schädlichen Insekten. Von Gustav Künstler	*Ditto.*
Ueber die Weizenverwüsterin Chlorops tæniopus Meig und die Mittel zu ihrer Bekämpfung. Von Professor Dr. Max Nowicki	*Ditto.*
Die Grundlagen des Vogelschutzgesetzes. Von G. R. von Frauenfeld	*Ditto.*
Die Pflege der Jungen bei Thieren. Von G. R. von Frauenfeld	*Ditto.*
Verhandlungen der Kaiserlich-Königlichen Zoologisch-botanischen Gesellschaft in Wien, 1871	*Society.*
Bulletin de la Société Botanique de France, 1869–70 and 1871	*Ditto.*

Edward Harris, Esq., was elected a Fellow of the Society.

WALTER W. REEVES,
Assist.-Secretary.

Brighton and Sussex Natural History Society.*

February 8th.—Ordinary Meeting. Mr. Hollis, President, in the chair.

A very pleasing diversion to the business of the evening took place in the shape of the presentation to Mr. J. Colbatch Onions, one of the Honorary Secretaries, of a handsome dining-room clock, with visible escapement, striking the hour and half-hour, furnished with perpetual calendar, and bearing the following inscription:—"Presented to J. Colbatch Onions, Esq., by the Members of the Brighton and Sussex Natural History Society, as a mark of their esteem and gratitude for his services."

Messrs. C. Carpenter, F. Smith, F. W. Salzmann, and J. Schweitzer, were elected ordinary members.

Mr. Wonfor announced the receipt, for the library, of the 'Proceedings of the Linnean Society,' 1871–72, and No. 54 of the Linnean Society's Journal, from Dr. Addison; 'Observations on the Climate of Brighton,' by Mr. F. E. Sawyer, from the Author, and the 'Fauna of Devon; 'Crustacea, Podothalmata,' the 'Parasitism of *Orobanche major*,' 'Experiments on Spontaneous Generation;' and the 'Boring of Mollusks, Annelids, and Sponges into Rocks, Wood, and Shells,' by Mr. E. Parfitt, from the Author.

Votes of thanks were given to the donors; to the Dispensary Committee, for the use of the Board-room for the soirée; and to Messrs. R. Glaisyer, Penley, G. Scott, C. P. Smith, Saunders, Walsh, and Wonfor, for their valuable services in arranging and carrying out the soirée.

Dr. Addison, F.R.S., then read a paper "On the Natural History of Cures and Healing."

The inquiry, "What is healing?" involved us in others. What is growth? especially as respects the first formation of blood-vessels in the embryo. What is inflammation? If dust entered the eye, inflammation arose; if a thorn pierced the finger, and, breaking, left a piece behind, there would be inflammation and suppuration; if a dead part had to be thrown off, the phenomena were inflammation, suppuration, and granulation. Granulations were characteristic of healing. They consisted of myriads of newly-formed blood-vessels, and the coats of the stomach were composed of globules or cells, not distinguishable from the colourless cells of blood. It was in a vast assemblage of such globules or cells, that new vascular tissue, new blood-vessels, were formed. Exactly the same materials and phenomena as might be seen at the earliest period of growth.

Some years ago an American gentleman exhibited a Hydro-incubator, in London, and allowed him, Dr. Addison, to purchase eggs at every period of incubation, and to examine them. On one occasion an egg was opened after seventy hours, when the parts were seen covered by innumerable embryonic vessels, which had nothing but globules in their structure. Without any other disturbance, a watch-glass was placed over the broken part of the shell, and the egg

* Report by Mr. T. W. Wonfor.

returned to the incubator. Twenty hours after, a great number of new and collateral vessels, strangely divergent from the natural ones, had been formed, to meet, as it were, the damage done by the premature breaking of the shell. All these vessels had nothing but globules in their composition, and were easily obliterated by moving the point of a needle in their midst.

Other facts of healing might be compared with embryo growth. If a tendon was ruptured, or a bone broken, great heat attended the first stage of reparation. In all parts the heat of the first stage of healing was above that of the neighbouring parts. These facts correspond with the heat necessary for the growth of the first blood-vessels in the egg. Blood-vessels of the embryo were destroyed by the gentlest handling, so likewise the new vessels in a healing sore, bled at the lightest touch.

In the embryo, colourless elements of blood constitute the first vessels; so, also, in the new vessels of healing parts. All parts of the embryo were more vascular than afterwards, when growth was further advanced: so with healing parts, granulations were, at first, more vascular than they were when turning into fibrous tissue. It took longer to repair a broken bone than it did to repair a ruptured tendon, which again was longer in healing than a wound in the skin, because in embryo growth, tendons were completed later than skin, and bones later than tendons.

It was well known that inflammation preceded granulation: what had it to do with healing? When fully-formed blood-vessels were called upon to contribute to healing, a change took place in their coats from elastic fibrous to fragile globular tissue, a kind of retrogression to the embryo state. The change might be said to do them violence, as it altered their state of cohesion and elasticity to one of fragility. Hence the pain of inflammation, demanding quiescence in parts preparing for healing. New blood-vessels for reparation could not possibly partake of the circulation without this change in the coats of the existing vessels. At the commencement of inflammation, blood has been seen depositing its colourless globules upon the interior of the vessels; these globules or cells, gradually accumulating, at length interpose between the stream of red blood and the elastic coats of the vessels, and substitute a fragile globular form or fibrous tissue. Vessels so altered were virtually embryonic, prepared to set off new vessels and new growths. Inflammation was the necessary preliminary to granulation. The two main constituents of the body were the solid parts and the blood. Injury to the latter was as fertile a source of inflammation as to the former. In extreme parts the antecedents of an injury could be seen, and the form, extent, and gravity of inflammatory action was measured by the triviality or gravity of the damage done. But in all that relates to blood-poisoning, the antecedents and the amount of damage done were hidden from view, but inferences might be drawn conversely. When small-pox was passing away through a moderate number of kindly pustules, and scarlet fever through a series of cuticular exfoliations, and in other fevers, when the crisis was moderate in type and duration, we might infer that no great damage

had been done to the blood, and that nature had found a good outlet for disordered and disarranged elements, the antecedents or cause of the fever.

When great mortality attended fevers or the dangerous ordeal of inflammatory abscess, suppuration and ulceration had to be passed through before recovery—much greater damage was implied, a greater augmentation of the fever poison, and a greater difficulty in eliminating it. There could not be a shadow of a doubt that blood was liable to as many accidents as the solid parts, and that inflammation was a struggle of nature to set matters to rights—a struggle which might be described as a more or less complete return of blood and blood-vessels within its area to the early stages of embryo growth. If an injury to a solid part was slight, no retrogression of blood-vessels took place. The blood rushed to the part, but there was nothing to be repaired; the injured parts recovered themselves, and there the matter stopped—this was simple inflammation. If more damage had been done, then the blood-vessels returned to the embryo state, and according to the gravity of the damage and the healing work to be done, so was the need of surgical help urgent. Likewise, in trivial cases of blood disorder, if the morbid humour could be worked out without material change in the coats of the vessels, the action was comprehended under the term rashes, pimples, &c.,—examples of simple inflammation. But in graver examples of blood-poisoning, when blood-vessels must retrograde to satisfy the needs of the damaged blood, the action was comprised under other terms, all referring to different degrees of the physiological work designed for the cure of local injuries, blood-poisoning—a physiological work amply vindicated its place in Natural History, without in the least degree diminishing the need of the experienced superintendence of the physician in regulating, controlling, and diverting the various forms in which it appeared in blood disorders.

Reading Microscopical Society.*

March 6th, 1872.—Captain Lang presided.

Mr. Tatem contributed a short paper "On a Presumed Form of Actinophryan Life." In it he referred to swarms of germs which eventually assume a spinose form, and have a forward movement effected by means of a vibrating filament. After some time these settle down, flatten themselves, and, throwing out pseudopoda, become *Actinophrys sol*. In this stage they were said to be very voracious, eating largely of *Astasia margaratifera*. By subsequent change the pseudopoda disappear, and the earlier forms are resumed; but these the writer believed to be, in reality, advanced forms.

Captain Lang exhibited a slide of grouped diatoms presented to him by Mr. Cole. This contained forty-nine forms, systematically disposed, and it would be difficult to find a fault in their arrange-

* Report supplied by Mr. B. J. Austin.

ment, perfection as to individual valves, or in the general brightness and cleanliness of the mounts.

He also brought for the inspection of the members a large collection of slides, kindly lent by Captain Perry, of Liverpool, principally selected diatoms, many being excessively rare and valuable. The collection also included many Foraminifera, scientifically arranged, and a beautifully-grouped slide of spicules of *Hyalonema*.

Mr. Tatem exhibited mounts of ant lion (*Myrmelion formicarium*) and of the tick of red deer.

The Croydon Microscopical Club.

Soirée.

[We regret that this and other reports have stood over so long; but it was quite impossible to insert them earlier.—Ed. 'M. M. J.']

The second annual soirée under the auspices of the above Club, was held on Wednesday evening, November 8th, 1871, in the Public Hall, which presented the same gay and brilliant appearance as characterized their first soirée last year.

Appended is a list of the more noticeable objects sent for exhibition:—

The President (Mr. H. Lee), in addition to six microscopes, contributed two emeu's eggs, laid in the grounds of W. H. Peek, Esq., M.P., of Wimbledon; sections of a very fine ammonite, from the lias of Lyme Regis, Dorsetshire; and casts of trout, painted by H. L. Rolfe. His microscopic objects chiefly consisted of fossil wood in coal; fructification of marine algæ; crystals in colloid silica; stomach teeth of Nereis, a sea-worm; anatomy of star-fishes; and embryo crustacea.

Mr. Frank Buckland sent for exhibition a skin of a Polar Bear, recently shot by Captain Gray, in the Arctic regions. The preserved skin of the monster was 8 feet long, and more than covered the large table on which it was spread. There were also a nondescript monster, constructed by the Japanese of papier-maché and wood, with a photograph of a similarly artificial animal; and a bottle containing a rat, preserved in spirits, whose history was a somewhat singular one. It had pushed its head through a ham bone, and being unable to extricate it, it lived for many months with the somewhat novel collar around its neck. This formed the subject of two humorous contributions by Mr. Lee, to the columns of 'Land and Water,' in 1867, which are well worthy of being reprinted, but which, of course, have no rank in a purely Scientific Magazine.

The treasurer to the club—Mr. J. W. Flower—was unavoidably absent, but his son, Mr. J. Flower, exhibited a series of skulls of quadrumana, or the monkey tribe.

Dr. Carpenter's collection of flints found in Croydon, attracted a large share of attention. In some of these flints were to be found beautiful specimens of the echinus. Here were also fossils from the

Antwerp Crag, and others from Swanage, and imbedded in the latter were to be seen perfect specimens of willow leaves. Some extraordinary shells of the *Malleus vulgaris*, and a gigantic oyster-shell, from India, with two cases containing beautiful specimens of marine Polyzoa, neatly mounted by Mrs. Bury, of Thornton Heath, were greatly admired.

Mr. Whitling exhibited a microscope and a graphoscope, and with the latter some very beautiful views.

Dr. Adams contributed largely to the Natural History department by sending specimens of the Australian Diver, the native bear of Australia (Koali, or "biter"), the duck-billed Platypus, or water-mole, the Echidna, or porcupine ant-eater; calabash, from the West Indies, Australian war-clubs and spears, some of which had been lent by Mrs. Lockyer, who also sent some Chinese chop-sticks. A wag, on observing that no object had been placed in Dr. Adams's microscope, wrote on the card which bore his signature, "*Ex nihilo nihil fit*," which caused no little amusement, in which the Doctor participated when he discovered the joke that had been perpetrated in his absence.

The cases of butterflies which had been artistically arranged in imitation of a flower garden, by Mr. Cooper, gardener to Mr. Paget, were greatly admired, not only for the harmonious blending of colours, but also for the taste displayed in disposing of these handsome insects to the best advantage.

Mr. Thomas Cushing exhibited three microscopes with interesting objects, amongst which was a most beautiful collection of natural plants; twenty-four photographs of scientific instruments constructed for the great trigonometrical survey of India; and a differential polariscope, which fully merited the large share of attention it received.

The following members of the Croydon Club also exhibited microscopes:—Mr. H. Ashby; Mr. J. Berney; Mr. A. Crowley; Mr. P. Crowley; Mr. H. Noakes; Mr. T. Brindley; Mr. E. Sturge; Mr. F. C. Clark; Mr. Loy; Dr. Owens (circulation in *Vallisneria spiralis*, a very beautiful object); Mr. F. West, jun.; Mr. McKean, jun.; Mr. W. H. Snelling; Dr. Strong (sections of wood); Mr. Sigsworth (antennæ of cockroach, polarized); Mr. C. W. Hovenden (three microscopes and a collection of old and rare coins); Mr. Cooper; Mr. Linney (an illuminated copy of the ten commandments under the microscope); Mr. Perry (some quaint old Chinese etchings); and Mr. Spencer, who exhibited a crystallized lens of the eye of a cod-fish, with regard to which some curious calculations were made by Sir David Brewster on measuring the object. He found that the number of fibres in each spherical coat was 2500; the number of teeth in each fibre, 12,500; number of teeth in each spherical coat, 31,251,000; number of fibres in the whole lens, 5,000,000; and number of teeth in the whole lens, 62,500,000,000. Mr. H. Noakes displayed, under the microscope, some foraminiferous sand; Mr. J. S. Johnson, *Deutzia scabra* (polarized light), and a beautiful collection of small shells from Devonshire; Mr. H. Lee, jun., petal of geranium; and Mr. Manners, wing of *Ornithoptera Richmondii*.

Commencing at the centre table, the first thing that struck the

visitor was an extraordinary double-erecting binocular, which enabled two persons to examine the same object at once. This instrument, exhibited and invented by Mr. J. W. Stephenson (of the Council of Royal Microscopical Society), was considered to be quite a novelty and a speciality of the evening. Captain Tyler displayed sections of Japanese glass-rope sponge, and many other rare specimens of sponge, both silicious and calcareous. Dr. Millar had on view several beautiful objects; the Rev. T. Wiltshire and Mr. Hilton displayed admirably-mounted objects—spicules in flint, and spicules out of chalk. Mr. Burr contributed a series of photographs showing the different phases of the moon; Mr. Butler exhibited a beetle's eye, magnified 400 times, on every facet of which was to be seen the second-hand of a watch, and as each of these hands moved simultaneously, some speculation arose as to the cause of this somewhat singular phenomenon. It was courteously explained that the eye was transparent, and that the second-hand of a watch had thus been reflected upon each facet, which produced this amusing optical delusion.

Dr. Braithwaite, Vice-President of the South London Microscopical and Natural History Club, exhibited a palate of a water snail; Mr. Ackland, a group of young oysters, shown by the aid of a new side-light reflector; Mr. F. Hovenden, one of the Infusoria; Mr. Robinson, an admirable specimen of native gold; Mr. Cottrall, a five-pound Bank of England note, magnified to its normal size; and a collection of selected diatoms. Mr. How (inventor of a new and improved microscope lamp with porcelain shades) exhibited the pollen of the marrow; Mr. Rogers, abdomen of a foreign bee, which excited much attention; Mr. Jackson, two admirable mountings of a fungus on an elm leaf, and wing of a Brazilian butterfly; Mr. Neighbour, a number of brilliant glass stereoscopic views, principally illustrative of Swiss scenery; pieces of mignonette and chrysanthemums, splendid objects.

Mr. Alpheus Smith, of the Quekett Club, exhibited specimens of the microscopic moth, *Cemiostoma laburnella*, and Salicine; Mr. Asbury Green, palpus of cardinal spider; Mr. Quick, foot of Dytiscus, toe of mouse, and head of gnat; Mr. Groves, a particularly good specimen of crystals of chlorate of potash; Mr. Burgess, section of yellow water-lily and group of young oysters; and Mr. Gay, the new Nudibranch (*Embletonia Grayii*); and the same in a larval state.

Mr. T. Charters White, of the Royal Microscopical Society, exhibited a young crab; Mr. W. T. Suffolk, lips of blowfly; Mr. J. Smith, Barbadoes Polycistina; and Dr. Millar, floreated spicules of Euplectella, *in situ*.

Mr. E. George, of the Forest Hill Microscopical Club, exhibited a specimen of the fructification of Chara; Mr. Martin Burgess, a foot of lady-bird; Mr. Burt, *Dicksonia Antarcitica*; Mr. J. R. Furneaux, parasite of canary; Mr. Thomas Rabbits, leg of diamond beetle; Mr. Goddard, Polycistina; Mr. E. F. Jones, a microscope of the last century, and dental plates of *Ophiocoma rosula*; Mr. Westbrook, larva of water newt, showing circulation of blood.

Amongst the makers of optical instruments were Mr. W. F. Stanley, of London Bridge, Messrs. Murray and Heath, Mr. James

Swift, Messrs. R. and J. Beck, and other celebrated makers, who exhibited instruments as perfect as the modern researches of science would permit, and some of the objects shown were very beautiful.

Taken altogether, the general opinion appeared to be that the exhibition of scientific objects was better than that of last year; and that greater interest is taken in scientific pursuits, owing to the establishment of the Croydon Microscopical Club, was evident from the increasing desire of the visitors to have the objects thoroughly explained to them; and it is only due to those gentlemen who exhibited, to state that they were untiring in their efforts to enlighten every thoughtful and anxious inquirer.

Microscopical Section of the Liverpool Medical Institution.*

The first meeting of this, the third session of the above Section, was held on October 20, 1871.

Mr. Hamilton in the chair, twenty-four members present.

This Section of the Liverpool Medical Society meets once in each month, and the subjects brought under its consideration are medical in character, or relate to medicine.

At this meeting the chairman urged each member of the Society to assist in prosecuting microscopical research, and added that the Council of the Society have voted a certain sum for that purpose, a room was being furnished in which members might work with the microscope and mount specimens.

Mr. Hamilton then read his paper on "The Identity of Life in its Earliest Manifestations."

The object of the paper, stated the author, is only tentative, a proposal, as the result of certain experiments to look for the origin of the earliest forms of life as a sequence of the disintegration of a compound life,—in a somewhat different manner than has hitherto been done. It is intended further to illustrate that the decomposition of organic matter, however varied in its kind, shows in the first stages of that decomposition similar forms (under the microscope) of rudimentary life. There is therefore a stage in the breaking up of all organic structures when they assume identical shapes.

The author's experiments consisted of two series. In the first of these, he placed portions of various kinds of animal and vegetable structures severally under conditions favourable for decomposition, and from the decomposition of each form of living thing he saw the same kind of animalcule spring. In the second class of experiments trial was made of solutions of carbolic acid, sulphurous acid, sulphate of copper, refined petroleum, chromic acid, and hydrocyanic acid, to prevent decomposition, and the author's conclusions concurred with those arrived at by Dr. Dougal, of Glasgow, and communicated to the last meeting of the British Association.

Dr. Braidwood next read his paper "On the Adulterations of Coffee, Pepper, &c., detectable by the Microscope."

* Received from Mr. Braidwood, Dec. 27, 1871.

An account was next given by Dr. De Zouche of a case of cerebral tumour (Glioma), of one of epithelial cancer of the rectum, and one of cancer of the stomach, sections of which were exhibited under several of the microscopes.

The members of the Society lastly proceeded to examine the specimens arranged under about twenty microscopes for their inspection.

The *second* meeting of this Section was held on November 17, 1871.

Mr. Hamilton in the chair, and eighteen members present.

At this meeting no paper was read, but descriptions were given of the specimens exhibited, and of the cases from which they were removed.

Dr. Carter narrated the case of a patient from whose body he had removed an extremely fatty liver, the cause of death being melæna.

Dr. Glynn exhibited a specimen of cirrhotic liver.

Mr. T. S. Walker showed sections of a melanotic tumour of the eyeball, in which the pigmentary deposit and the disintegration of the retinal tissue were well seen.

Mr. Banks exhibited sections of an adenoid tumour and of a tumour of nerve.

Dr. Braidwood showed portions of a very fine white powder which had become deposited on the inner surface of the glass of a locket enclosing a lock of hair from a child who had died a year previously from malignant scarlet fever. In the locket were placed, after the child's death, a lock of hair not disinfected (this was below a well-fitting glass cover) and a lock which had been disinfected with Condy's fluid (this was placed outside the glass cover and inside the lid of the locket). On the locket being opened a year later a fine white powder was seen on the inner surface of the glass, next to the *non*-disinfected lock of hair. When examined with a $\frac{1}{12}$-inch lens, this deposit was found to consist of very minute spherical granules (like the microzymes in vaccine lymph), and of sharply-defined, acicular, crystalline-like bodies.

Tunbridge Wells Microscopical Society.*

The October meeting of this Society was held at Rev. B. Whitelock's, when the subject of *Animalculæ* was considered. The meeting for the following month was held at the house of Dr. Johnson, the subject for discussion being *Sponges*.

* Report furnished by the Rev. B. Whitelock.

INDEX TO VOLUME VII.

A.

Actinophryan Life, on a New Phase of. By J. G. Tatem, 169.
Agassiz, Professor, on the Affinities of Crinoids, 78.
—— on the Young of Chironectes pictus, 129.
Alphonse de Brebisson, the Death of, 270.
Alternation of Generations in Fungi taking place in different Plants, 181.
Ambergris, a Substance like, 134.
Amblystoma lurida, the Development of. By Dr. P. R. Roy, 77.
Amphipleura pellucida, Note on the Resolution of, by certain Objectives. By Dr. J. J. Woodward, U. S. Army, 165, 233.
Angular Aperture, the importance of, 131.
Animal Membranes, Absorption of Insoluble Matter by, 179.
Anthony, John, M.D., the Markings on the Battledore Scales of some of the Lepidoptera, 1, 250.
Archives de Zoologie expérimentale et générale, the, 83.
Atmosphere, American Researches on the Microscopy of the. By Mr. C. Bailey, 19.

B.

Bailey, Mr. C., American Researches on the Microscopy of the Atmosphere, 19.
Barnard, F. A. P., a Few Additional Remarks on "The Examination of Nobert's Nineteenth Band," 119.
Bary, Professor De, on the Structure of the Bloom or Wax of Plants, 262.
Bastian, Dr., F.R.S., on the Modes of Origin of Infusoria, 265.
Battledore Scales of some of the Lepidoptera, the Markings on. By John Anthony, M.D. Cantab., &c., 1, 250.
Beale, Dr. Lionel S., F.R.S., the Nerves of Capillary Vessels, and their Probable Action in Health and Disease, 4.
—— on the Tissues of Man and Apes, 20.
—— on the Relation of Nerves to Pigment and other Cells or Elementary Parts, 45.
Beale, Dr. Lionel S., F.R.S., Nerve Researches: a Reply to Dr. Klein, 253.
Bichromatic Vision, J. W. Stephenson, F.R.A.S., on, 215.
Bicknell, Edwin, on Microscopic Object-glasses and their Power, 68.
——, Remarks on a Tolles' Immersion $\frac{1}{xx}$th, 70.
Bicknell, Mr., and Mr. Wenham, on the Dispute between, in the 'American Naturalist,' 178.
Bilharzia hæmatobia (Cobbold) in a Manchester Patient. By Dr. Henry Simpson, 123.
Binocular Vision, the Subject of, 232.
Bloom or Wax of Plants, the Structure of the. By Professor De Bary, 262.
Bog Mosses, on. By Robert Braithwaite, M.D., 55, 256.
Brachiopoda, Relation of, to Polyzoa, 82.
Braham, Philip, on the Crystallization of Metals by Electricity, 258.
Braithwaite, Robert, M.D., on Bog Mosses—a Monograph of the European species. Part III., *Sphagnum cymbifolium* Ehrh., 55. Part IV., *Sphagnum tenellum* Ehrh., 256.
Brakey, the Rev. S. L., Optical Curiosities of Literature, 222.
Bridgman, W. K., Maltwood's Finder Supplemented, 71.
Bronchi, the Muscular Fibres of the Small, 178.
Butterfly Scales. By the Chevalier H. de Cerrecq, 24.

C.

Capillary Vessels, the Nerves of, and their Probable Action in Health and Disease. By Dr. Lionel S. Beale, F.R.S., 4.
—— Circulation, an Improved Mode of Observing. By Mr. H. L. Smith, 268.
Carter, Mr., F.R.S., on the Examination of Sponges under the Microscope, 180.
Chalk, Foraminifera of the English, 182.
Characeæ, Where should the, be placed? By Mr. F. Currey, F.R.S., 266.

Chironectes pictus, the Young of. By Professor Agassiz, 129.
Chrysopa, the Embryology of, and its Bearings on Classification. By Dr. A. S. Packard, jun., 81.
Cilia, Experiments with Vibrating. By Professor J. Wyman, M.D., 80.
Claparède, M. Edouard, Death of, 22.
Clark, A. B., on the American Spongilla, a Craspedote Infusorian, 104.
Clione, the Mode in which it Burrows, 123.
——, Does it Burrow? By Edward Parfitt, 186.
Coleus Leaves, the Supposed Fungus on. By H. J. Slack, F.G.S., 217.
Collins's Light Corrector, 183.
Cooke, M. C., on Nucleated Sporidia, 22.
Correspondence:—
Birt, Mr. Thomas, 275.
Cerbecq, The Chevalier H. de, 24.
Cundell, Mr. G. S., F.R.M.S., 137, 270.
Hennah, Mr. J. H., 274.
Moss, Dr. Boyd, 31.
Parfitt, Mr. Edward, 186.
Pigott, Dr. Royston-, 136, 184.
Stodder, Mr. Charles, 26.
Tolles, Mr. Robert B., 135.
Wenham, F. H., C.E., 234, 272.
Woodward, Dr. J. J., U. S. Army, 27.
Crinoids, the Affinities of. By Prof. Agassiz, 78.
Currey, Mr. F., Where should the Characeæ be placed? 266.
Cylinder for Opaque Illumination, Col. Horsley's, 23.

D.

Dalton, Dr. J. C., on Spontaneous Generation, 232.
Darwin, Mr., in the Academy of Sciences, 133.
Davis, Mr. G. E., How to Destroy Disease Germs in Clothing, 21.
Definition, the Advancing Powers of Microscopic. By Dr. Royston-Pigott, M.A., 59, 101.
Diagrams, Microscopical, 182.
Diatom Hoax, the, 179.
Diatomaceæ, A Conspectus of the, 177.
——, the Distribution of, affected by Changes of Current in Lakes, 182.
Disease Germs in Clothing, How to Destroy. By Mr. G. E. Davis, 21.
—— ——, Vitality of, 21.
Dust of the Air, Various Investigations on. By Mr. J. Sidebotham, 18.

E.

Eimer, Th., Structure of the Mole's Snout, 18.
Erecting Arrangement, a New, for Use with Binocular Microscopes. By R. H. Ward, M.A., 11.
—— ——, Dr. Ward's New. By C. S., 268.
—— ——, a Note on, in Answer. By Dr. Ward, 269.
Erecting Binocular Microscope, Stephenson's. By J. W. Stephenson, Treasurer R.M.S., 167.

F.

Field Club Prizes of the Early Closing Association, 83, 134.
Fishes, the Blind of the Mammoth Caves of America, 132.
Flax, on a Means of Distinguishing the Fibres of New Zealand from those of Manilla or Sizal, by the Microscope. By Captain Hutton, 259.
Flea, the Penis of the. By Mr. W. H. Furlonge, 263.
Foraminifera of the Chalk of Gravesend and Meudon. By W. K. Parker, F.R.S., and Professor Rupert Jones, 79.
—— of the English Chalk, 182.
Fungi, Alternation of Generations in, taking place in different Plants, 181.
Fungus Growths in Mollusk Shells, 177.
Furlonge, Mr. W. H., on the Penis of the Flea, 263.

G.

Goniometer Eye-piece for the Microscope, on a New Micrometric. By J. P. Southworth, 13.
Gregarinidæ, the, Developed from Amœbæ. By M. E. Van Beneden, 125.
Guano, Is it Bird-dung or Infusorial Deposits, 127.

H.

'Hasler,' Excursion of the, 133.
Histology, a Text-book of Pathological, 133.
Hodgson, Mr. Richard, F.R.A.S., Death of, 270.
Hogg, Jabez, Hon. Sec. R.M.S., on Mycetoma, the Fungus-foot Disease of India, 98.
Hornbill (*Toccus melanoleucus*), a Paper on the Œsophagus of the, 134.

Horsley's, Col., Cylinder for Opaque Illumination, 23.
Hutton, Captain, on a Means of Distinguishing the Fibres of New Zealand Flax from those of Manilla or Sizal, by the Microscope, 259.
Huxley, Professor, Health of, 183.
Hydra carnea in Lake Superior, U.S., 181.
Hydrofluoric Acid on Glass, the Action of, viewed Microscopically. By H. F. Smith, Esq., 14.

I.

Illuminator, on an Improved Reflex, for the Highest Powers of the Microscope. By F. H. Wenham, 237.
Infusoria, the Modes of Origin of. By Dr. Bastian, F.R.S., 265.
Insect Scales, Report on Slides of, sent to the Royal Microscopical Society by the Chevalier de Cerbecq. By H. J. Slack, 48.

K.

Klein, Dr. E., Some Remarks on the Finer Nerves of the Cornea, 156.
——, Researches on the First Stages of the Development of the Common Trout (*Salmo fario*), 193.

L.

Lamps for the Microscope, on Two New, 23.
'Lens,' the: a Quarterly Journal of Microscopy and the Allied Natural Sciences, with the Transactions of the State Microscopical Society of Illinois. Edited by S. A. Briggs. Chicago, U.S.A., 1872, 261.
Lepidodendroid Stems, Description of certain. By Professor Williamson, 264.
Light Corrector, Collins's, 183.
Light, on a Silvered Prism for the Successive Polarization of. By J. W. Stephenson, F.R.A.S., 246.
Limulus, the Embryological Development of. By Prof. Van Beneden, 266.
Literature, Optical Curiosities of. By the Rev. S. L. Brakey, M.A., 222.
Lycopodiaceæ, the Arborescent, of the Coal-measures, on the Structure of the Stems of. By W. Carruthers, F.R.S., 50.
Lymph Follicles in Vagina, 132.

M.

Mac-Gillivray, Mr. P. H., Description of New Genera and Species of Australian Polyzoa, 20.
Maltwood's Finder Supplemented. By W. K. Bridgman, 71.
Meehan, Mr. Thomas, Monocotyledons the Type of Seeds, 78.
Metals, the Crystallization of, by Electricity. By Philip Braham, 258.
Microcythémie, de la, par MM. Vanlair et Masius, Professeurs à l'Université de Liège. Bruxelles, 1871, 261.
Micrographic Dictionary, the 3rd Edition. By J. W. Griffith, M.D., the Rev. M. J. Berkeley, M.A., and Rupert Jones, F.G.S. Parts I. and II., 76.
Micro-photography. By Mr. Chas. Stodder, 262.
Microscope, the, in the Diagnosis of Worms, 128.
——, Stephenson's New Erecting, 167.
Microscopic Objects, on the Classification and Arrangement of. By Dr. James Murie, F.L.S., 201.
Microscopist, the Amateur, 133.
Micro-telescope, on a New. By Prof. R. H. Ward, 73.
Microzymas, the Development of, 232.
Mole's Snout, the Structure of. By Herr Th. Eimer, 18.
Mollusk Shells, Fungus Growths in, 177.
Mouse, the Nerves of the External Ear of. By Prof. Schöbl, 79.
Murie, Dr. James, F.L.S., on the Development of Vegetable Organisms within the Thorax of Living Birds, 149.
—— on the Classification and Arrangement of Microscopic Objects, 201.
Muscular Fibres of the Small Bronchi, 178.
Mycetoma, the Fungus-foot Disease of India. By Jabez Hogg, Hon. Sec. R.M.S., 98.

N.

Nemosoma elongata, 179.
Nerve Researches, Dr. Beale's, 253.
Nerves, on the Relation of, to Pigment and other Cells or Elementary Parts. By Dr. Lionel Beale, F.R.S., 45.
—— of the Cornea, some Remarks on the Finer. By Dr. E. Klein, 156.
Nobert's Nineteenth Band, Note on "Dr. Barnard's Remarks on the Examination of." By J. J. Woodward, U. S. Army, 10.

Chord, the Centripetal Fibres
).
, the Examination of, under
Microscope. By Mr. CARTER,
., 180.
a, the American, a Craspedote,
llate Infusorian. By H. J.
:, A.B., 104.
eous Generation. By J. C.
)N, M.D., 232.
, Nucleated. By M. C. COOKE,

eris Oldhamia, 132.
ISON, J. W., on his New Erect-
nocular Microscope, 167.
Bichromatic Vision, 215.
a Silvered Prism for the Suc-
) Polarization of Light, 246.
:, Mr. CHARLES, on Micro-
graphy, 262.
Corpuscles, the Vienna, 181.

T.

J. G., on a New Phase of
)phryan Life, 169.
:he Odontoblasts of. By Mr.
WHITE, 263.
of Man and Apes. By Dr. L.
ALE, F.R.S., 20.
ift, How to make Sections of
Fragments of, 183.
Immersion $\frac{1}{12}$th, Remarks on.
)WIN BICKNELL, 70.
ROBERT B. (U.S.), on the
:tive Powers of Peculiar Ob-
s, 115.
1 Microscope. Par Ch. Robin.
16.
ie Common (*Salmo fario*), Re-
es on the First Stages of the
)pment of the. By Dr. E.
, 193.
losis, the Microscopy of. By
F. RODENSTEIN, 128.
)le, Dr. Matthews', a Modifica-
, 267.

V.

Vegetable Organisms, on the Development of, within the Thorax of Living Birds. By Dr. JAMES MURIE, F.L.S., 149.

W.

WARD, R. H., M.A., A New Erecting Arrangement, especially Designed for Use with Binocular Microscopes, 11.
—— on a New Micro-telescope, 73.
—— on Uniformity of Nomenclature in regard to Microscopical Objectives and Oculars, 262.
Wenham, Mr., and Mr. Bicknell, the 'American Naturalist' on the Dispute between, 178.
WENHAM, F. H., on an Improved Reflex Illuminator for the Highest Powers of the Microscope, 237.
WILLIAMSON, Professor, Description of certain Lepidodendroid Stems, 264.
WOODWARD, J. J., U. S. Army, Note on Dr. Barnard's Remarks on "The Examination of Nobert's Nineteenth Band," 10.
——, Note on the Resolution of Amphipleura pellucida by certain Objectives. By R. and J. BECK and by WILLIAM WALES, 165, 233.
WYMAN, Prof., Experiments with Vibrating Cilia, 80.

X.

Xenodochus carbonarius, a Rare Fungus, 130.

Z.

Zoology, Laboratory for Marine, 126.

END OF VOLUME VII.

ON: PRINTED BY WILLIAM CLOWES AND SONS, STAMFORD STREET AND CHARING CROSS.

Nobert's Nineteenth Band, A Few Additional Remarks on the Examination of. By F. A. P. Barnard, 119.
Nomenclature in regard to Microscopical Objectives and Oculars, on Uniformity of. By R. H. Ward, M.D., 262.

O.

Object-glasses and their Power. By Edwin Bicknell, 68.
Objectives, the Refractive Powers of Peculiar. By Robt. B. Tolles, U. S., 115.
—— and Oculars, on Uniformity of Nomenclature in regard to Microscopical, 262.
Ostracoda, the British, 132.

P.

Packard, Dr. A. S., jun., on the Embryology of Chrysopa and its Bearings on Classification, 81.
Parker, W. K, F.R.S., the Annual Address delivered before the Royal Microscopical Society, Feb. 7th, 1872, 89.
Passifloraceæ, the Natural History of the. By Dr. Maxwell T. Masters, F.R.S., 127.
Pattern Lead for Making Cells, 233.
Patterson, Mr., F.R.S., the Death of, 133.
Pennatulians, the Anatomy of the, 130.
Pigott, Dr. Royston-, on the Advancing Powers of Microscopic Definition, 59, 101.
—— on the various Phenomena exhibited by the Podura Test under Ordinary and Extraordinary Microscopic Resolving Powers, 170.
Pitchstones of Arran, the Microscopic Structure of, 125.
Pocket-case for Specimens, a Medical Man's, 267.
Podisoma fuscum and P. juniperi, Notes on. H. J. Slack, F.G.S., 217.
Podura Test, on the various Phenomena exhibited by the, under Ordinary and Extraordinary Microscopic Resolving Powers. By Dr. Royston-Pigott, 170.
Polarization of Light, on a Silvered Prism for the Successive. By J. W. Stephenson, F.R.A.S., 246.
Polyzoa, Description of New Genera and Species of Australian. By Mr. P. H. Mac-Gillivray, 20.
Proceedings of Societies:—
Brighton and Sussex Natural History Society, 38, 145, 191, 281.
Croydon Microscopical Club Soirée, 284.
Liverpool Medical Institution, 287.
Reading Microscopical Society, 147, 283.
Royal Microscopical Society, 31, 84, 139, 143, 189, 234, 276.
South London Microscopical and Natural History Society, 40, 148.
Tunbridge Wells Microscopical Society, 288.
Protozoa, Transmutation of Form among, 182.
Pus-globules, Peroxide of Hydrogen in. By Dr. J. Day, 79.

Q.

Quinine, Its Action on the White Corpuscles of the Blood, 131.

R.

Richards, Mr. E., on Two New Lamps for the Microscope, 23.
Robin, Ch., Traité du Microscope, 1871, 16.
Rodenstein, Dr. C. F., on the Microscopy of Tuberculosis, 128.
Roy, Dr. P. R., on the Development of Amblystoma lurida, 77.

S.

Schöbl, Prof., on the Nerves of the External Ear of the Mouse, 79.
Sections of Small Fragments of Soft Tissues, How to Make, 183.
Seeds, Monocotyledons the Type of. By Mr. Thomas Meehan, 78.
Sidebotham, Mr. J., Various Investigations on the Dust of the Air, 18.
Simpson, Dr. Henry, on Bilharzia hæmatobia (Cobbold) in a Manchester Patient, 123.
Slack, H. J., Sec. R.M.S., Report on Slides of Insect Scales, sent to the Royal Microscopical Society by the Chevalier de Cerbecq, 48.
——, F.G.S., the Supposed Fungus on Coleus Leaves; and also Notes on Podisoma fuscum and P. juniperi, 217.
Southworth, J. P., on a New Micrometric Goniometer Eye-piece for the Microscope, 13.
Sphagnum cymbifolium, 55.
—— tenellum, 256.

Spinal Chord, the Centripetal Fibres of, 180.
Sponges, the Examination of, under the Microscope. By Mr. CARTER, F.R.S., 180.
Spongilla, the American, a Craspedote, Flagellate Infusorian. By H. J. CLARK, A.B., 104.
Spontaneous Generation. By J. C. DALTON, M.D., 232.
Sporidia, Nucleated. By M. C. COOKE, 22.
Stauropteris Oldhamia, 132.
STEPHENSON, J. W., on his New Erecting Binocular Microscope, 167.
—— on Bichromatic Vision, 215.
—— on a Silvered Prism for the Successive Polarization of Light, 246.
STODDER, Mr. CHARLES, on Microphotography, 262.
Syphilis Corpuscles, the Vienna, 181.

T.

TATEM, J. G., on a New Phase of Actinophryan Life, 169.
Teeth, the Odontoblasts of. By Mr. T. C. WHITE, 263.
Tissues of Man and Apes. By Dr. L. S. BEALE, F.R.S., 20.
——, Soft, How to make Sections of small Fragments of, 183.
Tolles' Immersion $\frac{1}{x}$th, Remarks on. By EDWIN BICKNELL, 70.
TOLLES, ROBERT B. (U.S.), on the Refractive Powers of Peculiar Objectives, 115.
Traité du Microscope. Par Ch. Robin. 1871, 16.
Trout, the Common (*Salmo fario*), Researches on the First Stages of the Development of the. By Dr. E. KLEIN, 193.
Tuberculosis, the Microscopy of. By Dr. C. F. RODENSTEIN, 128.
Turn-table, Dr. Matthews', a Modification of, 267.

V.

Vegetable Organisms, on the Development of, within the Thorax of Living Birds. By Dr. JAMES MURIE, F.L.S., 149.

W.

WARD, R. H., M.A., A New Erecting Arrangement, especially Designed for Use with Binocular Microscopes, 11.
—— on a New Micro-telescope, 73.
—— on Uniformity of Nomenclature in regard to Microscopical Objectives and Oculars, 262.
Wenham, Mr., and Mr. Bicknell, the 'American Naturalist' on the Dispute between, 178.
WENHAM, F. H., on an Improved Reflex Illuminator for the Highest Powers of the Microscope, 237.
WILLIAMSON, Professor, Description of certain Lepidodendroid Stems, 264.
WOODWARD, J. J., U. S. Army, Note on Dr. Barnard's Remarks on "The Examination of Nobert's Nineteenth Band," 10.
——, Note on the Resolution of Amphipleura pellucida by certain Objectives. By R. and J. BECK and by WILLIAM WALES, 165, 233.
WYMAN, Prof., Experiments with Vibrating Cilia, 80.

X.

Xenodochus carbonarius, a Rare Fungus, 130.

Z.

Zoology, Laboratory for Marine, 126.

END OF VOLUME VII.

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THE MONTHLY

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TRANSACTIONS

OF THE

ROYAL MICROSCOPICAL SOCIETY,

AND

RECORD OF HISTOLOGICAL RESEARCH

AT HOME AND ABROAD.

EDITED BY

HENRY LAWSON, M.D., F.R.M.S..

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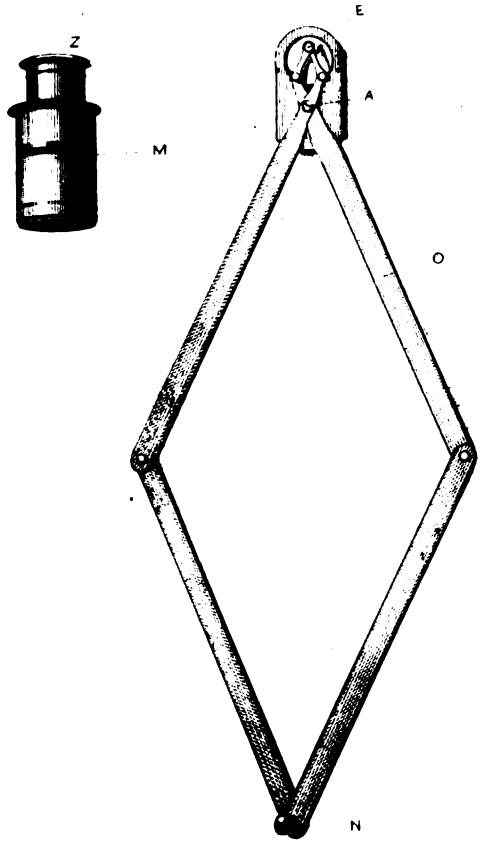


Mr. Palmer's Lantern

THE

MONTHLY MICROSCOPICAL JOURNAL.

JULY 1, 1872.

I.—*On a Micro-pantograph.* By Isaac Roberts, F.G.S.

(*Taken as read before the* Royal Microscopical Society, *June* 5, 1872.)

Plate XXI.

Dr. Beale, F.R.S., in his treatise on 'How to Work with the Microscope,' says:—"It may be truly said that no real advance in our knowledge of the minute structure of animal or vegetable tissues can be communicated to others, unless accurate drawings are made." . . . "We may feel sure that those who follow us will respect our drawings, if honest copies of nature, although very little of what is now written will be read some years hence." These statements will be accepted as truisms, and yet how few are the observers with the microscope who can draw at all, not to say draw truthfully.

This defect is not to be wondered at, for the difficulties are

EXPLANATION OF PLATE XXI.

Fig. 1.—The letters E, A, O, and N, indicate parts of the micro-pantograph. E is the glass disk, with micrometer cross-lines ruled upon it. It is cemented over a small hole drilled through the centre of the rivet which forms the joint at the minor extremity. A is the centre or fulcrum round which the instrument freely moves. O is one of the major limbs. N is the drawing-pencil holder. It is placed over a hole which is drilled through the rivet which forms the joint at the major end. Z is the eye-piece of the microscope. M is a slit cut into the side of the eye-piece, into which slit the minor end of the micro-pantograph and the support shown under E, A, Fig. 1, are inserted and firmly fixed. To draw any object it is only necessary to place the end of the pantograph in the slit of the eye-piece; place the eye-piece in the microscope; adjust the height and inclination of the drawing table and paper or pad to the plane of the pantograph, and with the right-hand forefinger and thumb guide the drawing-pencil with a slight pressure over the paper, at the same time looking through the eye-piece at the object and guiding the centre of the micrometer cross-lines over the respective parts of it, and an accurate drawing of the object will be traced upon the paper. I obtained a very satisfactory result even with the first trial, the object drawn being the leg of a bee.

Fig. 2 is a general view of the drawing arrangements when they are in use. The proportions of an instrument made to draw the apparent size of magnified objects are as follows:—Length of the minor sides of the parallelogram within the eye-piece, half an inch; length of the major sides, 5⅛ inches. The micro-pantograph when extended the full length measures 12¼ inches.

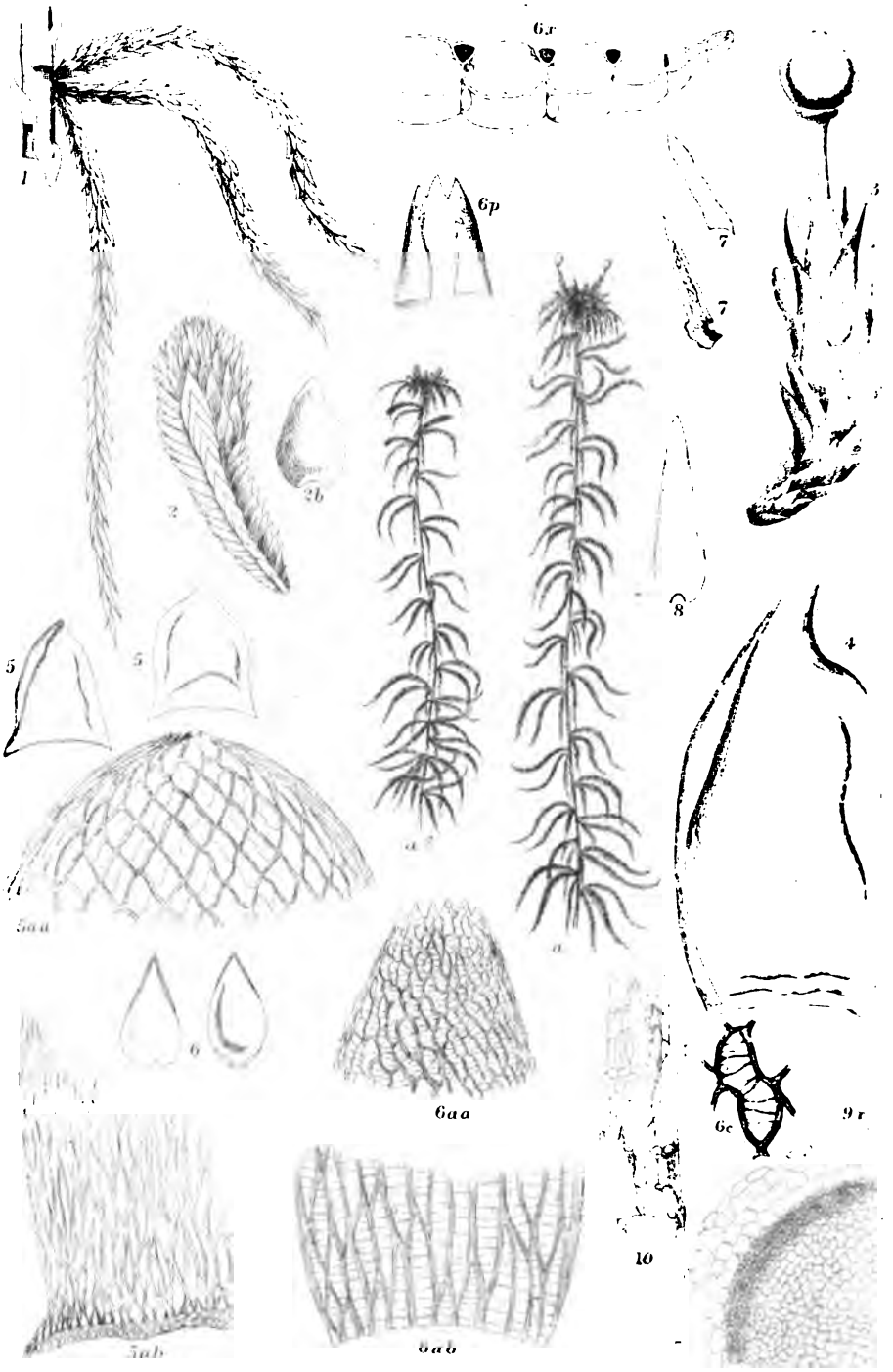
great. I cannot decide whether it is more difficult to draw with the aid of the appliances hitherto in use, or by the eye without them.

I now introduce to your notice a very simple instrument which I have made, and which I think will enable any person, after a few hours' practice with it, to draw with great ease and accuracy. I call the instrument a *Micro-pantograph*, and the plate which accompanies this paper, and following description, will enable you to understand its construction and mode of application to the microscope for the purpose of drawing.

I do not patent the instrument, but freely present it to the workers engaged in the cultivation and advancement of microscopical science. Any microscopist who possesses a little skill in mechanical operations may easily make a micro-pantograph for himself; or any optician can make it and adapt it to any microscope at a trifling cost.

Following is a description of the instrument:—It consists essentially of two parallelograms having their major and minor sides and angles respectively proportioned in all positions in which the instrument can be placed. The major and minor sides rotate freely about the centre, or fulcrum, which they have in common, and which is fixed to the eye-piece of the microscope, in the focus of the eye-lens. A pencil is attached to the major end joint of the instrument, and a small glass disk, ruled with a micrometer lined cross, is attached to the minor or eye-piece end joint, in the position where pointers are placed; and similar focussing of the eye-lens is requisite to see both the cross and the object at the same time that is necessary to see a pointer and object. When drawing, the hand guides the pencil which marks the drawing upon the paper, and at the same time guides the micrometer cross-lines over the field where the object appears in the microscope.

The drawing-paper, or pad, should be attached to a small table (shown in Fig. 2), which is capable of being adjusted to the required height for the microscope used, and the top must be made to incline to any required angle, according to the position of the microscope.



Br... Mus...

II.—*On Bog Mosses.* By R. Braithwaite, M.D., F.L.S.

3. Sph. rubellum Wilson.

Bryol. Brit. p. 19 (1855).

Plate XXII.

Syn.—*Sph. rubellum* Wils. l. c. T. LX. Schimper, Torfm. p. 70, T. 20 (1858); Synop. p. 680 (1860). Berk. Handbk. p. 306 (1863). Lindb. Torfmos. No. 12 (1862). Hartm. Skan. Fl. 9th ed. II, p. 83 (1864). Milde, Bryol. Siles. p. 383 (1869).—*Sph. acutifolium*, Var. ζ *rubellum* Russow, Torfm. p. 41 (1865).

Dioicous. Plants slender, laxly tufted, resembling the small flaccid red form of *S. acutifolium* in habit, and *S. tenellum* in tenuity. Stem fragile, 2–5 in. high, sublignose, purple, coated with a thickish bark composed of *four strata of cells, destitute of pores.* Ramuli in distant fascicles, shortish, often curved or twisted, three, 2 arcuate, 1 deflexed; their flask-shaped cells with the apex prominent, and a little recurved. *Cauline leaves large,* erect or reflexed, *broadly ovate-oblong, concave,* finally plicate, furnished at extreme apex with several small teeth, *narrowly margined,* hyaline cells large 2–3 partite, non-fibrose, non-porose. Ramuline leaves at base of branch very small, ovate, at middle *oblong-ovate, with the margins inflexed in the upper part, elliptic when expanded under pressure, gradually acuminate, the point with three stoutish teeth,* and several minute ones below them; leaves of deflexed branches elongated, all narrowly margined, reddish, here and there yellow or green; *hyaline cells with annular fibres,* sparingly porose, gradually smaller toward the edge, strongly projecting and confluent at the back, on the inner side removed from each other by the interposed chlorophyll cells.

Male plants like the female, and more numerous; amentula short, clavate, purple with broadly ovate bracts.

Fruit immersed in the capitulum or on a shortish peduncle; peduncular leaves numerous, loosely imbricated, subrecurvo-patent

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EXPLANATION OF PLATE XXII.

Sphagnum rubellum.

a.—Female plant. *a* ♂.—Male plant.
1.—Part of stem with a branch fascicle.
2.—Catkin of male flowers. 2 *b.*—Bract from same.
3.—Fruit with its peduncle. 4.—Peduncular leaf.
5.—Stem leaves. 5 *a a.*—Areolation of apex of same. 5 *a b.*—Ditto of base.
6.—Leaves from middle of a divergent branch. 6 *x.*—Transverse section. 6 *p.*—Point of same. 6 *a a.*—Areolation of apex. 6 *a b.*—Areolation of base. 6 *c.*—Single cell from middle × 200.
7.—Intermediate leaves from base of a divergent branch.
8.—Leaf from a pendent branch.
9 *x.*—Part of section of stem.
10.—Branch denuded of leaves.

toward apex, elongate-oblong, slightly acuminate, convolute, straw-coloured. Capsule globose dark brown. Spores ochreous.

Hab. In bogs among grass and carices, often with *Sph. acutifolium* and *tenellum*. Fruit rare. June and July. Scandinavia, the Tyrol, England, Scotland, and Ireland. The specimen figured is from Risley Moss, near Warrington, sent by the late Mr. Wilson. It is probable that this species is often passed over as *Sph. acutifolium*, var. *purpureum*, to which it must be confessed it bears a very suspicious resemblance; and if we overlook the dioicous position of the inflorescence, there is no other character that will absolutely separate them. Russow indeed boldly unites them, and states, moreover, that *S. acutifolium* is sometimes dioicous: this I am unable to confirm; and as we find the reproductive organs in all mosses very constant in position, we can only refer it to *acutifolium* from a Darwinian point of view by supposing that the male and female organs after a long struggle have succeeded in maintaining separate establishments. The other points of distinction from *acutifolium* are, "the larger non-fibrose stem leaves and the broader branch leaves;" but I have shown that the presence or absence of threads in the hyaline cells may largely depend on conditions of growth, and is liable to variation in all species; the greater width of the leaves must be admitted, but this cannot be regarded as more than a relative character. Its habit appears to be constantly more slender than *S. acutifolium*, and the leaves of the divergent branches have the cells in their upper half differing less in size from the lower than they do in *S. acutifolium*, the threads in *rubellum* being also entirely annular; the pores are smaller than in *acutifolium*, and more equal in size at the base and apex of the leaf, and lastly, the catkins of male flowers are cuspidate in *S. acutifolium*, clavate and obtuse in *S. rubellum*.

III.—*Remarks on the Homological Position of the Members constituting the* THECATED SECTION *of the* CLASS ROTATORIA. By Charles Cubitt, F.R.M.S.

(*Taken as read before the* Royal Microscopical Society, *June* 5, 1872.)

Plates XXIII. and XXIV.

By Thecated I mean all those animals that are invested in a Theca or sheath, which, whether organically attached to the animals or not, forms no part of the general integument of the body which will be denominated the *corinum*. These Forms, moreover, are all supported on a *pedicle*, or footstalk, by means of which they attach themselves permanently to some support or basis, either individually or in clusters.

In a somewhat hurried epitome communicated to the 'Monthly Microscopical Journal' in October last, I confined myself to a cursory review of the homologies of two species, the first, a Form that I had inadvertently placed in the Genus Floscularia as Floscularia *coronetta;* and the second, Limnias *annulatus*, a rare species, which I in that communication called Melicerta *annulatus;* and finding that certain objections have been raised to the temerity of associating Melicerta, Limnias, Tubicolaria, and Œcistes in one Genus; Lacinularia, Megalotrocha, and Conochilus in another Genus; and placing these two Genera in the same Family while constituting a Family for the reception of Stephanoceros and Floscularia; I feel it incumbent on me to extend these remarks somewhat further in support of the views I had so briefly set forth; for having since that date been closely occupied in the collection of materials for a Memoir of the THECATED SECTION of the CLASS ROTATORIA, which I trust at no distant period to submit to the review of my brother microscopists, I have had ample opportunities for confirming the views I had so briefly set forth; and in this short review it will be necessary to confine my remarks to a consideration of their Dermal and Alimentary Systems.

I find myself following the steps of Mr. Gosse, who in the year 1851 published a consistent classification for the CLASS ROTATORIA in the 'Annals and Magazine of Natural History,' in which he subdivided these particular Forms which I define by the term THECATED, precisely as I find them work out on a careful consideration of their anatomical details. Mr. Henry Davis was fully aware of this when he described and so carefully delineated two new species, although he felt dissatisfied with the position in which he provisionally placed them; still, accepting the Ehrenbergian notion, the one which he denominated Œcistes *intermedius* should have been Limnias *intermedius*.

This SECTION divides itself into two Families—Flosculariæ,

containing the two Genera Stephanoceros and Floscularia; and MELICERTADÆ, containing the two Genera Melicerta and Lacinularia: and to see at a glance where the first FAMILY differs from the second, and where the Genera of each agree in their general organization, the diagrams (Plate XXIII., Figs. 1, 2, 3) are submitted, on each of which a line is drawn from the mouth to the anus; and if we consider this to be the axis of the body, we find in the first FAMILY that both the marginal wreath of setæ, the *trochus* (which I employ also for the ciliated wreath of the second), and the secondary belt of cilia, the *cingulum*, surround the axis, and the anus is placed on the *neural* side, the side on which the ganglion is seated; but in the second FAMILY the trochus subtends the axis, and the cingulum, after traversing the margin of the *corona* (the miscalled *disk*), also surrounds the axis, while the anus is situated on the opposite, the *hæmal* side. The trochus is indicated on the figures by a full black line, and the cingulum by double fine lines.

There is a particular reason for rejecting such inconsistent terms as *dorsal* and *ventral* in this SECTION, for in the FAMILY FLOSCULARIÆ they virtually represent the *same* side of the animal. I shall therefore, with the examples of both Huxley and Allman before me, employ *hæmal* and *neural*, on the same grounds that Allman adopts them in his 'Memoir of the Fresh-water Polyzoa,' *viz.* that "notwithstanding an apparent contradiction in denominating as 'hæmal' any portion of an animal totally deprived of a blood-vascular system, they have the advantage of stating a simple fact."

The organization of these two FAMILIES differs conspicuously in their *physique*, and equally so in their DERMAL and ALIMENTARY SYSTEMS; and in selecting an individual from each, for the purpose of eliminating their true relations, I shall employ Floscularia *campanulata* to illustrate the one, and Melicerta *pilula* the other, from the fact that they have recently been recorded as 'New Species of Rotatoria' by Dr. F. Collins;* this last species being a Form with which I have for some years been intimately acquainted, which I had denominated Melicerta *pilula* (long before I committed my observations to print), from the fact that she fortifies the gelatinous basis of the theca with her own *excremental pilules*. This, as a new species, was recorded in a paper communicated to the Quekett Club in the year 1868 by Mr. J. G. Tatem, who at that date admitted that he had no specific title wherewith to denominate it. And Dr. Collins, in describing this, states that "the pellet with which the animal builds its tube is formed in a *kind of sac* situated at the lower extremity of the abdomen, and is discharged through the cloaca." He describes also a new species of Floscularia, which he denominates Floscularia *trilobata*, somewhat similar in appear-

* 'Science Gossip,' January, 1872.

FLOSCULARIÆ

MELICERTADÆ

Fig. 1

Pedunculus

Fig. 2

Melicerta

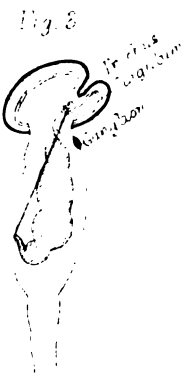
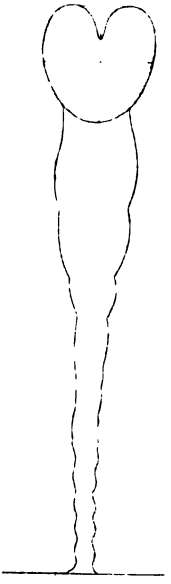


Fig. 3

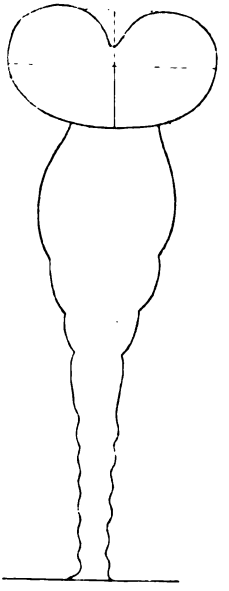
Lagenella

Fig. 4



Lagenella setosa

Fig. 5.



Lagenella alba-flavicans

West & Co. imp.

Diagrams.

ance to F. *campanulata*. "The dorsal lobe is frequently much longer than the other *two*, and when such is the case it is *commonly* curved forwards over the funnel-shaped mouth, and presents a somewhat hooked appearance: the setæ are also *differently arranged*," *i.e.* from those of F. *campanulata*, "being placed between the lobes as well as on their summits." "The cloaca is situated high in the body;" he shows it at the neck. Let Dr. Collins imagine himself in the same predicament, and he will have a case equally consistent with the laws of animal physiology. The so-called intestine is shown to extend all along the hæmal side of the body, on the side *opposite* to the ganglion; but in every individual of this Family the anus is situated on the *same* side as the ganglion.

Dermal System.

The theca in the first Family (Plate XXIV., Fig. 1) is secreted from a secerning gland situated at the foot, and the integument, after expanding at the base upon its selected habitat, ascends to the neck of the animal, and from thence it returns downwards along the body, forming an *annular* space between the inner and outer integuments, which space is filled with a hyaline fluid similarly with the *ectocyst* of Lophopus *crystallinus* of the Polyzoa. In Floscularia, however, this fluid becomes somewhat turbid in the older animals.

The inner lining forms a *tubular* space commencing at a point somewhat below the anus, thus leaving a free passage for the discharge of the ova and fæces along the body of the animal. The attachment of the theca with the pedicle differs in extent in different individuals. I have had ample opportunities of proving beyond dispute that this organic attachment of the theca is real, having liberated numerous patients from their selected supports without in any degree injuring the theca; and when in this free condition, and left with perfect freedom of motion, these animals made considerable invaginations of the theca with every regression of the body, which under such abnormal conditions were frequently repeated, both at their anterior and posterior regions.

The animals of the Genus Floscularia manifest a predilection for liberating themselves from their investing thecæ, and re-establishing themselves in new ones, a habit which is not practised by Stephanoceros. But there is a peculiarity with the theca of Stephanoceros that does not obtain to such a definite extent with any of the Floscularia; I refer to the corrugations which occur along the whole length of the theca, and upon the origin of these corrugations much misapprehension has been manifested. They have actually been attributed to the "tube sinking from its own weight"; the absurdity of which is at once apparent when we know that the specific

gravity of the substance of the tube is *less* than that of the water in which, when liberated, they float. These corrugations are due to natural causes, aided by mechanical effects; they are produced by the acts of the animal's regressions, which are very frequently indulged in for periods of some hours' duration, and which are no doubt periods of repose, when beyond the mechanical effect of such a contracted position there is possibly a partial arrest in the secretion of the substance of the theca.

The theca of the Family Melicertadæ, on the other hand, is never organically attached after it is once deposited; the gelatinous basis of which this foundation is formed is secreted by a secerning gland, situated in the anterior region, and in a neural aspect, in close proximity with the ganglion, and it is discharged from an orifice in the centre of the *figurum* (Plate XXIV., Figs. 2, 3, *r*). This cup-like organ is present in every individual of this Family, and is furnished with cilia in every species that attaches any foreign substance to the theca. It is, however, unciliated in Melicerta *annulatus*, where the theca is free from any extraneous accession except such as become attached parasitically. In M. *pilula* it is also unciliated, for here the animal fortifies the gelatinous basis with her own voided excremental pilules. And in Lacinularia *volvox*, that free roving cluster, it is also unciliated where the theca is always devoid of any foreign accession.

There are two species in which the theca is constructed upon a symmetrical plan, Melicerta *ringens* and M. *annulatus*, and it attains a high degree of perfection in M. *ringens;* and I take this opportunity of recording that it has been my privilege to discover the particular organs by whose functions the mathematical precision with which the pellets are deposited upon the theca is governed. They have their homologue in M. *annulatus*, the only other species that constructs its theca on a symmetrical plan. I refer to the two "hooked spines" situated on the hæmal side of the neck, which are the most conspicuous organs on the emergence of the animal, and these have been erroneously called "lips." I must, however, content myself with stating that, notwithstanding their being situated on the hæmal side, they are employed in governing the mathematical precision with which the pellets are deposited on the opposite side; and in this brief review I confine myself simply to recording the fact. A more detailed description will be given in the 'Memoir' on which I am at present employed.

In the particular instances of Melicerta *ceratophylli*, M. *crystallinus*, and M. *intermedius*, the theca is formed of a consistent shape, and with well-defined walls, leaving a clear space for the motions of the inhabitants, but without any system of parts symmetrically arranged, and with these, the *upper* portions are more densely blended with foreign accessions than the *lower;* while with M.

Fig 4

Fig 3

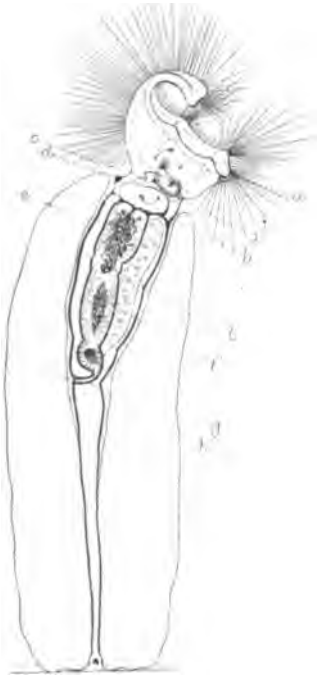


Fig 5

Fig 2

Fasciolaria Campanicola — *Melocera pilula*

REFERENCE.

a Trochus	*f Stomach*	*m Parotid Gland*
d Ganglion	*g Intestine*	*n Cordon*
b Cilia	*h Rectum*	*o Liver*
c Tongue	*i Anus*	*p Pancreas*
d Ganglion	*k Pharynx*	*q Rhizus*
e Cornea	*l Mantle*	*r Pigment*

C. Cubitt del. West & ...

Fasciolaria and Melocera.

will at once make manifest; for although they both exhibit a palpable bilaterality in the corona, they differ essentially in their respective characters, for we see in L. *socialis* that both axes of the disk are equal, while in L. *albo-flavicans* the horizontal axis is just double that of the vertical. Dujardin denies the propriety of their separation, but Ehrenberg considers the *absence* of a divesting theca, and the attachment of the ova by a filament to the body, to be sufficient grounds for separation. Now the very assumption of the filament attaching the ova to the body is the best evidence we could desire, to show that the group is invested in a gelatinous matrix. The *appearance* of the filament is simply the microscopical rendering of the integument of the theca itself surrounding the ova. Ehrenberg's drawing speaks for itself.

Alimentary System.

However dissimilar the organization of the setiferous trochus (*a*) of the one Family may be with the ciliated trochus of the other, they are constituted to perform the same functions in the animal's economy as the first instrument employed in securing and directing prey to the mouth. In Stephanoceros *Eichhornii* and S. *Horatii* the corona is invested in one continuous series of setæ all along the margin of the trochus, but in Floscularia the lobes only are furnished with these prehensile organs, with the one exception of Floscularia *campanulata*, where they occur also in one continuous and unbroken series. Plate XXIV., Fig. 1.

In the first Family the alimentary particles, when captured by the trochal setæ, are directed by their intermittent action into the *calix* (*b*), in which the ciliated *cingulum* (*a'*) is situated. I employ the term calix to the funnel that precedes the alimentary canal, not in its botanical significance of the leaf-like organs that surround and support the corolla, as Professor Allman has applied it to the diaphanous membrane surrounding the lower portions of the tentacles of the Polyzoa, but in its more general signification of a *cup*. The cingulum creates a vortex within the calix, in which the particles, alimentary or otherwise, become involved, passing by its influence upwards on the hæmal side, and downwards on the opposite, where they become subjected to the scrutiny of the tongue-like little organ the *lingula* (*c*), that exercises such a marvellous faculty in selecting from the heterogeneous mass those portions which are specially qualified for alimentary purposes, and in rejecting others as refuse; and situated on the neural side just above the ganglion (*d*), within the calix, its position and functions are identical with the *epistome* of the Polyzoa.

The calix communicates, by means of an œsophagus, with a capacious chamber, the *cardiacus* (*e*), and it is seen that alimentary

matters, whether animal or vegetable, frequently pass through the *maxillæ* (*l*), suffering but little disintegration from their action when they return again and again into this chamber, the walls of which are lined with follicles, that, notwithstanding the position this chamber occupies in relation with the jaws, I assume to be *gastric* follicles, for the *stomach* (*f*) which succeeds it in this FAMILY consists of a single chamber, whose sides are thickened by large turgid cells that secrete the *bile*. The stomach discharges its fæcal contents at once into the *intestine* (*g*), wherein they become formed by the action of its cilia into crude pellets, that are discharged by a short rectum (*h*) to the anus (*i*), and the anus in this FAMILY is situated on the neural side, the same as with the Polyzoa, and is placed at the lowest extremity of the body, and not, as Dr. Collins represents it, on the hæmal side at the *neck* of his pseudo-new species: he has simply mistaken the integument of the ovarium, which *is* placed on the side opposite to that occupied by the anus, and it is frequently found to be devoid of ovarian nuclei. The rectum and anus are always placed on the neural side in this FAMILY, and at the lowest extremity of the body.

In the second FAMILY the ALIMENTARY SYSTEM differs essentially from the other, for the provender which is first brought by the persistent action of the trochal cilia under the influence of the ciliated cingulum, whose action creates two currents that act in opposite directions, the one from the right and the other from the left, by which the heterogeneous mass of particles is conveyed to the lingula (*c*), that is here situated on the opposite, the hæmal side, where, by the influence of this organ, the particles so involved are segregated and severally disposed both for alimentary and fabricating purposes; when those selected for aliment pass at once by a ciliated *pharynx* (*k*) to the *maxillæ* (*l*), which organ in this FAMILY is provided with a *parotid gland* (*m*), situated in close proximity therewith, and the maxillæ produce a much more complete degree of mastication than that organ in the first FAMILY. In MELICERTADÆ it occupies a bulbous chamber, which is separated from the *stomach* (*f*) by a *cardia* (*n*), and the stomach virtually consists of three chambers, for wherever digestion is concerned, certain ingredients are necessary for its due performance;—gastric, biliary, and pancreatic fluids—which in the vertebrates are secreted from a separate set of organs that communicate with the alimentary canal by ducts; but with the Rotifera the alimentary canal consists of a single channel divided into three compartments, that are each organized to secrete these several fluids, so that in Melicerta the first chamber is the stomach proper (*f*), whose sides are lined with *gastric follicles*, that convert the masticated food into *chyle;* the second (*o*) being the biliary chamber acting in the capacity of a *liver;* and the third (*p*) a *pancreatic*

chamber wherein the *chyle* becomes converted into *chyme*, and from this chamber the digested food passes by means of a contractile *pylorus* (*g*) into the *intestine* (*g*), which is highly ciliated, and wherein the fæces become formed, by the rotation of the contained cilia, into well-defined pellets, that are ejected by a muscular contraction of the long *rectum* (*h*) to the anus (*i*), which acts as the common duct for both the fæces and the ova; and the monocyst at its junction with the anus is seen to be considerably folded in upon itself. In the particular instance of M. *pilula* the fæcal pilules attain a much greater bulk than in any other species, and with such pilules this little Melicertian fortifies the gelatinous basis of the theca, which, like every Form, is secreted from the figurum (*r*), that in this particular species is unciliated.

IV.—*Further Remarks on Tolles' ⅛th and Powell and Lealand's Immersion $\frac{1}{16}$th.*

By Edwin Bicknell, Cambridge, Mass.

On page 225, vol. vi., of the 'Monthly Microscopical Journal,' I made some "Remarks" on Dr. Woodward's "Note" in the number for September preceding. In the number for January, 1872, vol. vii., page 28, Dr. Woodward says that I have "wilfully" misinterpreted his *published measurements* of the wet front of the Powell and Lealand $\frac{1}{16}$th. I have *not* misinterpreted the *actual power* of the above-mentioned objective, although Dr. Woodward's published measurements may look so exact—on paper; and in order to make out my case I shall not go beyond Dr. Woodward's own statements and "published" work, except in one case, to obtain my evidence.

On page 29, vol. vii., Dr. Woodward gives a table of measurements of different objectives from Powell and Lealand, both wet and dry: I wish to state here that I have nothing to say in regard to any of the *dry fronts*, and only with regard to *one* immersion $\frac{1}{16}$th, and that one is No. 2. I assume this to be the one from the magnifying power as given in the table agreeing with Dr. W.'s *published* measurements.

Dr. Woodward quotes from Mr. Charles Stodder* that "objectives are named when adjusted for uncovered objects, a fact not generally known by purchasers. The power increases, that is, the focus is shorter as the collar is turned to work through the covering glass." This is the method which I have used during the past two years, and I consider it the most certain and reliable.

During the past winter Mr. Stodder has loaned me the glass positives of *Amphipleura pellucida*, which were made at the Army Medical Museum, with the Tolles' ⅛th described by Dr. W. in his "Note," and also the positive made with the Powell and Lealand $\frac{1}{16}$th at the same time. These positives are marked precisely like those described by Dr. W. in his "Note," and are probably duplicates of those sent to the Editor of this Journal by Dr. Woodward. I have carefully and repeatedly counted the lines in the positive made by the Tolles' ⅛th (without eye-piece at 48 inches distance from the object to the screen). The length counted is exactly ⅛th of an inch (the reason of this will be seen farther on), and in this space I find just 74 (seventy-four) lines, making 370 lines to the inch in the positive. Dr. Woodward gives the fineness of the lines on the *Am. pellucida* at 95,000 to the inch. Dividing 95,000 by 370 gives 256+ diameters as the power of the objective at 48 inches distance, which is just what Dr. W. has marked the positive. 48 divided by 256 gives ·1875, making the objective at that position of

* 'M. M. J.,' Oct. 1871, p. 208, note.

cover adjustment a little less than ⅛th of an inch in equivalent focal length, that is, of higher power.

Now, on counting precisely the *same* portion of the *same* frustule in the positive made with the Powell and Lealand objective (the portion here counted being one *inch* long, whereas in the other it was one-*fifth*, but in both cases exactly the same portion), I find just 75 lines (seventy-five lines) to the inch. 95,000 divided by 75 gives 1266+ diameters at 48 inches distance. 48 divided by 1266 gives ·0387+, making the objective at this point of the cover adjustment a little less than a $\frac{1}{25}$*th of an inch in equivalent focal length.* It will be seen that this differs materially from Dr. Woodward's "published measurements." He gives in his table on page 29, vol. vii., 1100 diameters at full correction for thickest cover at 48 inches distance (yet on the glass positive which I have counted he has marked 1140 *diam*). I make it 1266+ diam. Let us reverse the process, take the Doctor's "published measurements," 1100 diam., and multiply it by 75, the number of lines to the inch in the positive; this will give 82,500 as the fineness of the lines on the *Am. pellucida* which he has photographed (1140 diam. will give 85,500), quite a falling off from his 95,000. If the "published measurements" are right, the *Am. pellucida* is not 95,000 to the inch.

It will be seen that instead of being about three times the power of the Tolles' ⅛th, which Dr. W. has called a little higher than a ⅛th—and my count of the photograph agrees closely with his statement—the Powell and Lealand objective is *five times* (very nearly) *the power*. This increase of fifty per cent. over the nominal power cannot be altogether ascribed to the cover adjustment, as Dr. W. in his table above quoted gives the entire range at less than twenty per cent. In fact, the objective is at least a $\frac{1}{15}$th at its uncovered point, and this *was known to Dr. Woodward* before he made the comparison with the Tolles' ⅛th, with the disparaging assertion that "the new ⅛th cannot be claimed to supersede the highest powers now in use, yet nevertheless is not, in my opinion, injurious to the ⅛th." Mr. Charles Stodder writes, with good authority, probably under the date July 20th, 1871 (this was nearly two months before I saw Dr. Woodward's "Note"), "He has now ascertained that the Powell and Lealand so-called $\frac{1}{16}$th objectives are really $\frac{1}{10}$th; the so-called $\frac{1}{18}$th is a $\frac{1}{19}$th."* This did not reach this country until after my "Remarks" had been sent to this Journal, and I knew nothing of Mr. Stodder's communication until I saw it in print, and he knew nothing of mine until he saw it in print.

In the number of this Journal for April, 1872, page 166, Dr. Woodward alludes to a so-called $\frac{1}{30}$th by Wm. Wales, and after, "a determination of its magnifying power, however, shows that un-

* See 'M. M. J.,' vol. vi., p. 203.

covered it magnifies rather less, at covered rather more, than the Powell and Lealand $\frac{1}{16}$th." Further, "its equivalent focal length at uncovered is therefore $\frac{1}{16}$th, very nearly." The same is repeated in the 'American Naturalist' for April, 1872. Why does Dr. Woodward still persist in calling an objective a $\frac{1}{16}$th, which he has repeatedly admitted to be higher than a $\frac{1}{16}$th, and which Mr. Charles Stodder writes he has "ascertained to be a $\frac{1}{19}$th"? On page 30, vol. vii., of this Journal, Dr. Woodward has published a table of powers in decimals of an English inch. In this table he gives the *dry* Powell and Lealand $\frac{1}{16}$th at uncovered point the expression ·0623. That is all right as far as the dry front is concerned, but if the reader wishes to know the power of the immersion front at its lowest power he may substitute the decimal ·0533, and he will be not far from right.

I can still furnish further evidence to prove my position, but do not think it at all necessary. The members of the Royal Microscopical Society can count the lines in the glass positives, which I believe they have duplicates of, and satisfy themselves of the truth or otherwise of my statements. I counted the lines with a low power, 4-inch, and positive eye-piece, and was very careful to count the same part of the same frustule in both cases.

Dr. Woodward brought out his tables in "fair play" to Messrs. Powell and Lealand, and undertook to show that the objective was *not* of higher power than its denomination. I write this communication in order to show that the immersion front made it of *much higher power*, and those makers whose objectives are correctly marked, or nearly so, would not have "fair play" if their work was tested against this objective and its actual power not stated.

V.—*On Uniformity of Nomenclature in regard to Microscopical Objectives and Oculars.* By R. H. Ward, M.D.

The *nominal focal length* of an achromatic objective, as used by microscopists generally, represents its amplifying power as actually used in the compound microscope. Even the equivalency in amplifying power with a single lens of the same focus is no longer distinctly realized, while the size and appearance of the combination, its working focus, angular aperture, and microscopical efficiency, are not even hinted by the figures used. The nominal focus represents the magnifying power and those properties dependent on it. Like other measurements, these must be stated by comparison with known standards. To use diverse and unknown units of measurement in cases designed to be compared with each other is simply

self-stultification. To call two lenses, of identical magnifying power, respectively ¼th and ⅛th inch lenses, is just as indefensible as to call two houses of equal height, 40 and 50 feet high respectively. To argue against the existing looseness of usage in naming lenses, is only to state what everybody knows in regard to the advantages of uniform standards of measurement generally. So impressed are many microscopists with the urgency of this question, and so determined are they to escape from some of the present confusion, that a committee has been appointed to report on the subject. Though that committee is unprepared to report, it is believed that giving publicity to some facts and opinions involved in the consideration, may lead to useful agitation and to increased definiteness of ideas and of information in regard to it. Of course it would be premature to claim or expect accuracy of statement or safety of opinion in such a complication of disputed questions; and what is said, is designed to be contributory and suggestive, and in no degree dogmatic or final.

The great variation in objectives of identical name is familiarly known and is undisputed. Among other people one-fourth of an inch is less than four-tenths and more than one-fifth; but among microscopists it may often be more than the first or less than the last. An indefinite number of figures might be published to prove or illustrate this irregularity, the writer having been particularly interested in making and recording these comparisons for more than a dozen years, and Messrs. Bicknell, Biscoe, Higgins, Cross, and many others having been especially interested in the same study; but it is idle to prove what everybody knows and admits. So familiar have some of these apparent errors become by use, and good usage too, that they have been often accepted as established, even one of the latest authorities * stating the power of the ¼-inch objective five times as high as that of the 1-inch.

In the early days of the compound microscope as a really useful instrument, we find microscopists wishing that microscope makers would "grind their glasses to some settled standard."† We are willing to be more reasonable now, or else the conditions stated have become more difficult. We do not desire, nor consider it practicable, that the opticians should make all their combinations of certain definite and conveniently-graded powers; but we do propose so to name our powers, if we can, that each number shall group together all those powers of which it is the nearest and best description.

Makers would doubtless be considered as doing a favour to those who use their instruments if they would, after finishing lenses, carefully estimate their powers and name them by the frac-

* Suffolk, 'Microscopical Manipulation.' London, 1870.
† Baker on Microscopes. London, 1742.

tions most nearly representing those powers. But even if this were done, and much more now when this is certainly not done, or not done upon such a uniform plan as to be satisfactory, microscopists should always re-examine their lenses, in order to be definitely informed in regard to one of their most important properties.

The easiest method of examining the *magnifying power* of an objective, by measuring the image (of a known object) which it forms at a standard distance (now 10 inches), was as well understood a hundred years ago as now; a lattice of fine silver wire or of human hair, or a scale ruled on glass, being used to measure the image.* A positive ocular † or the eye-lens of a negative one is used as a simple microscope with which to read off the measurement. If a separate piece of apparatus were to be made for the purpose of measuring these powers, a positive ocular, with micrometer attached, would doubtless be preferred, it being placed by means of the draw-tube or some other contrivance at such a height that its micrometer should be 10 inches from the objective. Its reading would then give the real size of the image formed at that distance by the objective, and the ratio of this number to the known size of the object, say the distance apart of two lines on a stage micrometer, would give the magnifying power of the objective. But as few are possessed of a large variety of apparatus, or care to buy a piece for so infrequent a use as this, the measurement is generally made with an arrangement which every microscope ought to include—a negative ocular, with a micrometer in the focus of its eye-lens, whose advantages for general micrometry are so well understood, giving the best view of the object and a sufficiently good view of the measuring lines, that it is usually preferred for that purpose. Of course the field lens is removed in measuring the power of the objective alone,‡ but replaced for ordinary work. If it should be thought best to name lenses by their magnifying power alone, the power ascertained could be at once attached to the lens, the present 1-inch lens becoming No. 10, or x 10:§ but if it should be the usage to name it by its power when combined with some standard (say 2-inch) ocular, it would be marked No. 50, or x 50, or perhaps x 45 or x 55. Should it be preferred to retain the nomenclature by inches of focal length, a power of ten dia-

* The measurement of the image, formed by the objective only, on a screen at a distance of several feet, as employed by Dr. J. J. Woodward at the Army Medical Museum at Washington, is unquestionably the most reliable method of determining the amplifying power; but is a method which requires too many applications and too much skill to be universally applicable.

† The convenience and growing popularity in America of this Continental term suggest the propriety of its general substitution for the awkward name eye-piece.

‡ Dr. J. J. Higgins, in the 'American Naturalist,' Dec. 1870, p. 628.

§ It might be 9 or 11, and thus the various degrees of power would be conveniently expressed.

meters might be called a 1-inch lens, and powers above and below rated in proportion. This plan is within reach of the opportunities of every microscopist, while the plan of actually employing a single lens of small aperture and exactly 1-inch focus as a standard of comparison is only adapted to the use of the opticians, and is not free from question as to what standard is meant after all. The lens made as a standard is probably not a 1-inch lens at all (principal focus), for the principal focus is never used in the microscope; and authorities differ as to whether it should have conjugate foci of 1 inch and 10 inches, or 10 inches apart (1 inch and 9 inches). Assuming x 10 as a 1-inch power, would be most easily applicable and unmistakable; and this power (ten), divided by the ascertained power of any ocular or objective, would give the equivalent focal length of that objective or ocular without comparison and beyond dispute.

The chances of error in this case are the same as in ordinary micrometry, with one or two additions, and should in all cases be ascertained in order to test the reliability of any series of observations. They are due to the uncertain value of the divisions of the stage micrometer, to the like (but less important) variability of the measuring scale, to the uncertainty as to the exact optical correspondence of the lines selected for comparison in the two scales, and to the uncertainty as to obtaining exactly the assumed distance between the upper scale and a given point of the objective. The first of these errors is the largest, and its magnitude would surprise many who have noticed and admired the remarkable "perfection" of the common micrometers. A micrometer which ought to be the best in the writer's possession, with lines 100, 1000, and 2000 to the inch, has a certain error of ·02 and a limit of error of ·035. This is entirely too much latitude for a single source of error, and of course it is nearly eliminated by comparing a large number of spaces belonging to at least several different scales, rejecting any scales which by differing widely from the average standard are presumably erroneous, and averaging the rest. The remaining sources of error may be similarly reduced by averaging, though their aggregate limit of error, ascertained by comparing the average measurements with extreme figures beyond which there is no possibility of doubt, will be found to be very small and inconsiderable.

The standard *distance of measurement* in estimating magnifying powers may be stated to be, at present, 10 inches. The distance of 5 inches has been recommended, even somewhat recently,* and 8,† 9,‡ and 10§ inches have been successively used. The smaller number were evidently too small, and the last, 10 inches, seems to be permanently accepted as most correct theoretically and

* Brocklesby, N.Y., 1851.
† Baker. London, 1742.
‡ Martin Fokes, Esq., P.R.S., 1742.
§ Lardner, Carpenter, Suffolk, &c.

most convenient in use. If, however, the metric system were to come into general use, this distance would be changed to 250 millimètres, with increased convenience, and with a scarcely appreciable difference in results. The sooner such a change is made the better, provided it is certain to come at all, and possibly it might be considered only a fair concession to the convenience of the great number of Continental microscopists, and to the excellence of their metric system, to make this change without further delay.

The propriety of measuring the image at this standard distance, when estimating the power of objectives or oculars is undisputed, and it would seem equally indisputable that the whole power of the compound microscope should be obtained in the same manner, were it not that the authorities have always differed in regard to the subject. When Hooke, Griffith, Hogg, and other eminent authorities have directed that the image should be measured at the distance of the object on the stage, and Lardner, Carpenter, and Suffolk, in common with most microscopists, measure the image 10 inches from the eye wherever the object may be, it is useless to appeal to authorities. It would seem, however, that the former direction, to measure the image at the distance of the object, must be an advertency which could lead only to confusion. The writer has fully stated this question in a recent review,* and therefore omits further discussion of it here.

A more difficult question is as to the *point in the objective from which the measurement should be made.* If the objective had an optical centre and we could find it, there would be no difficulty in the case. But the modern objective has no permanent optical centre, at least none that we can easily find and use, and unless some one can give us a better rule, we may be obliged to measure from the bottom of the whole system, or from (about) the centre of the lowest pair or set of lenses. Mr. Charles R. Cross † has proposed to evade this difficulty by measuring 10 inches between the conjugate foci used, without regard to the position of the objective—a plan which would be very eligible with high powers, but inconvenient if not inapplicable with low powers, since few compound microscopes have a body short enough to bring the conjugate foci within 10 inches of each other with very low objectives, and, if they did, the magnifying power, instead of being that generally used, would be greatly reduced or altogether suppressed.

The very low-power objectives (say 4 and 5 inch) are usually mounted short in order to leave sufficient room between them and the stage, and their power, as ascertained by an arbitrary rule, would be greater than that at which they are usually worked, unless, in their ordinary use, the draw-tube were habitually raised enough to compensate for the shortness of their mounting.

* The 'American Naturalist,' June, 1871, p. 229. † Boston, 1870.

At what point of *screw-collar adjustment* the angular aperture and the magnifying power should be computed is one of the most complex questions involved in the discussion, and an entirely unsettled one. Most makers state the angular aperture of their lenses at its highest point, but no such uniformity of usage exists in regard to their magnifying powers.

With the lenses of a dozen years ago this would be comparatively unimportant, but with many of the high-power and high-angle lenses of the present day, the effect of the screw-collar movement is too great to be disregarded. It has been proposed, and would be most easy, always to rate objectives at their arrangement for uncovered objects, this being a naturally fixed point, and the only one at which the maker's judgment in regard to the accuracy of the correction is usually known: but this usage would greatly underrate many of the high objectives. On the other hand, rating them at their highest adjustment, or at an average between the two, might be vitiated by the fact that the point of highest correction is not a natural and fixed one, but is somewhat dependent on the judgment or caprice of the maker, some lenses of equal power being capable of a much larger range of corrections than others are. And finally, if we could agree upon some standard thickness of glass, and the glass were sufficiently uniform in refracting power, the same standard would scarcely be convenient for all powers (low powers being generally worked by the great majority of microscopists through glass, say $\frac{1}{100}$th or $\frac{1}{125}$th inch, for which many high powers are incapable of good adjustment), and few microscopists are sufficiently expert in the use of the screw-collar to make the same adjustment from the same glass-cover. Adopting the highest point of adjustment would perhaps involve the least change from present usage; and in cases of unusual interest or importance it might be well to give both extremes, or else to specify the angle and power at which the combination was worked to accomplish the results specified. Attention need hardly be called to the fact that this great increase of power and angle, amounting sometimes to more than one-half of the minimum amount, is due entirely, not to the interposition of the cover-glass or other medium, but to the change in the relation of the lenses caused by the movement of the screw-collar. Where an extra front of different properties is added, we have essentially another objective whose power and angle should doubtless be separately stated.

The use of *linear measurement* in recording and stating powers has become so general that there may now be said to be no respectable deviation from the custom. In the early history of microscopy, powers were generally stated, according to the visible flatness or depth of the object, in superficial or cubical measure, and it was plausibly urged that this represented the real, visible enlargement

of the natural object; but, aside from the inconvenience of the large and often incomprehensible numbers thus obtained, this method gives in one sense the magnifying power, but in no sense the microscopical power employed. The power to see small things depends, so far as real or apparent size is concerned, on the distance from each other of minute points of structure, and this is in the exact ratio of the linear magnifying power. Squaring or cubing this power has acquired a suspicion of sensationism, if not of charlatanism, and is generally avoided in science.

If anything could be more confused and confusing than the different real and nominal powers of the objectives, it would be the corresponding powers of the *eye-pieces* or oculars. Made without any pretence of uniformity, and named without any serious attempt at significance, it has seemed until recently that no escape from the confusion was to be looked for. Yet it would seem to be convenient and altogether unobjectionable to have the oculars so named as to express their magnifying power, and the practice of doing this has been already introduced into this country. Some microscopists have renamed their oculars by their magnifying power, on the basis of one inch to ten diameters, and I am informed by Mr. Bicknell that Tolles has already adopted the same plan, in naming those of his manufacture, discarding the letter nomenclature (A, B, C, &c.) and selecting 2 in., 1½ in., 1 in., ¾ in., ½ in., $\frac{1}{10}$ in., and ¼ in., giving powers of 5, 7½, 10, 15, 20, 30, and 40 diameters. The writer has applied the same names to his oculars, applying the intermediate fractions $\frac{8}{10}$ inch, ⅔ inch, and ⅓ inch, to intermediate powers; and he is satisfied, by experience of its convenience, that this nomenclature only needs a trial, to secure its adoption by all who use the microscope for other purposes than amusement. Of course any microscopist, having determined the power of an objective and the powers of the microscope when that objective is used with his various oculars, can obtain the powers of his oculars by dividing the latter numbers by the one first named, and can then name his oculars, like the objectives, either by their magnifying powers or by their equivalent focal lengths. The rivalry of makers and the interests of trade are not involved in this case as in that of the objectives, and there may be no reason why this plan, if as acceptable to microscopists generally, as it has been to a few, should not come into immediate use.

In order to work the objectives and oculars at their standard powers they should be, of course, about 10 inches apart, either by length of compound body or by use of draw-tube; and it is believed that most objectives whose corrections are accurate enough to show any difference will work best at about this distance. Should a decidedly different distance be used in any observations of importance, it would be well to state that fact in recording the observation.

In reviewing this subject the following *points* would seem to be reasonably well *settled*. Objectives should be, and could be to a much greater extent than they now are, rated according to a uniform standard. They should be named not arbitrarily, but in a manner indicative of their magnifying power. Ten inches is the standard distance of measurement in estimating powers. This distance should be taken from the eye to the rule by which the measurements are made, without regard to the distance of the object on the stage. Magnifying power is always stated in linear measure. The magnifying power and angular aperture, as well as the maker's name, should be engraved on all objectives, and added to all particularly important drawings made by their means. Oculars should be named, like the objectives, in such a manner as to indicate their magnifying powers or equivalent focal lengths.

The following are some of the more important *queries* which still remain *open*. Should the standard 1-inch objective be characterized by magnifying ten diameters as used in the compound microscope, or should it be compared to a simple lens of actually measured focus or foci? Should the objective be named by its equivalent focal length, or by its amplifying power, or both? Should our standard distance of measurement be changed from 10 inches (254 millimètres) to 9⅞ inches (250 millimètres)? From what point in the objective shall the distance to the scale be measured? At what point of screw-collar adjustment shall the objective be placed for rating its angular aperture and amplifying power? Should the name *ocular* be substituted for "eye-piece" in general use?—*The American Naturalist.*

VI.—*Notes on some Microscopic Organisms.*

By Prof. A. M. Edwards (U.S.A.)

The following observations I desire to have recorded, the more especially as, from all I can learn, they are unique, or, at least, have not been seen and published by anyone else. At the same time, I wish now to place upon record some discoveries connected with the life-history of certain animal organisms of minute size which I consider of importance and throwing considerable light upon a rather obscure portion of Biology, namely, the so-called subject of "Spontaneous generation." And I must say that I cannot but think that the use of this title, to designate the phenomena considered, is extremely inappropriate, and had best be discarded, as it does not properly indicate what is meant, and at the same time, I am sure, often prejudices persons in advance against

even considering the record of experiments or published deductions. A really more correct name to use would be that proposed by Prof. Huxley, of Abiogenesis, until the whole matter has had a fairer hearing than there is a desire to accord it in certain quarters at the present time. Such plain and evidently truthful records as those of Pouchet and Bastian cannot be sneered aside by using the weight of any name, however worthy of honour and respect; and whatever the deductions to be drawn therefrom, the spirit of fairness, which should always influence the acts and words of truly scientific observers, demands, that no aspersions should be cast upon what they, or others working in a similar direction, may have already or should hereafter publish. I speak thus, for I have myself, in a degree, had to encounter much of this very unfairness, and I shall hereafter claim an immunity from it when I publish, as it is my intention to do, some experiments and deductions of my own, tending, as I hope, to assist in elucidating this interesting and important subject. In the meantime, and even aside from this connection, I desire to call attention to the value of the observations I have been so fortunate as to make, and which I now, for the first time, put into print.

It is my intention to make this communication as brief as possible, the more particularly as, although the observations described appear at first sight to be tolerably complete, yet, for my own part, I must consider them as but partial and fragmentary, and merely as memoranda of a phase in the hitherto insufficiently studied life-history of a group of plants a more thorough knowledge of which is of the utmost importance and interest. Therefore I thus set down what I have myself seen, intending to follow it up with more extended researches as opportunity offers. At the outset I would remark that the investigation of such phenomena as I now mention requires no more special apparatus than a tolerably good microscope furnished with a magnifying power of about four hundred diameters, and which is usually and conveniently obtained by employing a one-quarter or one-fifth of an inch objective on the usual ten-inch length of tube or body, and a B or No. 2 Ocular. But what is certainly required is a large share of patience and perseverance, and is, perhaps, exemplified by the fact that while studying this subject, as I have mentioned, at one time I remained at the microscope, only at long intervals removing my eye from it, for the greater part of three consecutive days. That is to say, certain points were observed about the middle of one day and all the rest of that day and evening; the whole of the next day and evening and nearly all of the succeeding day were spent in these investigations, watching the changes and transformations I am about to describe. And this was only at one time, for many hours thereafter were spent in the same

way, and I feel sure that it is only by such earnest application that we can acquire any knowledge of the phenomena peculiar to what we know as life which will be of any lasting value.

The plant I examined belonged to the genus *Œdogonium*, of Link. The particular species was undetermined, and when it is considered upon what slight and evidently artificial characters species have been founded among the Cryptogamia, I am sure that I will be excused from venturing upon such delicate ground, especially as by so doing I run no danger of offending those who do not recognize the existence of species at all, or, on the other hand, puzzling those who do recognize the existence of natural species, but are not anxious to have them multiplied. The form observed is extremely common, growing in clear springs in various sections of the country, and is especially plentiful around the city of New York. To the unaided eye it appears as a bunch of fine filaments of a light green colour, and by the unknowing collector of microscopic wonders is usually classed under the inclusive title of "Confervæ." My first investigations were made in the month of April, a season when this peculiar phase of this plant appears to be common. And here I would impress upon students of nature, and more especially those who use the microscope to assist them in their investigations, the great advantage to be derived from making collections early in the spring. At that time the young stage of many organisms may be procured, and, as that season is also the time of rapid change and development, we are placed in the very best condition for watching transformations that may take place, and determining the true position of many forms which might be, and in many cases, I am convinced, have been ranked as perfect species. Let the young biologist remember that our knowledge of a species is never complete until we have made ourselves acquainted with the whole cycle of its existence from the germ to the adult, from the parent to the offspring; and the early spring in such latitudes as have a severe winter, during which almost all life lies dormant, is the time for such studies.

Up to this time the amount of knowledge bearing the portion of the life-history of the germs Œdogonium of which I desire to treat, is very incomplete; there being gaps which I think it will be found that what I have seen and have to describe, just fit into and fill. The perfect plant itself consists of an extremely fine, green-coloured filament, cylindrical in form, and having its frond divided at regular intervals by partition walls, so that the individual plant may be represented by a series of tubular cells or boxes, like tall pill-boxes, united end to end. The enclosing wall of these cells is itself colourless, and it would be difficult to see it, did it not possess a different refractive power upon light to the water in which it floats. It probably consists essentially of the same substance,

chemically considered, as the enclosing cell-wall of the Desmidiæ, and commonly known as cellulose (a term, by the way, used in an extremely loose and indefinite manner), and is possessed of considerable tenacity, as will be seen to be the case farther on, and is evident by the fact that we can handle the filament quite roughly without tearing the cells asunder: thus it is common to find this plant growing in running water where it withstands the flow of the stream. The mode of reproduction known to exist is, as far as I can learn, by the shrinking inwards of the inner cell-membrane, commonly known as the "Primordial Utricle" of Mohl, and enclosing the cell-contents away from the tough cellulose coat, while, at the same time, the cell-contents themselves assume a more or less coarsely-granular condition, apparently from the enlargement of the individual particles of which it is made up. It is recorded that thereafter, at a certain period in this change, the outer cell-wall splits across at a point near to one end of the cell, and, while the lid so formed remains attached to one side the other and largest portion, the cell-contents escape from the cavity into the surrounding water, and gradually assume the spherical form. Thereafter there is developed upon one side of the sphere a ring of ciliæ which become more and more active until, at last, they move about with such energy that the little green globe assumes an extremely active motile condition, flying about for some time in such a lively manner that an observer meeting with it and unaware of its origin would certainly be inclined to rank it within the confines of the animal kingdom. In this state it has been called a "motile spore," but we will see, farther on, that it hardly can be ranked as an ovum in the ordinary acceptation of the word.

Up to this period our record, as hitherto published, is complete; but just here is a gap, and it has been my good fortune to make such observations as fill it and complete our knowledge of the life-history of this plant. It is known, however, that the motile condition just described is but transitory, and perhaps it may have been on account of the velocity of its movement, and the consequent difficulty of keeping it under observation, that the next step has not, as yet, been observed. In some unknown way, then, it has been supposed that the active spherical form assumes the static condition, develops rootlets or filaments, which serve to attach it to other objects, and then, being fixed, it thereafter by means of the usual well-known method of cell subdivision develops into a new filament resembling exactly the parent plant from which it sprung. As I have said, I have been enabled to show how the motile form is changed or, more properly speaking, developed into the static form, and, at the same time, I think, throw some light upon an important portion of microscopic biology. The changes and transformations which I herein record, I have seen not merely a few

times, but perhaps thirty or forty, so that I am enabled to speak confidently as to the accuracy of my notes, as I have watched the whole process. It is as follows. At first the bright green-coloured cell-contents, around and investing which I have not been able to satisfy myself that I have seen a "primordial utricle," grow gradually coarser in texture by a process of differentiation of the mass in such a way that granules appear which increase in dimensions at the expense of the surrounding and investing substance, until the whole cell is filled with a coarsely-granulated mass, differing little in colour from the original cell-contents. At the same time the whole green mass recedes somewhat from the enclosing cellulose wall, and instead of filling it completely withdraws itself in such a way that its outline, near the ends, becomes rounded. This change is by no means slow, but, when the dimensions of the plant are magnified by the microscope, appears to be rapid. I should say, although I have not timed it, that this stage occupies from fifteen to twenty minutes in its accomplishment. Soon thereafter the cell-contents contract still more, moving towards one end of the cylindrical cell. Then, with a sudden snap, fracture takes place almost entirely across the tough cell-wall at a point about one-twelfth of its length from one end, that is to say a portion measuring about one-twelfth of the length of the whole cell splits across with a perfectly smooth and even fracture, still adhering by a very small portion, and looking like the lid to a box, is thrown back more or less, so as to expose the transformed cell-contents to the surrounding fluid. Occasionally but not commonly the fracture extends entirely across the cell-wall, and the two resulting portions become separated. Usually it occurs in the way I have described, so that after a while, and when all the cells of a filament have thus opened, it becomes transformed into a chain of straight portions bent usually in one direction in consequence of the dehiscence occurring for the most part along one side of the filament only. But now the green cell-contents move towards the openings thus formed, and slowly and steadily push themselves outwards, and, being elastic, escape after the manner that a small elastic sac filled with semi-fluid contents would escape from the hand if pressure were brought steadily to bear upon it. But in this case the emergence is not caused by the closing in of the cell-wall, but by a motile power resident in the elastic sac and its contents. It is not shot forth suddenly from the cell cavity, but squeezes itself out, and as soon as it has escaped assumes the form of a perfect sphere, and, as if exhausted by its previous exertions, comes to a rest. But although the whole mass is now in motion, the cell-contents do not remain at rest, but go on to another change. The bright green, coarsely-granular sphere being at rest, is seen first to become somewhat clearer upon the surface, and evidently a process of differen-

tiation goes on by means of which a very delicate investing membrane is formed, but it is so delicate that it can only with difficulty be seen, and in fact can hardly be said to exist as a separate membrane. But upon one side is now seen to appear a bulging outwards of the mass until a nipple-like protuberance is formed, which however is not filled with the green matter formed elsewhere, but is clear and colourless. I have noticed that in most cases this protruding portion is formed at the point just opposite to the place in the cellulose cell-wall of the filament from which the spherical body has just escaped. But as this is not invariably the case it can hardly be considered as anything more than accidental. Soon thereafter there is seen to be a slight agitation going on upon the surface of the sphere, near to, and upon one side of the clear space. This movement then assumes a more definite character, and at last a moving cilia is seen to be formed or differentiated from the outermost portion of the globe. Soon another and another are formed in the same manner, attached by their bases around the clear space, and soon, that is to say in fifteen or twenty minutes, a ring of active ciliæ is seen to surround the nipple-like projection. As soon as they have all made their appearance, immediately they all begin to move together and in a violent manner, so that motion is imparted to the whole mass, and it swims about actively through the water. And this motion is so active that it is often difficult to follow the mass, and to keep it within the field of the microscope. For several minutes, or more rarely, for half an hour, this motion goes on, and up to this point I find that these changes have been observed and recorded by others. But now comes what I consider the most important part of this history.

It can be readily understood that, on account of its violent action, the further history of the spherical mass could not easily be followed, but fortunately on several occasions I saw specimens entangled in a mass of filaments in such a way that they could not escape from the field of view. Then I saw that the motion became gradually less and less vigorous, and at the same time the ciliæ disappeared one by one, melting out of view, being apparently absorbed again into the mass from which they were originally developed. At the same time one of them seems to elongate until it is one to three times as long as the diameter of the spherical mass, and also its point of attachment changes until we find it springing from the centre of the clear projection. While this is going on, the whole mass, not changing its position, assumes, however, a totally different outline. That is to say, the clear nipple-like blunt part remains about the same, but the opposite portion becomes pointed, while the intermediate space is inflate in such a way that the whole creature in outline somewhat resembles a trefoil clover. The inflated part, however, does not remain rigid, but

soon subsides, while the clear end becomes more pointed, and now the creature is spindle-shaped in outline. As there is now but one cilium, it is by means of it that the creature moves about in an extremely active manner. In some, and in fact in by far the most cases I observed, the swelling of the middle portion is not fixed, but moves down the length of the creature towards what may with some propriety be termed the posterior extremity, as it is always projected backwards when it is moving through the water, and there disappears, to be soon followed by another swelling and wave-like projection, and so on. In fact, an action resembling very closely that seen to take place in the intestines of animals, and known as peristalsis, takes place. Many individuals move directly and straight onwards, preserving their body perfectly rigid, so that after a time the cell-contents are seen to arrange themselves in longitudinal bands. Others, again, revolve on their longest axes, and soon the cell-centents of these are seen to have arranged themselves in spiral lines corresponding to this movement. Some of these retain their straight condition, while others become bent around so as to form almost a complete circle, and then proceed onwards by a rolling motion. There is a regular and determined passage from one of these states to the other, as I was able to ascertain by careful watching; but the most remarkable fact connected with the whole matter is, that all of these forms are precisely similar to creatures which have been ranked by Ehrenberg and others in the animal kingdom, under different names, but most commonly that of *Euglena*. To make the resemblance of the Euglenas still more marked, as soon as the circlet of ciliæ has disappeared, and the mass elongates, a bright red spot appears near the clear end, and usually also, one or more clear seeming vacuoles are seen to arise within the green mass. The red spot has been called an eye, and the vacuoles stomachs; and in this way Ehrenberg was enabled to classify these forms as "Polygastric Animalcules." The spirally-twisted forms have been placed in a separate genus, and in fact I have seen, in the way mentioned, developed from the cell-contents of a filament of Œdogonium, forms identical with several genera of "Polygastric Animalcules." After a little longer time the cell-contents have again changed in appearance so as to be coarsely granular, each granule being so large and distinct that it can readily be distinguished, and now the active motion of the mass ceases, and it takes on the static condition. This it does by increasing in size, elongating and losing its cilium and red "eye" spot, while the clear portion elongates, subdivides, and branches out and becomes fixed either to a full-grown filament of Œdogonium or some other submerged substance that may serve it as a support. Now the cell-contents become finely granular again, and arrange themselves against the cell-wall, which is thickened

considerably. Soon a bending in of an inner membrane or "primordial utricle" is seen to take place, and cell-division after the well-known method occurs, until a filament is formed exactly like that from which the original green sphere was projected.

The important points, then, recorded in this paper are the finding of the means by which the active spherical form is converted into the still state previous to growth into a filament. The most remarkable fact, however, is the identification of this phase with one or more of the forms which have been hitherto classed either in the vegetable or animal kingdom, most commonly the latter, according to the prejudices of the observer.

This note of the transformation of Œdogonium is the only one concerning the life-history of plants that I desire to record at the present time, but I have made so many detached observations very much of the same kind, that I wish to state that I am convinced it will be at some future day shown that all of the green, and some of the red-coloured forms similar to Euglena, and which have had several names bestowed upon them, are but transition states of fresh-water or marine Confervoid Algæ.

The second observation that I have to record is of certain phases in the life of animate organisms which have been commonly considered as belonging to the animal kingdom. But my notes here are more incomplete than in the case of the motile forms of the alga just mentioned, as it has been only within the last few weeks that I have seen what I am about to describe, and then only a few times, so that I wait for more opportunities for observation to confirm my experience. And here let me say that, apparently, the stages of change of these seemingly otherwise simple organisms I here record are, like the vegetable one just described, confined to the spring time of the year; and even then to a very few days. Of course these changes cannot be supposed to take place, for instance, within the space of one week, and in every individual in a single locality; but the changes are so rapid that it can only be by constant and patient observation that we may hope to see them occur, whilst slight modifying causes may defer or hasten the stage in different cases.

It was on one of the bright days during last spring that I collected in one of the pieces of fresh water in the Central Park in New York, a mass of matter made up of vegetable and animal material, but containing, as I knew, that which would yield material for observation and study by means of the microscope. Observing it, then, in that way, I was pleased to find in it numerous individuals belonging plainly to the group of organisms which have been grouped together under the head of *Amœba*. But remembering the observations of Dr. J. Braxton Hicks on the occurrence of amœboid forms in certain undoubted vegetables, I

was of course unprepared to assign them to a position in the chain of life without further study. To decide this question if possible, and ascertain the origin of these wandering masses of protoplasm, I watched them at intervals for the better part of two days, and I saw the following changes take place. From an almost hyaline condition the *Amœba* became gradually more and more granular, the granules increasing in dimensions until the individuals appeared to be packed almost full of dense oil-globules. Then they came to a rest, or at least their hitherto lively movements were arrested, and presently near one end appeared evolved, so to speak, from the mass cilia, one after the other, until a crown of them was seen surrounding what was plainly now a defined locality. At the same time a change was going on all over the *Amœba*, by reason of which at last from this simple mass of albuminoid material a true ciliated animalcule, belonging, I believe, to either the genus *Kolpoda* or *Paramecium*, which resemble each other very much, was evolved.

The question at once, then, presents itself, Is not the amœba, in this case, the young motile condition of the ciliated animalcule? Such is my opinion. But we will require many more observations to decide whether, as I am of opinion is the case, they all pass through this condition, and also, at the same time, to throw more light upon a field in which I have spent much time in observation, namely, the origin of these simple organisms like the *Amœbæ*. At some future time I may take the opportunity of detailing what I have seen in this direction.—*Read before the Lyceum of Natural History, New York.*

PROGRESS OF MICROSCOPICAL SCIENCE.

The Development of the Sturgeon.—In a late number of the 'Bulletin of the Acclimatization Society of Paris' there is an account of the sturgeon's reproduction (sturgeon of the Volga), by Professor Owsjannikow. It seems that the sterlet (*Acipenser ruthenus*), the smallest of the Russian sturgeons, spawns in the Volga early in May on rocky bottoms, the temperature of the water being at 10° R. (= 54½° F.). The eggs are readily fecundated by the artificial method. After they have been in the water a few minutes they adhere to any object which they touch. The development of the embryo can be observed in progress at the end of one hour. On the seventh day they hatch. At first the young fish are 0^{m}·007 (about $\frac{3}{100}$ inch) long. At the age of ten weeks they are nearly two inches long. They feed on larvæ of insects, taking them from the bottom. Both in the egg and when newly hatched, the sterlet has been taken a five days' journey from the Volga to Western Russia, and in 1870 a lot of the eggs were carried to England to stock the river Leith. This species passes its whole life in fresh water. The other species inhabiting the Baltic, *Acipenser sturio*, *A. Huso*, *A. stellatus*, and *A. Güldenstädtii*, are anadromous. These species hybridize, and freely, and from this circumstance some Russian savants have pronounced them only varieties instead of species.

The Stomata of Leaves are well described for a couple of species in the 'American Naturalist,' by Professor T. D. Biscoe. In studying the development of these complex organs, he takes the youngest leaf of the plant, and finds on its base (the youngest portion) no trace or hint of stomata. A very little higher up he finds the epidermis appearing, many of the cells having built a partition across their front end, cutting off about a quarter of the original cells. These small cells are distinguished from the remaining portion of the originally single cells, and from the undivided cells, by being filled full of granular protoplasm, while the other cells are only partially filled with the protoplasm constituting the nucleus. These little cells, called mother cells, soon grow so as to become longer than broad, and are raised by the more rapid growth of the surrounding cells so as to leave an air-space below. An approach to a spherical form is now made by the mother cells, and the walls of the neighbouring cells are a little thickened with the deposition of cork substance giving the first trace of the thick-walled square of the ripe stomata. Next the mother cell divides by the formation of a thin partition which runs in the direction of the point of the leaf, and is perpendicular to its surface. Soon this partition thickens in the middle, and splits through the thickened portions to within about a fifth of each end. All further growth only effects minor changes in the form of the cells, or an increased thickening of their wall.

The Structure of Graptolites.—Professor Allman, in a recent paper "Annals of Natural History," May, deals with the affinities of these

animal remains. He thinks that certain of the projections upon them are the equivalents of the *nematophores* in living Hydroida.

The Double Markings of Triceratium are the subject of a paper by Col. Dr. Woodward, in the 'Lens' [last number], and are illustrated by an admirable Woodbury Photo-relief print, which brings them out with the most marvellous distinctness. These photographs we wish we could reproduce, for they certainly most fully bear out the remarks made by Dr. Carpenter in his work on the microscope, when he says that "in certain species of Triceratium, Coscinodiscus, and Actinocyclus the floors of the hexagonal depressions are studded with markings resembling those of Pleurosigma, and these are particularly conspicuous in the beautiful Heliopelta." Previous attention had been drawn to this circumstance by Stodder, Ralfs, and Reynolds. Dr. Woodward states that he has repeatedly verified the accuracy of the above statement for all the genera named, but limits himself in the present paper to a few remarks on a single species of Triceratium. Professor Christopher Johnston, of Baltimore, very kindly lent him, not long since, a slide containing a large frustule of *Triceratium fimbriatum*, on which the beadings, which with high powers can be made out on all the species of Triceratium, were remarkably distinct, and hence capable of being shown with a comparatively moderate magnifying power. The author then describes the method of photographing adopted by him for the two photos which appear in the illustration. He states that when the rows of dots seen in the picture of the broken frustule are examined by immersion objectives of the highest power and the best quality, they appear with white light as round garnet-red beads on a greenish ground, approximating in size and appearance the beads of *Pleurosigma angulatum*. He has endeavoured to obtain photographs of them with high powers, and sent silver prints of the results to the editor and the society. The comparative thickness of the frustules, and the marginal walls of the areolæ, interfere considerably with clear definition, with these high powers; still, he thinks an impartial examination of the photographs sent will dispel all doubts as to the real nature of these markings.

A Skin Disease (Molluscum contagiosum) resulting from a Parasite. —It is stated that Mr. Balmanno Squire thinks that he has discovered parasitic spores in the sebaceous matter which forms the pulp of the tumours in molluscum contagiosum. The spores, he thinks, are somewhat longer than those of the parasite of favus. Mr. Squire believes that the parasite causes, by the irritation it excites in the sebaceous glands, the increased secretion of sebaceous matter which characterizes this disease.

The Microscope in Entomology.—According to Mr. S. H. Scudder, who writes in the 'Entomologist's Monthly Magazine,' the microscope may be of advantage in the study of caterpillars—of course with low powers. But with such assistance he says that the shape and sculpturing of the head, the form of certain segments, and especially the precise number, location, and disposition of the spines, thorns, and hair-emitting warts of the body will be found to furnish

abundant means of distinguishing the most closely allied and minutely subdivided genera.

Saccammina Carteri, a Foraminifer from the Carboniferous Limestone of Northumberland.—This discovery, which is in stated the valuable Natural History Transactions of Northumberland and Durham for 1871, we regret not having seen earlier. Still, as it is of interest, we lay it before our readers even now, as many of them may not have seen the volume which records it. It is a curious Foraminifer, and is amply illustrated by a very beautifully-executed plate in the end of the volume. A question appears to arise whether apparently independent specimens were originally connected together. Thus, says the author of the paper, Mr. H. B. Brady, it is a question whether these bodies represent individual animals, or to what extent they may have been connected with each other when living. It is not at all unusual to find on any weathered piece of the rock two segments connected by a stoloniferous tube—rarely three are found in this condition—and in one or two instances four or five have been noticed still retaining connection with each other. The bulk and weight of the segments and the comparative tenuity of the intermediate processes would be sufficient to account for the separation into single chambers, were this less constant than it is; but there is no need to suppose that the single segment may not represent a perfect animal equally with the many-chambered shell. Occasionally, though very rarely, a chamber is found with a round imperforate base and a single orifice at its apex, and if this is taken to correspond to the ordinary form of *Lagena*, the fusiform chambers may be regarded as analogous to the distomous varieties of that genus. The moniliform fossils might be compared to the *Nodosariæ*, but that all that have as yet been met with have an aperture at each end of the series of segments, and, for anything known to the contrary, the test might extend itself indefinitely in either direction. The test is composite and arenaceous, the constituent particles being fitted and cemented together so as to give a nearly smooth exterior. The size of the sand-grains and their mode of aggregation is a character of some importance amongst the recent *Lituolida*, but, as has been before stated, the process of mineralization has obscured the minute structure of the fossil in these particulars. The interior of the test is commonly smooth, resembling the recent *Saccammina;* but it sometimes presents a surface of very short, delicate, labyrinthic, shelly ingrowths. This cancellated or labyrinthic structure is often met with amongst the arenaceous Foraminifera, and in some genera it is developed to an enormous extent. Here and there a specimen may be found with a sort of circular patch on the surface, which has the appearance of a cicatrix resulting either from the gradual closing-in of an orifice or possibly the reparation of some injury to the shell-wall. These slightly raised concentric markings, apparently deposited regularly from without inwards, occurring frequently and with considerable uniformity, can scarcely be accidental. The positions in which they are generally noticed, *viz.* the sides rather than the ends of the segments, is an objection, though possibly not a fatal one, to the supposition that they mark the closure of normal apertures. He has

named the genus after the same genus of Professor Sars, but the species after Mr. Carter, F.R.S.

Miasm viewed with the Microscope.—A series of researches, rather like those conducted some six years ago by Professor Salisbury, M.D., of Ohio, U. S. A., have been conducted by M. Bolestra, and recorded in the 'Comptes Rendus' of the French Academy. The water of the Pontine Marshes, and of similar malarious regions, he found to contain invariably, along with the common infusoria, a minute algoid vegetation, with an abundance of transparent, greenish-yellow spores, $\frac{1}{1000}$ mm. in diameter. This vegetation develops slowly in pure water and at low temperatures, but rapidly in the heat of the sun and amid decomposing organic material. It floats upon the water, giving an iridescent film when young, and its spores are found in the air near the marshes, and even at Rome, being most abundant in warm weather, and after a rain or during a fog, and least so in a cool, dry atmosphere. M. Bolestra regards these spores as the miasmatic agent in the production of the intermittent fevers for which the localities are badly celebrated.

A Conspectus of the Diatomaceæ.—Professor W. H. Smith has sent us the 2nd part of his 'Memoir on the Diatomaceæ,' which he purposes to go on with till it is completed. We observe with some satisfaction that the author has been at some pains to explain the more recent views of diatomacean structure in this part, which he did not do in the preceding one .Still, however, it is evident from the date of publication, that this was done before our previous remarks had come under his notice. He believes that all the frustules are "siliceous boxes," with either one portion (the cover) slipping over the other, as in *Pinnularia*, or with edges simply opposed, as in *Fragilaria*. If we take a frustule of *Melosira*, it may be compared to a pill-box—one portion slipping on to the other. The great majority of diatoms are thus constituted. It is perfectly evident that, in the case of the formation of a new valve, in the processes of self-division, this new part, which slips out from the older, must be somewhat smaller. Inside of the box is a membrane, inclosing the internal coloured or colourless substance, imbedded in which may always be seen, at least in the larger forms, a distinct nucleus, sometimes two, and sometimes a "germinal dot," with numerous fine threads radiating from the nucleus, or the germinal dot. As the frustule increases in width, one portion slips out from the other, and sometimes successive additions of siliceous matter are made to the edges of the box, somewhat analogous to the successive additions to the edge of the shell of a mollusc. When the widening of the frustule has reached a certain extent, the lining membrane at the places which would be exposed if the two portions were wholly to slip apart, infolds. He has reason to believe that, previous to this, a double membrane of extreme tenuity has been formed, commencing its growth *at* the nucleus (which itself is divided), and extended to the margins of the cell, which is thus divided into two nearly equal parts; for as soon as this infolding commences (perhaps now accelerated by the admission of water), the line of division can

be seen progressing steadily inwards by the parting of this thin double membrane, so that in fifteen or twenty minutes the fissure is complete. He has, in very large *Pinnulariæ*, witnessed the whole phenomenon, from its inception up to the final self-division. While the *fissure* occurs in the short period of time named, to complete the *self-division* requires about six days. The part which slips out has, of course, one of the old valves upon it. Again, by self-division, the new valve becomes an old valve for the second-formed frustule; and so the frustules become smaller and smaller, as stated by Braun, 'Rejuvenescence in Nature,' Ray Soc., 1853, p. 132. The minute forms are, indeed, younger, or more newly formed, and, of course, more numerous. At this juncture, conjugation occurs, and a return to the normal condition of the original large frustule, by the formation of a sporangial frustule of double the size of the parent frustules. The following seems to be the author's admission of the peculiar elevations which undoubtedly exist in a certain number if not all cases. "As regards the terms "costæ" and "striæ," the distinction is altogether too vague to rely strongly upon them as characters; the term *pervious* costate, or striate, is sufficiently decided. Mr. Slack's researches appear to prove that even the coarsest costæ are simply very close or confluent dots. Only in the small forms, however, will any difficulty be experienced in deciding between costate and striate, as these terms are generally understood; probably here other characters will decide, leaving in these obscure cases the kind of marking as undecided."

Connection between Pyæmia and Bacteria.—At a recent meeting of the Pathological Society (May, 1872), Dr. Sanderson gave a series of demonstrations the object of which was, we believe, first to show the presence of bacteria in the infecting fluid of pyæmia, in the blood of living pyæmic patients, and in pyæmic abscesses; and, secondly, to prove that these living organisms were not introduced into the body from without, but were apparently produced within the body. It will be remembered that in certain experiments recorded in one of the reports of the Medical Officer of the Privy Council, Dr. Sanderson had shown that bacteria were introduced into fluids by contact with impure surfaces, and that when glass-rods, test-tubes, and the like, had been heated before being brought into contact with fluids, no bacteria were therein developed. Dr. Sanderson, we believe, had taken care in his experiments on pyæmia to avoid the introduction of germs from without into the peritoneal cavity. He first showed the liquid taken from the peritoneum of a guinea-pig twelve hours after the injection of fluid into it from the peritoneum of a kitten in which peritonitis was excited by the injection of peritoneal fluid. This liquid was swarming with minute bacteria, chiefly spheroidal, this being the feature of the liquids possessing the most intense infective character. The second specimen was the blood of an animal infected as the guinea-gig, and bacteria existed in the blood of the living animal taken from the heart. The existence of these organisms in the blood was regarded as the index of the general state of infection. The third fluid was that contained in the peritoneum of a guinea-pig twelve hours after injection into that part, of the subcutaneous fluid formed

in connection with an abscess produced by the introduction of the solution of ammonia beneath the skin, the injection being made with calcined instruments, and with the special object of avoiding the introduction of external agents; and this fluid contained bacteria. A fourth fluid was exhibited, *viz.* the liquid from an abscess of the thigh of a puerperal woman, removed seven weeks after her confinement, the fluid being obtained through a fine canula into a glass tube, both of which had been superheated, the latter being at once closed. This fluid contained living organisms in it at the first moment of examination.

Is there Loss of Epithelium in Cholera?—The 'Lancet,' May 25, has an interesting annotation on this subject. The writer clearly holds that the epithelium of the intestine is not shed in cases of this disease. He states that in an inquiry into the subject there are two things that require to be kept apart, first, the post-mortem evidence of this loss of epithelium; secondly, its relation to the disease, supposing it to be an ante-mortem phenomenon. He does not contend that intestinal lesions, or this separation of the epithelium, are not commonly found after death from cholera; but he asserts that the doctrine which regarded these morbid processes as essential to the production of its characteristic symptoms is an unproven, if not altogether false one. If Dr. Cunningham's experience was founded on the thorough and accurate observation of even one fatal case in which there was found little or no detachment or destruction of epithelium, it is clear that it cannot be an essential phenomenon. It is scarcely to the point to urge that this writer's observations are based on a limited number of post-mortem examinations, seeing that in a series of microscopical examinations of the dejecta of cholera cases he failed to discover the presence of such epithelium, unless very exceptionally. It must be remembered, moreover, that the published observations of Dr. Lewis, his *confrère* in the scientific investigation into cholera instituted by the Government, point to exactly the same conclusion. "Judging," says Dr. Lewis, "from the cholera stools which have come under my observation in Calcutta—several hundred specimens — I believe that not more than two out of twenty slides will contain distinct traces of columnar epithelium." Considering the rapid transudation of fluid into the intestines, and the maceration which the mucous membrane undergoes, he would expect to find the epithelium separated after death; but if its rapid desquamation be the cause of the symptoms during life, the wonder is that any should be found in the intestines of a patient, and that it should not all have been washed away with the dejecta.

Phases in the Life-history of Monads.—The following extract from Dr. Bastian's, F.R.S., paper, to which we referred in our last, are full of interest. The changes which have been thus observed, says the author, referring to previous observations, constitute a very remarkable series, of which the following is a brief summary:—The simplest living units (*Bacteria*) first swarm in the infusion; these become aggregated at the surface so as to form a "proligerous pellicle," in

which embryonal areas gradually appear: as a result of segmentation in these embryonal areas, specimens of *Monas lens*, $\frac{1}{3300}$" in diameter, more or less suddenly make their appearance; they increase in size, occasionally assume an amœboid appearance for a time, and are ultimately transformed into real Amœbæ. The transition is effected by the loss of the flagellum, the appearance of vacuoles in their interior, and the simultaneous manifestation of polymorphism and a creeping mode of progression; at the same time a nuclear corpuscle develops in the interior, and the whole animal grows considerably. At last the Amœbæ gradually cease to exhibit their characteristic movements, whilst they become more or less spherical and motionless. Ultimately a firm bounding membrane is produced, and they pass into the encysted condition, in which, although slightly smaller in size, they constitute spherules $\frac{1}{1650}$" in diameter. On the removal of some of this pellicle to the surface of a fresh infusion, the Monads and Amœbæ greatly increased in size; all the Monads gradually became converted into Amœbæ, and some of these at first went through the ordinary process of encystment, though at last (on account of some more sudden change in the fluid) they seemed suddenly to lapse into a morbid state. They were apparently unable to encyst themselves; and not being capable of continuing as Amœbæ, there sprang up in their interior a teeming progeny of new units (*Bacteria*), the production of which occasioned the final dissolution of the organisms in which they were evolved.

NOTES AND MEMORANDA.

The Best Mode of Carmine Staining the Tissues.—On this subject a valuable paper appears in the last number of the 'Lens,' by no less an authority than Col. Dr. Woodward. The writer specially alludes to carmine staining, and speaking of the supposed power which it gives of distinguishing living from dead tissues, he guards his readers against the hasty assumption that these important distinctions may safely be judged to indicate anything further than chemical differentiation. If the parts stained by carmine are entitled to be regarded as possessing any more significant vital properties, such as would justify the designation "germinal matter" bestowed upon them by Dr. Beale, and after him by so many English and American microscopists, this must depend upon quite other facts than their relations to carmine. Staining by carmine may be effected in a number of ways. The preparation originally employed was the simple ammoniacal solution, used first by Gerlach. It has subsequently been proposed to add glycerine or alcohol, or both, to the ammoniacal solution. Various formulæ of this character are strongly recommended in the books, and any of them can be made to answer. Thiersch, however, correctly pointed out, in his work on Epithelial Cancer, that the ammoniacal solution of carmine is not staple. First made, it stains less energetically than after a few days, when certain ill-defined de-

compositions have commenced in the fluid; but as these progress its staining energy again diminishes. It has also certain disadvantages when used after other reagents. Thus, if applied to specimens previously coloured by silver, the black markings are promptly dissolved and the preparation is rendered useless. Thiersch therefore proposed as a substitute a rather complicated fluid, in which the ammonia was nearly neutralized by oxalic acid, and decomposition prevented by the presence of alcohol. This fluid was advantageously employed at the Museum for several years, but its proper preparation requires care and skill, and it gives no better results than the simpler method which will presently be recommended. In the same work, Thiersch advised for certain special purposes the use of carmine dissolved in a saturated solution of borax; and, with some modifications, this method is probably the most useful of any yet proposed. According to his formula, one part of carmine by weight is dissolved in sixty parts of a saturated solution of borax, the fluid mixed with twice its volume of absolute alcohol, filtered, and the filtrate used for staining. These directions have been quite extensively copied. It will be found, however, that the greater part of the carmine, crystallizing in some as yet unstudied combination with the borax, remains on the filter, and the fluid which passes through is comparatively pale, and stains but slowly. On the other hand, the crystals which remain on the filter, if dissolved to saturation in distilled water, yield a fluid which stains with great energy and uniformity. At first it might be supposed that the same quantities of carmine and borax solution might be mixed, and would answer every purpose. So, in truth, it would, when first mixed. On standing, however, like the ammoniacal solution, this mixture decomposes. Precipitation by absolute alcohol effects some change in its composition which secures its preservation unaltered for a considerable time. The solution made by dissolving the crystals just described in distilled water should therefore be employed. It should be filtered and transferred to a well-corked or stoppered bottle, in which it may be preserved several months without deteriorating, and will be found to be the most convenient carmine solution for staining.

Application of the Microscope to Petrography and Mineralogy.—We learn from the 'Neues Jahrbuch für Mineralogie,' &c., that Herr Rosenbusch observes under the microscope the action of pulverized minerals with reagents, such as disengagement of gas, gelatinization, &c., which show when these minerals consist of mixtures. The method of effecting various reactions which can be observed under the microscope is described.

A New Old Book on the Microscope.—We learn from America that a book which was first issued nearly twenty years ago has just come out in a new edition. It is Professor Brocklesby's 'Views of the Microscopic World.' We regret to learn from some of our American contemporaries, however, that it is in many respects an inferior book to the old one, that in point of fact it has not been brought down to the time. Most probably we shall soon have a copy of the work ourselves, and can then pass our own opinion on it.

CORRESPONDENCE.

Optical Curiosities of Literature.

To the Editor of the 'Monthly Microscopical Journal.'

Boston, *May* 24, 1872.

Mr. Editor,—In your May issue the Rev. S. Leslie Brakey, A.M., takes notice of the reply that I gave to the question of B. in the November, 1871, number, in regard to the angular aperture of the famous ⅛th Tolles' objective belonging to the United States. The notice requires a rejoinder from me, if I will not submit to the imputation of a want of instruction, or in another word ignorance.

The notice reads, "In three of the cases the figures are no doubt correct; in the remaining one—the object immersed, uncovered—prompted by Mr. Tolles, he reports the aperture as 140°!!! Colonel Woodward . . . . also answers for the four cases, but better instructed than his countrymen on coming to the critical case, he leaves out the figures." There are several things involved in the above passages to which I ask the attention of yourself and of your readers. First, he *asserts* what it was impossible for him to know, if it had been true, and which he had no reason or right to assert, that I was "prompted" by Mr. Tolles. In fact, the figures were taken from the record of the sale, July 19th, 1871, and were made from the original memorandum (now before me) of the maker, June 27th, and correspond with those of Colonel Woodward's measurement communicated to me in July. The three exclamation marks are original interpolations of Mr. Brakey, I surmise, intended to signify his "surprise." We will examine further on the equity of that surprise. Mr. Brakey does not assign any *reason* for doubting the angle, he leaves that to be guessed at. Now I will assure him again that the angular aperture of the lens at uncovered immersion is 140°, and 175° at maximum. And what will "surprise" him again, perhaps, is that Colonel Woodward's own answer, to which Mr Brakey pays a compliment, says precisely the same thing. Colonel Woodward's words are,* "on coming to the critical case" "Its angle is then 140°. Without further change of adjustment a drop of water may be introduced, and it is fitted for the study of uncovered objects wet." This is exactly what the reverend gentleman says Colonel Woodward left out, and precisely what I gave, taken from the original record of the maker, not prompted by Mr. Tolles after November. Does Mr. Brakey know that immersion objectives can be used on objects mounted dry as well as on those in balsam?

These quotations show the animus that prompted the gentleman's paper: but there is more yet. The original question of B., now acknowledged to be Mr. Brakey,† reads thus:—"It would not surprise me to hear that Mr. Tolles' fifth has an aperture of 150° or 160°." In November he would not be "surprised" by an aperture of 160°. In April he is astonished to the extent of three exclamation

* 'M. M. J.,' January, 1872, p. 28. † 'M. M. J.,' Nov. 1871, p. 241.

marks by an aperture of 140°. He has apparently made progress in his study of optics.

When he writes the second chapter of the "Optical Curiosities of Literature" he can add his own lucubrations as the most ridiculous specimen of the Curiosities of Optical Literature.

I am happy to add that there are parts of Mr. Brakey's paper in which I heartily concur. I will mention his remarks (p. 224) on the ignorance of some of the working opticians of the science of their art, of the why and wherefore of the formulæ; and those on p. 229 on illumination and interpretation. These last cannot be too strongly impressed on the minds of all observers, whether expert or novices.

CHAS. STODDER.

N.B.—It is not my province to notice the attack on Mr. Tolles; I only wish to say that Mr. Tolles has not seen it, and it is not likely that he will for some weeks.

MR. BICKNEL AND MR. WENHAM.

To the Editor of the 'Monthly Microscopical Journal.'

CAMBRIDGE, MASS., *May* 29, 1872.

DEAR SIR,—On page 234 of the Journal for May, 1872, I find a communication from Mr. F. H. Wenham in an unusually venomous strain. I wish him to separate my identity entirely from that of C. S. Further, that I do not expect, *or want*, him to "apologize" for anything that I may have said; and also, to notice that the term "wilful deception" *does not* occur in any of my communications.

Very truly yours,

EDWIN BICKNELL.

To the Editor of the 'Monthly Microscopical Journal.'

BRIGHTON, *June* 5.

SIR,—Will you be kind enough to correct in your next number the following errata in the report you have given of my lecture or paper read before the Brighton and Sussex Natural History Society, "On the Natural History of Cure and Healing." The errors are important ones, as they render unintelligible *some* of the sentences in which they appear.*

I am, Sir,

Your obedient servant,

W. ADDISON.

CORRIGENDA TO DR. ADDISON'S PAPER.

P. 281, twelfth line from bottom, for "coats of the stomach"! read *coats of the new vessels.*

P. 282, fourteen lines from bottom, for "a fragile globular form or fibrous tissue," read *a fragile globular for a fibrous tissue.* Twelfth line from bottom, for "preliminary to granulation," read *preliminary* TO HEALING *by granulation.* Ninth line from bottom, for "extreme"! read *external.*

P. 283, between "local poisoning blood poisoning" insert *and.*

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* The proof was corrected by the Secretary of the Society, to whom as usual it was sent.—Ed. 'M. M. J.'

Immersion Objectives: their Aperture.

To the Editor of the 'Monthly Microscopical Journal.'

Regent Street, *June* 21, 1872.

Sir,—A contributor to the 'M. M. J.' of May, 1872, who seems to me to have an unworthy habit of introducing personalities, has picked up at second hand, without acknowledgment, a criticism of Dr. Royston-Pigott's,* which I should not care to notice, but that I am reminded by Mr. Jabez Hogg that I omitted to say a few words of explanation in answer to Dr. Royston-Pigott.

In October, 1868, I addressed a communication to the Royal Microscopical Society with special reference to the subject of immersion objectives. I ventured to suggest that "the limiting angle of refraction in water being about 48°, it follows that whatever is the degree of obliquity in the incident light on the object, the immersion objective never has to do with rays of greater obliquity than 48°." The "limiting angle" referred to is evidently the limiting angle of transmission between water and air, therefore I was referring to the condition of the rays from the dry object at their incidence on the immersion objective. That I did not say this specifically was doubtless the cause of Dr. Royston-Pigott's criticism. I may add that, save by Dr. Royston-Pigott and the contributor above referred to, I am not personally aware that anyone interested in the subject found a difficulty in interpreting my observation.

Dr. Royston-Pigott has not upon all occasions expressed himself with that accuracy which is desirable. For example, in 'M. M. J.' No. XIX.,† speaking of the rays emanating from a luminous point in balsam, he says that "every ray . . . . up to 62° 57′ will be able to reach the facet lens of the objective *vid water*." But his own diagram,‡ however inaccurate it may be as a representation of an immersion front designed to transmit very oblique rays, is accurate as against this statement of his, for he exhibits several rays less oblique than 62° 57′ in the balsam which emerge into the water film and are there lost; they certainly are not drawn as "reaching the facet lens of the objective."

Again, in the same number of the 'M. M. J.,' p. 138, Dr. Royston-Pigott says: "*Whenever amongst several parallel plates the ray is examined, its direction in two identically refractive media is always found to be the same* [sic] *after any number of intermediate refractions.*"

In the standard works on Optics quoted by Dr. Royston-Pigott, namely, Griffin, Parkinson, &c., the direction of these rays is not described as "the same," but as "parallel." If I am not mistaken, "the same" means one thing, and "parallel" means another.

But these are only slight blemishes in these two interesting papers on "Immersion Lenses," which I should not have noticed but that I am advised that I am bound, if possible, to exhibit a weak point in my critic's own positions if only to show that I have read his attack on myself.

* 'M. M. J.,' No. xxvi., Feb., 1871, p. 71.
† July, 1870, p. 23.
‡ 'M. M. J.,' No. xxi., Sept., 1870, facing page 134.

I think Dr. Pigott deserves the thanks of the Society for the arduous labour he went through in compiling his ready reckoner for angles of refraction.

I am, Sir, yours, &c.,
JOHN MAYALL, jun.

PROCEEDINGS OF SOCIETIES.*

ROYAL MICROSCOPICAL SOCIETY.

KING'S COLLEGE, *June* 5, 1872.

Charles Brooke, Esq., F.R.S., V.P., in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations to the Library and Cabinet of the Society was read, and a vote of thanks passed to the respective donors. Among the recent donations there was a considerable collection of palates of Mollusca, accompanied by some beautiful drawings, sent by Mr. Mapleson, of Victoria, and a descriptive paper and a number of shells had now been received. Mr. McIntire had kindly undertaken to look over the objects, and see if the names were correct. The Secretary said the object last sent was a valuable addition, and he should propose a vote of thanks to Mr. Mapleson, which was unanimously accorded.

The Secretary also announced another present from Dr. Blake, of San Francisco, consisting of specimens of Algæ collected from hot springs in the States of California and Nevada. The Secretary read a letter from Dr. Blake, describing the conditions under which the specimens had been obtained. Mr. Roper, who had paid great attention to the subject, had kindly undertaken to examine the collection. A vote of thanks was then cordially given to Dr. Blake.

A paper was read from Mr. Roberts, of Rock Ferry, Cheshire, "On the Use of the Pentagraph in Connection with the Microscope."

A vote of thanks was passed to Mr. Roberts.

A paper from Mr. Cubitt, entitled "Remarks on the Homological Position of the Members constituting the Thecated Section of the Class *Rotatoria*," was taken as read.

A vote of thanks was then given to Mr. Cubitt.

Mr. Slack said, "At the last meeting of the Society Mr. Wenham read a paper on a new mode of illumination, which it was not possible to discuss properly, as no Fellow had had the opportunity of examining the plan. Since the paper was read, however, he had (and he believed Mr. McIntire also) examined the instrument. Mr. Wenham had contrived to enable observers to do with greater facility what was to be done, as he had previously explained, with a certain amount of

* Secretaries of Societies will greatly oblige us by writing their report legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—ED. 'M. M. J.'

difficulty and trouble, *viz.* by using the parabola and truncated lens. He (Mr. S.) had tried the new plan on several objects, and he had succeeded in obtaining some very striking appearances with butterfly scales lying flat on the glass and not on the cover. He had been able to get a beautiful definition of several diatoms. In one of the insect scales he noticed a striking exhibition of a complicated fracture running in a zigzag line. The fracture ran exactly between the spherical dots, which is the course cracks always take in diatoms. He could find not that any dot had been broken through either on the upper or lower surface. This mode of illumination did not, in his hands, succeed when the light passed through objects of considerable thickness, and possessing high refractive and reflexive powers. Such objects gave rise to internal reflexion, and a considerable amount of distortion ensued. He thought the instrument, nevertheless, to be a very valuable one, though it might not be applicable universally, for it gave information which could be obtained in no other way."

Mr. McIntire said he had tried the instrument (Mr. Wenham's illuminator) with considerable success and satisfaction. It could hardly be expected that the best effects should be obtained at the first trial, yet those he had obtained were very good, and practice had enabled him to acquire further skill in getting beautiful results. He had resolved, without much difficulty, *N. cuspidata*, *N. rhomboides*, *Surirella gemma*, and the Podura scales, all on a field more or less dark. The markings of the Podura scales came out with a distinctness which left nothing to be desired—a convincing proof of the accuracy of Mr. Beck's view as to their structure. The delicate cross-strise on the test-scale which gave rise to Dr. Pigott's beads also occasionally came clearly in sight. A great deal of care was necessary in adjusting the object-glass and manipulating the illumination in order to ensure the best effects and to reduce the flare caused by too much light. With such objects as *Arachnoidiscus* and *Heliopelta* this was especially the case. It would be found also that objects mounted in balsam and illuminated by this apparatus could only be examined satisfactorily with dry lenses. He was altogether much pleased with the invention, and considered it an important addition to the limited resources at present at the disposal of microscopists in using high powers, whether for display or investigation.

Mr. Wenham said he wished to make some remarks relative to objects having bulk. These ought to have some extent of surface in contact with the glass. If in examining objects such as *Gorgonia* any means could be applied for increasing the surface of contact, the object would be displayed beautifully, but taken alone on account of its irregularities there must be only a few minute points in contact with the glass which abstract light partially, and in a way that might give false appearances. But if a fluid be introduced beneath the object, the light would be generally admitted and diffused throughout its structure. He would recommend that experiments be tried with hairs of animals or insects; for instance, a mouse's hair, which is a difficult object to show up well. If this object be laid on a total reflecting surface alone, without any intimate contact therewith, it

would be invisible, but if it be wetted, then luminosity occurs; the water serving as a channel for conveying light into the object.

One preparer of objects had offered to mount diatoms and scales on the slides specially for this method of illumination if required.

Mr. Richards brought before the notice of the Society an apparatus, founded upon Mr. Stephenson's plan, for focussing under water. The difference between his own and Mr. Stephenson's apparatus was this: In the latter each protecting cap must be made to suit every size of object-glass, while in the former (Mr. Richards'), by means of three concentric screws, the nozzle of the cap can be attached to an object-glass of any diameter, intermediate or the smallest. In using the instrument for salt water the tube can be made of ebonite or brass, varnished.

This apparatus is more particularly adapted to the American arrangement brought before the notice of the Fellows in the January number of the Journal.

The Secretary announced that Mr. Ward had brought over two object-glasses, the latest production of Mr. Wales, the well-known American maker, and the Fellows spent a considerable time in the examination of these glasses.

The meeting was then adjourned until the first Wednesday in October.

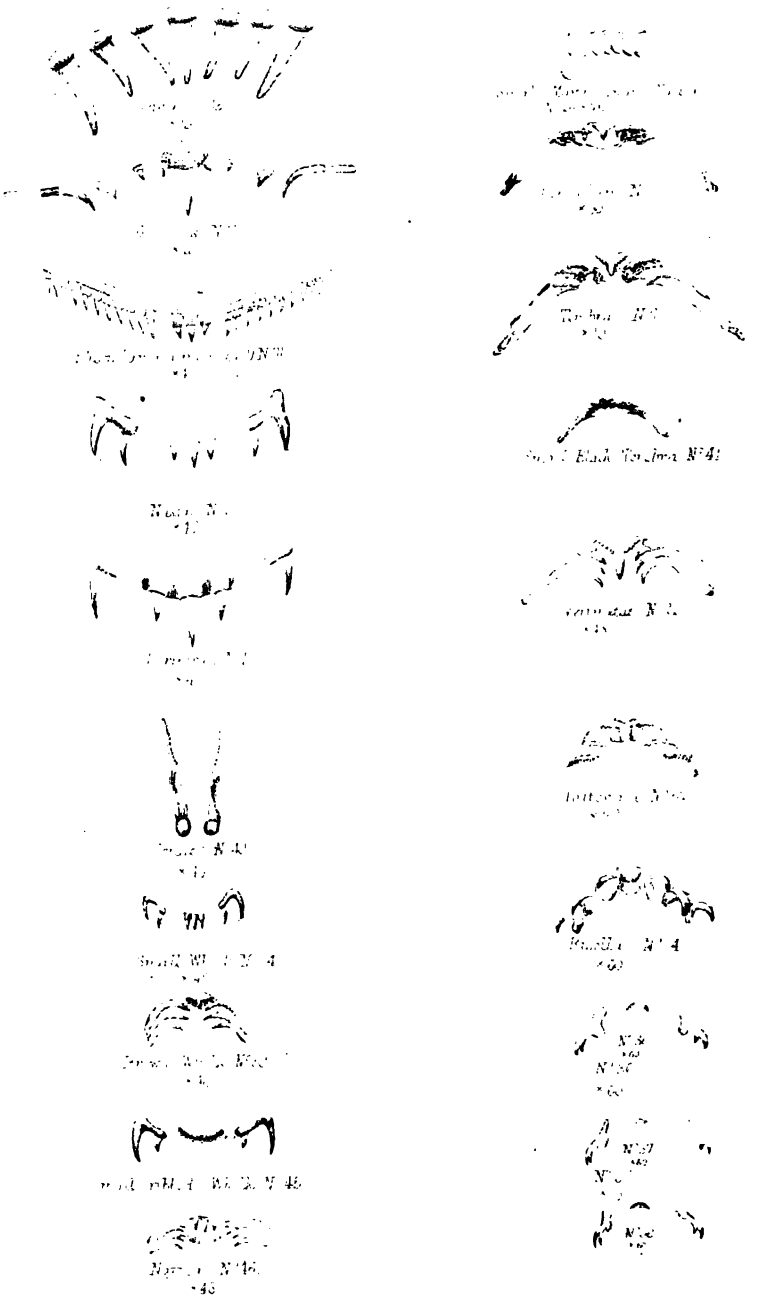
During the month of August the Library and Museum will be closed.

Donations to the Library and Cabinet from May 1st to June 5th, 1872:—

	From
Land and Water. Weekly	*The Editor.*
Nature. Weekly	*Ditto.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal. Weekly	*Society.*
Quarterly Journal of the Geological Society, No. 110	*Ditto.*
Journal of the Linnean Society, No. 67	*Ditto.*
Proceedings of the Royal Society of Tasmania, 1870	*Ditto.*
The Lens, No. 2	*Editor.*
The English Flora, Vol. V. Part 1st comprising the Mosses, Hepaticæ, Lichens, Characeæ, and Algæ. Edited by W. J. Hooker, 1833	*W. W. Reeves.*
Conspectus of the Families and Genera of the Diatomaceæ, Parts 1 and 2. By Professor H. L. Smith	*Author.*
Notes on New and Rarer Species of Diatomaceæ of the U. S. Seaboard. By F. W. Lewis, M.D.	*Prof. H. L. Smith.*
On Extreme and Exceptional Variations of Diatoms in some White Mountain Localities, &c. By F. W. Lewis, M.D.	*Ditto.*
On some New and Singular Intermediate Forms of Diatomaceæ. By F. W. Lewis, M.D.	*Ditto.*
Bulletin de la Société Botanique de France. July, 1871	*Society.*
A slide of Foraminifera from the River Dee	*Thos. Shepheard, Esq.*
Fifteen Slides of Starch Granules	*J. Needham, Esq.*

John Emanuel Winspear, Esq., was elected a Fellow of the Society.

WALTER W. REEVES,
Assist.-Secretary.



Mollusean Palates.

Reduced to $\frac{1}{2}$ of nature / Drawing

THE

MONTHLY MICROSCOPICAL JOURNAL.

AUGUST 1, 1872.

I.—*Notes on Victorian Mollusca and their Palates.*

By CHARLES M. MAPLESTONE.*

(*Taken as read before the* ROYAL MICROSCOPICAL SOCIETY, *June* 5, 1872.)

PLATES XXV., XXVI., AND XXVII.

OCTOPOPIDÆ. *Octopus*, No. 18.—Not common; seems identical with *Oct. vulgaris*.

Palate, formula, 3.1.3. The teeth do not agree with those drawn in Mr. Hogg's paper, 'M. M. J.,' March, 1868. The central tooth has two lateral denticles. 1st lateral is much smaller. The 3rd is not so large. The bases of the teeth are also different.

SEPIADÆ. *Sepia*, No. 43.—Common. Shell and animal seem identical with *Sepia officinalis*.

Palate, 3.1.3. The teeth are all very much alike. The 3rd lateral wants the expanded base which Mr. Hogg gives it, and in

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* The subjoined letter accompanied this paper:—

No. 4, JOSEPHINE TERRACE, ALBERT ST., EAST MELBOURNE,
February 20, 1872.

SIR,—Having made a collection of the palates of Mollusca while resident on the sea-side of our harbour, and having been in correspondence with Mr. S. J. McIntire on the subject, at his suggestion I beg to forward you by this mail (as a registered packet) drawings of the same, together with some notes on them, that may be of interest, and deemed suitable for the pages of the 'Monthly Microscopical Journal.' The drawings are a first attempt *with a pen*, and I trust they will be found capable of reproduction without much difficulty.

I have made the notes as concise as possible, perhaps too much so, and have arranged them according to the Families, as far as I am able. I have throughout retained the numbers that I gave them in the order of collection, as my slides and shells are so numbered; but of course if they are inserted in the Journal it will be better to substitute those of the figures of the plates in the order in which they may appear. I will forward the shells to you, and will advise you of the means by which I do so before closing this, hoping you will be enabled to place them in the hands of some one who will be kind enough to name them correctly, and which names may I ask you to insert in their places, and to send me a list with my numbers added, so that I may with certainty know the proper names.

Although I have not drawn the palates, I send also some fresh-water shells, of which No. 48 is one that I can find no genus in Woodward that I can include it

his figure the well-marked longitudinal muscular bands are not shown. Specimen in cabinet of R. M. S.

Muricidæ. *Triton* (?), No. 35.—Long, mottled, spiral shell. Varices small, irregular. Hab., seaweed below low-water mark. Very rare; only found one or two specimens alive; dead shells common. Operculum horny.

Palate, 3.1.3. Median reflexed, denticulated with large central tooth. 1st lateral, with a slight spinous process above the tooth. Specimen in cabinet of R. M. S.

Fasciolaria (*monilia?*), No. 11.—Animal bright red in colour. Operculum horny, claw-shaped, but large enough to close aperture. Hab., below low-water mark. Scarce.

Palate, 1.1.1. The median having three denticles, the bases of which are finely furrowed. Laterals 11 denticles. It is a remarkably clear and regular palate.

Buccineadæ. *Nassa* (?), No. 9.—This is the common dog whelk, occurring plentifully between tide marks, and is a voracious feeder on dead and living molluscs, crabs, &c. A perfectly-formed shell is rare, the apex appearing to suffer from abrasion. It is variously marked. Some are reticulated or chequered; in others the dark markings form lines, and occasionally all trace of marking is lost. Operculum horny, obovate apex at end.

Palate, 1.1.1. Median, three denticles of equal length; laterals with basal tooth. Specimen in cabinet of R. M. S.

Purpura, No. 17, probably *P. textilosa*.—Hab., near low water.

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in. It is a sinistral shell, and has longitudinal ridges. I trust I am not asking too much of you in this matter; my anxiety to place the palates, &c., before the readers of the Journal in as complete a form as possible must be my excuse.

I enclose with the shells a small packet for Mr. McIntire, which please be kind enough to hand to him.

I am, Sir,
Your most obedient servant,
Chas. M. Maplestone.

Walter W. Reeves, Esq., *Assistant-Secretary, Royal Microscopical Society, King's College, London.*

Mr. Henry Woodward has seen the shells, No. 48, and kindly written the following note about them.

Walter W. Reeves.

British Museum, *July* 5, 1872.

Dear Sir,—I have now the pleasure to return you the little shells. I only this very morning found leisure to compare them, having been very much pressed for time. They are the young of a fresh-water shell named *Physa alicia*, var. Lovell Reeve. In our case the adult attain to nearly four times the size.

Yours very truly,
Henry Woodward.

Walter W. Reeves, Esq.

Very rare where I made collection, but common in other places. Operculum lamellar, horny.

Palate, 1.1.1. Lateral has no basal tooth. Median, three denticles, middle one longest. Specimen in cabinet of R. M. S.

Purpura (?), No. 24.—Small whelk, purple inside. Hab., near low water on serpulæ-covered rocks. Common. Operculum claw-shaped, horny.

Palate, 1.1.1, very like that of *Nassa*, but much smaller. The denticles of the laterals are long and thin. Specimen in cabinet of R. M. S.

Purpura (?), No. 39.—Brown whelk, similar in shape to the last, but the mouth was thin and semitransparent. Shell covered with a dark brown epidermis, which was produced into irregular longitudinal short fringes. The palate is different from *Buccineadæ*. I have no specimen of shell. The only one I met with was transmitted to the late Mr. Hall; it may probably belong to *Muricidæ*.

Palate, 3.1.3. Median reflexed, denticulated. 1st and 2nd laterals slightly denticulated. 3rd smooth.

Purpura (?), No. 45.—Shell transversely ribbed; inner lip callous, extends over large portion of whorl; outer lip thickened. Possibly *Nassa thersites*. Hab., low water. Specimens were found in enclosure for graving dock.

Palate, 1.1.1. Central tooth lunate, with numerous round pointed denticulations. Lateral teeth with a basal tooth and small flat processes on inner side.

Conidæ. *Conus*, No. 40 (*C. punctatus ?*).—Shell with thick dark-brown epidermis. Marked with irregular brown spots. Column plicated at lower portion. Animal pink, with black eyes. Hab., near low water. Only found on two occasions. First a single specimen, from which I failed to obtain the palate. Second, when there was an extra low tide and calm weather, when I obtained several.

Palate, 1.1. Teeth long, serrated near tips. Bases orbicular, very difficult of dissection.

Naticidæ. *Natica*, No. 46.—Hab., sand near cockle bed between tide marks. Very rare. Operculum large, paucispiral, horny; jaws spiny; very large mouth; short foot.

Palate, 3.1.3. Median reflexed; three denticulations. 1st and 2nd laterals, claw-shaped, bifid. 3rd lateral simple.

Note.—On drawing this shell in my diary I find there is another (probably a Naticina) that I had erroneously considered but a variety of this.

Natica (?), No. 26.—Small striped marine snail. Abundant.

Hab., sand near high-water mark. Has very large mouth; apparently feeds on animal matter contained in the sand, which abounds with live foraminifera, as I found the intestines full of it. Operculum horny, subspiral; shell snail-shaped, one or two dark brown stripes. Palate consisting of rows of simple sickle-shaped teeth, altogether different from any marine form I have seen. Specimen in cabinet of R. M. S.

CERITHIADÆ. *Cerithium*, No. 7.—Hab., sand, generally partially imbedded, between tide marks, nearer high than low water. Rare. Operculum horny, multispiral.

Palate, 3.1.3. Median reflexed; central denticulation much the longest. 1st lateral with slightly rounded claw, edges partially reflexed and finely denticulated; 2nd and 3rd, edges reflexed, denticulated. Specimen in cabinet of R. M. S.

Cerithium (?), No. 10.—Shell granulated. Hab., stream on sand, nearer high-water mark than low. Operculum horny, multispiral.

Palate, 3.1.3. Median reflexed. Five denticulations, central longest; base articulated. Laterals, edges reflexed, denticulated. Specimen in cabinet of R. M. S.

TURRITELLIDÆ. *Turritella* (?), No. 41.—This is smaller than the last described. It has not a canal; peristome entire, longitudinally furrowed. Hab., sand between tide marks.

Palate, 3.1.3. Median reflexed; three denticulations, not articulated. Laterals reflexed, denticulated.

Vermetus, No. 22.—Only found one specimen, in the root of a large mass of kelp, but have lost it, so cannot describe it at all.

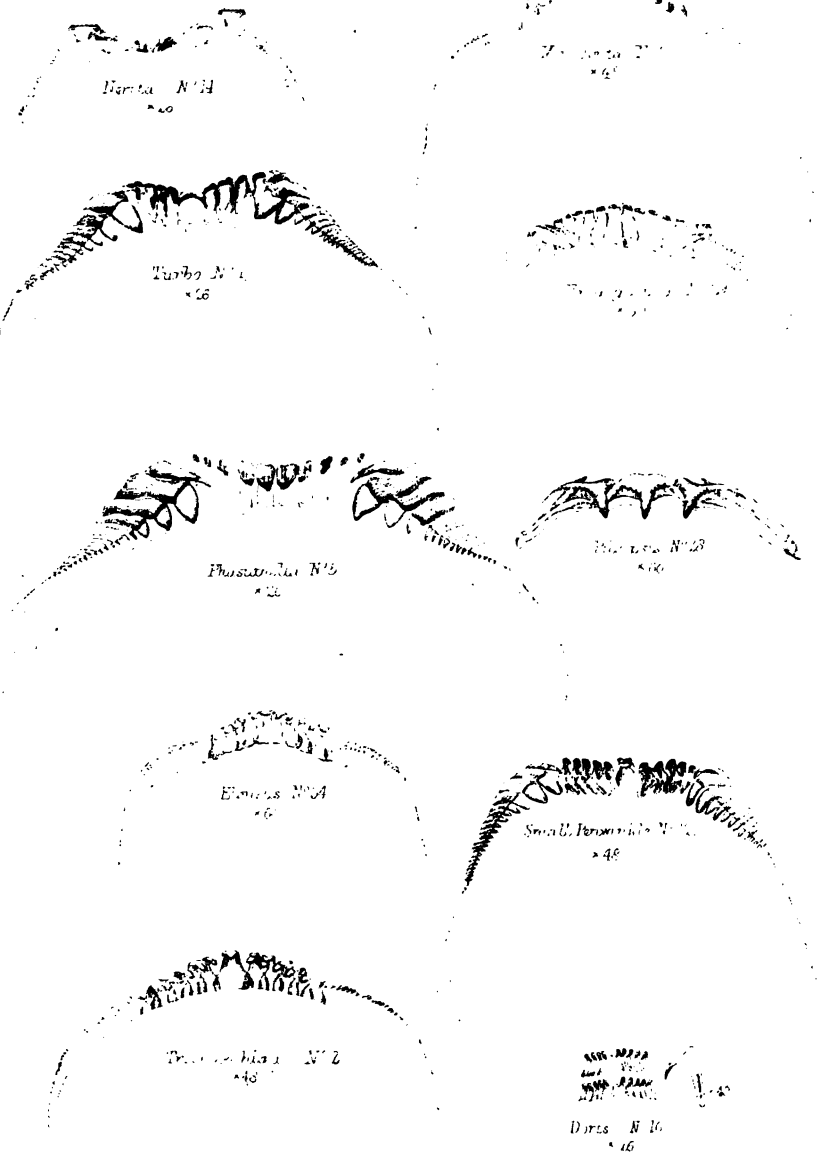
Palate, 3.1.3. Median reflexed, denticulated, central one very long. 1st lateral, long tooth at end, lower edge denticulated. 2nd lateral tooth-shaped, slightly denticulated. 3rd similar, but not denticulated.

LITTORINIDÆ. *Littorina*, No. 52.—Shell smooth, generally corroded; lip slightly everted. Hab., between tide marks at Schnapper Point; operculum horny, paucispiral.

Palate, 3.1.3. Median reflexed, central denticulation truncated. 1st and 2nd laterals, upper edges reflexed and denticulated; 3rd has much longer comb-like denticulations.

Risella, No. 4.—Shell with irregular keels more or less developed. Hab., between tide marks, nearer high than low water. This is the common dog periwinkle, so called from not being edible: it is carnivorous. Operculum paucispiral.

Palate, 3.1.3, very long. Median reflexed, with broad, central denticulations. Laterals reflexed. Specimen in cabinet of R. M. S.



Molluscan Palates

Retrieved from the Natural History Archive

Risella (?), Nos. 36, 37, and 38. *Rissoa* (?).—Hab., low water, on seaweed on rocks; only found on one occasion. They may be merely varieties, but I found the following distinctions:—No. 36, brown, lip toothed, pink inside. No. 37, larger than Nos. 36 and 38, brown marked, lip toothed, purple inside. No. 38, smooth lip; thin, semitransparent, showing brown markings through.

Palates, 1.1.1, very similar, but I think the figures sufficiently indicate, in conjunction with the shells, a difference of species. Median crescentic; laterals with two denticulations. In No. 36 the base is much dilated; in No. 37, less so, and brought almost to a point at the back; and in No. 38, slightly dilated and more like No. 36. Specimens of Nos. 36 and 37 in cabinet of R. M. S.

Neritidæ. *Nerita*, No. 14.—Hab., crevices of rocks near low water; rare; dark bluish black in colour; operculum calcareous, paucispiral inside callous, and black outside, articulated. Posterior end of animal much shortened; appears to absorb internal whorls.

Palate, ∞.3.1.3.∞. Median small, toothed. 1st lateral broad, subtrapezoidal, upper edge reflexed and denticulated near outer portion. 2nd, very small, almost hidden. 3rd, with large prominent head or cap, nearly semicircular, edge crenated. Uncini numerous, reflexed, toothed; outer ones serrated.

Turbinidæ. *Turbo* (*Lunella?*) *undulata*, No. 1.—Two to three inches in diameter, deeply umbilicated. Operculum calcareous, subspiral inside, callous outside. Hab., seaweed on rocks at low water. Called "Warrener." Edible. Common often bears Pileopsis, No. 28, parasitic.

Palate very large and beautiful. ∞.5.1.5.∞. Median reflexed slightly, smooth with process above. Five laterals reflexed, crenated, outer edges of bases expanded, overlapping at sides. 1st unc. small, claw-shaped tooth; 2nd with large, smooth tooth, sides crenated; the others gradually diminishing in size, gradually changing into a ligulate form, with ends slightly reflexed. Specimen in cabinet of R. M. S.

Phasianella, No. 5.—Hab., low water and below, rarely seen above; live shells rare, dead ones plentiful in some parts. This shell is most beautifully marked with crescentic-shaped patches of various shades of pink, red, purple, brown, and yellow, forming different patterns; the crescents sometimes running together and forming irregularly-shaped markings; in others more regular, and in longitudinal rows, generally close together, but in varying distances and sizes, down to very fine longitudinal lines in which the crescents are very small. Occasionally marked as in the drawing in Woodward's 'Manual of Mollusca,' but rarely. It is very rarely found perfect; even when alive the lip is very brittle.

Palate, ∞.5.1.5.∞. Median doubtful, but think I have occasionally seen it, very small; laterals reflexed plain. 1st Unc. claw-shaped rugose. 2nd, large smooth tooth, sides rugose, others similar, diminishing in size and changing to long-curved serrated teeth, very numerous. Slide in cabinet of R. M. S.

Elencus, No. 34.—The base is not so flat as the figure in Woodward. Hab., low water; very rare; green pearly lustre inside. Operculum horny, multispiral.

Palate, ∞.5.1.8.∞. Median reflexed, denticulated. Laterals with ovate teeth, denticulated; bases overlapping at sides. Uncini hooked, very numerous. Specimen in cabinet of R. M. S.

Trochocochlea, No. 2.—Hab., rocks between tide marks. The common periwinkle, gathered with No. 8, as the same. Shell with rugose longitudinal furrows, marked in larger specimens similar to *T. zebra*, which this may be. Inner lip with a slight tooth. Operculum horny, concentric.

Palate, ∞.5.1.5.∞. Median reflexed; laterals, upper portion reflexed, ligulate, denticulated; bases broad, overlapping. Uncini hooked, serrated; very numerous. Specimen in cabinet of R.M.S.

Monodonta (?), No. 8.—Hab., rock between tide marks. The difference between this and Trochocochlea is in being smooth, no furrows, and a rather flatter shell. Foot of animal is much longer. Operculum concentric, horny. Inner lip with a slight tooth. Sometimes bears Limpet, No. 30, as a parasite.

Palate, ∞.5.1.5.∞. Median longer than Trochocochlea; laterals have a smooth central tooth; bases overlapping at sides. Unc. hooked, serrated; very numerous. Specimen in cabinet of R. M. S.

Monodonta (?), No. 32.—Small flat periwinkle with calcareous subspiral operculum. Hab., rocks and seaweed below low water; rare; only found on one occasion. Shell flatter than the previous two, with wavy, transverse, slightly-elevated ridges.

Palate, ∞.5.1.5.∞. Similar in character to Turbo and Phasianella; median inflexed at top and sides; laterals reflexed, with very broad, wrinkled, overlapping bases. Unc. with large teeth gradually diminishing, and changing into hooked, serrated teeth; very numerous. Specimen in cabinet of R. M. S.

Monodonta, No. 33. (Monodonta?).—Small periwinkle, rough, longitudinally granulated; rare; only found on one occasion. Operculum horny, concentric. Hab., seaweed on rocks below low water.

Palate small, ∞.5.1.5.∞. Median reflexed, denticulated, with very broad, expanded base; laterals the same, with overlapping

bases. Uncini hooked, serrated; very numerous. Specimen in cabinet of R. M. S.

HALIOTIDÆ. *Haliotis*, No. 15.—Hab., rocks at low water; only found on one occasion, but more plentiful in other places. Specimens were five and six inches across; animal weighed 14 oz. without shell.

Palate very large and fine, ∞.5.1.5.∞. Median small, reflexed. 1st lateral subtrapezoidal; upper edge reflexed. 2nd, reflexed. 3rd, with large tooth, very rugose. 4th and 5th, similar but smaller. Uncini hooked, finely serrated; numerous. I have reckoned the 3rd, 4th, and 5th laterals to be such as they are, very different from the Uncini. Specimen in cabinet of R. M. S.

FISSURELLIDÆ. *Emarginula*, No. 29 (Hemitoma ?).—Liberty cap limpet. Hab., rocks near low water; not common; very delicate, ribbed, white shell.

Palate, ∞.5.1.5.∞. Median and laterals broad, slightly reflexed, crenated; basis overlapping to a small extent. 1st Unc. very large, with large irregular tooth; others small, hooked; numerous.

CALYPTREADÆ. *Pileopsis*, No. 28.—Hab., parasitic on Turbo, never found by me in any other position; it sometimes bears small ones parasitic on itself; it nurses its young, which have spiral shells, and wears away the shell of its host slightly.

Palate, 3.1.3. Median reflexed, denticulated, middle one very long. 1st lateral denticulated, one of which is produced into a long tooth. 2nd and 3rd partially reflexed and denticulated; 2nd but slightly. Specimen in cabinet of R. M. S.

PATELLIDÆ. *Patella*, No. 3.—Large striped limpet, one to two inches across, occasionally larger. Hab., rocks near low water. Shell pearly inside; radiating stripes of dark colouring.

Palate very long, from five to six inches even in the smaller ones. 3.–.3. 1st lateral reflexed, apparently articulated. 2nd lateral reflexed, bifid at extremity. 3rd indistinct, probably two small teeth. Specimen in cabinet of R. M. S.

Patella, No. 21.—Small deep limpet, not common. Hab., near low water.

Palate, 3.–.3. 1st lateral, long tooth at top, with a basal tooth. 2nd ligulate. 3rd very small, and apparently composed of two very small teeth placed close to the base of the 2nd. Specimen in cabinet of R. M. S.

Patella, No. 25 (longicosta ?).—Not common; porcellaneous inside; shell ribbed, but not quite so prominent as Woodward's drawing of *P. longicosta*. Hab., near low water.

Palate, 4..4. 1st lateral, with basal bifid tooth. 2nd ligulate very slightly reflexed. 3rd and 4th ligulate reflexed.

Note.—This is the only palate of the limpets which clearly shows four laterals. Specimen in cabinet of R. M. S.

Patella, No. 30.—Rare; very small deep limpet; found by me only parasitic on No. 8; smooth, with few brown markings near edge.

Palate, 3.–.3. 1st lateral, with basal bifid tooth. 2nd and 3rd ligulate; indistinctly divided. Specimen in cabinet of R. M. S.

Patella (*Acmæa ?*), No. 20.—Small flat limpet, not common; dark brown markings.

Palate, 3.3. 1st lateral, with basal bifid tooth. 2nd lateral ligulate, with a basal tooth. 3rd ligulate. These two are better seen in the side view. Specimen in cabinet of R. M. S.

Siphonaria (*aspera ?*), No. 27.—Nutmeg limpet. Hab., near low water; rare; but more common in other places.

Palate, ∞.1.∞. Median very small, laterals hooked, with double points. Side view gives better idea of the form than front view. Specimen in cabinet of R. M. S.

Chitonidæ. *Chiton spinosus*, or *Acanthopleura* (?), No. 6.—One to four inches long; common; edges covered with spines. Hab., rocks near low water.

Palate, 3.4.1.4.3. Median reflexed. 1st lateral slightly reflexed. 2nd lateral long, with large trifid head. 3rd, ligulate, slightly reflexed. 4th, long, claw-shaped. Three outer ones (Uncini ?) flat, very slightly reflexed. Specimen in cabinet of R. M. S.

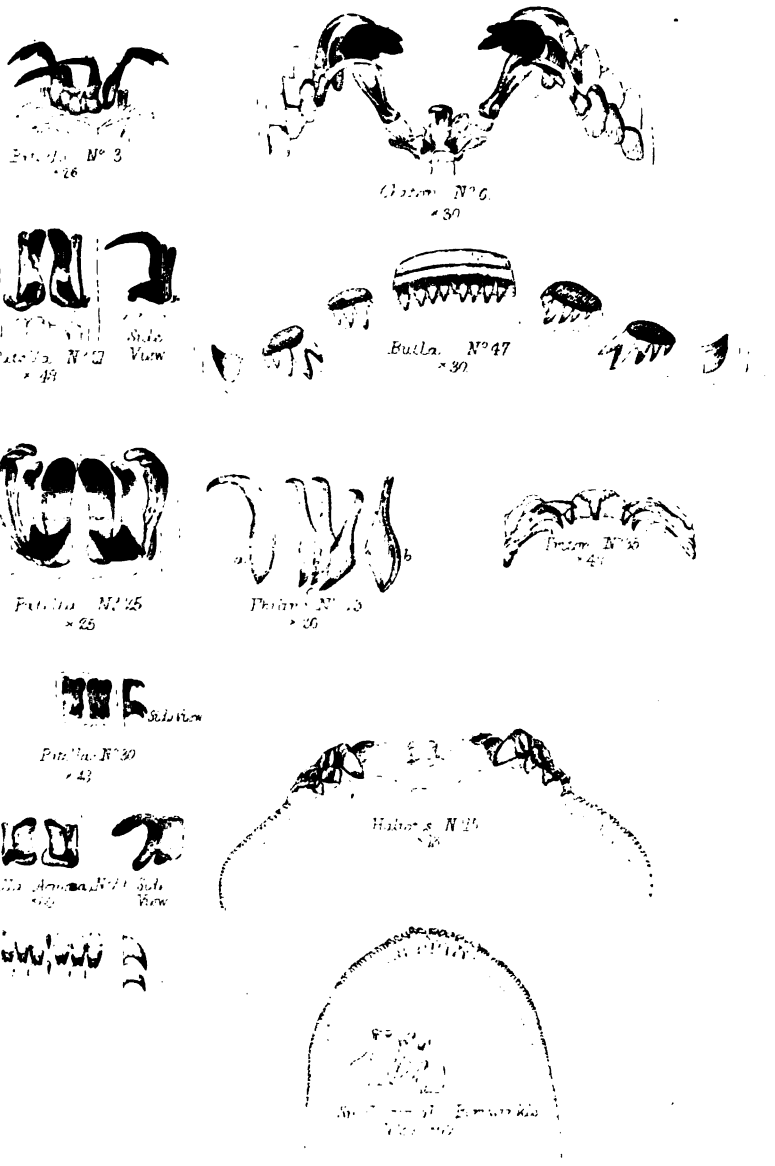
Bullidæ. *Bulla* (*oblonga ?*), No. 47.—Hab., below low-water mark; only found alive in enclosure for graving dock; has three chitinous gizzard teeth.

Palate, 3.1.3, very large. Median broad, with a small central denticulation, and eight large lateral ones. 1st lateral, dark teeth, with light large denticulations. 2nd similar, but first denticle is bifid. 3rd triangular, slightly reflexed.

Philine (*aperta ?*), No. 23.—Hab., in sand below low water; only found an occasional solitary specimen, except in enclosure for graving dock, where large numbers of fine ones were exposed. Two to four inches long, three calcareous gizzard teeth.

Palate, 1.1. Teeth claw-shaped, denticulated on inner margin. Plate XXVII., Fig. 23, *a* shows side view; *b*, front view; *c*, view from intermediate point. Specimen in cabinet of R. M. S.

Doridæ. *Doris* (*?*), No. 16.—Hab., low water on rocks; very rare. Limaciform; tentacles two; simple truncated narrow cones.



Mollusca palates

W. West & Co. imp.

Gills exuding from end of back. Pink in colour, with dark blue and red spots; only found two specimens, three and four inches long respectively.

Palate, ∞.1.∞, broad and short. Median very small, straight; laterals hooked; shape shown in side view.

Doris, No. 19.—Hab., low water on rocks; only found two or three specimens, one to two inches long; shape similar to *D. Johnstoni*; light orange colour.

Palate, a muscular band with a few irregularly-shaped concretions, can hardly be called teeth, distributed thereon without any apparent order.

The accompanying drawings of the palates of Mollusca are of those that I obtained (with one exception, Littorina) at Williamstown, at the head of Port Phillip Bay, some thirty miles from the open ocean.

They are the results of about three years' collecting, and, I believe, include all the species of palate-bearing Mollusca to be found in that particular locality (the statements as to rarity or otherwise are only applicable to it), but they are far from representing those of coast generally, as I have had no opportunity of collecting live shells elsewhere.

All the drawings are made direct from the microscope with the camera lucida. The nature of some of the palates of the smaller Turbinidæ is such that it is almost impossible, in mounting them in fluid, to prevent the edges from curling up, and I found great difficulty in seeing the second and third laterals of most of the limpets; they seem to be rudimentary, and hardly worthy the name of teeth.

The palates of the Turbinidæ are all very beautiful; some of them gorgeous when viewed with the polariscope and selenite plate, and they require a more skilful pencil than mine to show their beauty.

There are some Strombidæ, Volutidæ, Cypræidæ, and a beautiful "Paper Nautilus" (Argonauta) on our coasts, that I have not yet met with alive, but it will be seen that at the spot where my gatherings were made most of the families of Marine Gasteropoda were represented.

I have found two fresh-water shells (the most common is a Physa), and two or three others were forwarded to me by a friend, but a severe illness prevented me from mounting the palates, which were in course of preparation, for when I next was enabled to look at them they were destroyed. All their palates are of similar character to those of the Helicidæ.

I have seen only one land shell, that of a black snail (in our museum), but have not found any myself. The only localities in

will at once make manifest; for although they both exhibit a palpable bilaterality in the corona, they differ essentially in their respective characters, for we see in L. *socialis* that both axes of the disk are equal, while in L. *albo-flavicans* the horizontal axis is just double that of the vertical. Dujardin denies the propriety of their separation, but Ehrenberg considers the *absence* of a divesting theca, and the attachment of the ova by a filament to the body, to be sufficient grounds for separation. Now the very assumption of the filament attaching the ova to the body is the best evidence we could desire, to show that the group is invested in a gelatinous matrix. The *appearance* of the filament is simply the microscopical rendering of the integument of the theca itself surrounding the ova. Ehrenberg's drawing speaks for itself.

Alimentary System.

However dissimilar the organization of the setiferous trochus (*a*) of the one Family may be with the ciliated trochus of the other, they are constituted to perform the same functions in the animal's economy as the first instrument employed in securing and directing prey to the mouth. In Stephanoceros *Eichhornii* and S. *Horatii* the corona is invested in one continuous series of setæ all along the margin of the trochus, but in Floscularia the lobes only are furnished with these prehensile organs, with the one exception of Floscularia *campanulata*, where they occur also in one continuous and unbroken series. Plate XXIV., Fig. 1.

In the first Family the alimentary particles, when captured by the trochal setæ, are directed by their intermittent action into the *calix* (*b*), in which the ciliated *cingulum* (*a'*) is situated. I employ the term calix to the funnel that precedes the alimentary canal, not in its botanical significance of the leaf-like organs that surround and support the corolla, as Professor Allman has applied it to the diaphanous membrane surrounding the lower portions of the tentacles of the Polyzoa, but in its more general signification of a *cup*. The cingulum creates a vortex within the calix, in which the particles, alimentary or otherwise, become involved, passing by its influence upwards on the hæmal side, and downwards on the opposite, where they become subjected to the scrutiny of the tongue-like little organ the *lingula* (*c*), that exercises such a marvellous faculty in selecting from the heterogeneous mass those portions which are specially qualified for alimentary purposes, and in rejecting others as refuse; and situated on the neural side just above the ganglion (*d*), within the calix, its position and functions are identical with the *epistome* of the Polyzoa.

The calix communicates, by means of an œsophagus, with a capacious chamber, the *cardiacus* (*e*), and it is seen that alimentary

which they are likely to be met with here are the heads of gullies in the highest ranges, where the foliage of the fern-trees, &c., is dense enough to modify the summer heats.

My first impulse for research among the Mollusca was given by the late Mr. Hall asking me to obtain palates for him. A subsequent residence at the sea-side gave me the opportunity, which I availed myself of, and in my eagerness to make up a collection as soon as possible (my time for such work being limited) I regret I omitted to take any notes or make any drawings of the animals themselves. I am therefore unable to supply a great deficiency, as I cannot trust my memory for accuracy on this point. The shells I have drawn for myself in my diary, but have not forwarded copies, not deeming them to come under the head of "microscopical objects," though in absence of the shells themselves they would add greatly to the "natural history" value of the collection.

Some of the palates were kindly mounted by Mr. McIntire, and are in the Society's cabinet.

II.—*The Nerves of Capillary Vessels and their probable Action in Health and Disease.* By Dr. Lionel S. Beale, F.R.S., Fellow of the Royal College of Physicians; Physician to King's College Hospital.

(*Conclusion of a Paper commenced in the January Number of the Journal.*)

Part II.—Physiological and Pathological.

Considering the arrangement of nerve fibres in the organs of sense, no one would be likely to conclude that those distributed to capillary vessels were in any way concerned in special sensation. We know that many of the ultimate parts of organs which take part in sensation—which indeed constitute the active portion of the sensitive apparatus, are destitute of vessels altogether. These and other considerations render it certain that the nerve fibres of the capillaries are not nerves of special sense.

The capillaries are not provided with muscular fibres, nor is there any contractile tissue to be demonstrated in connection with them. Although it has been confidently asserted that the capillary walls consist of *protoplasm* (!), anyone who will be at the pains of examining actual capillaries, as, for example, those of the pia mater, will soon be convinced that this view is a mistake. It is, in fact, one of those conjectural anatomical observations which are in favour in these days. An hypothesis was advanced to account for the passage of blood corpuscles through the capillary walls, but before this hypothesis could be received it was necessary that some one should demonstrate the *protoplasmic character of the capillary walls.* Protoplasm was therefore conjecturally discovered in the capillaries by more than one microscopical philosopher.

The walls of capillaries may be well studied in the capillary vessels of the ciliary processes of the eye. The *membrane* of which they consist can be seen, and the bioplasts connected with it demonstrated in considerable number, and without difficulty. The membrane is transparent, slightly fibrous in the vessels of old animals, highly elastic, but destitute of any structure that can be regarded as *contractile.* The capillaries torn from the grey matter of the brain and those of the choroid plexus are also favourable for investigation. We may therefore, I think, dismiss the idea that these nerve fibres are motor, or are in any way concerned directly,—that is, by any influence exerted by them on the walls of the capillaries, in reducing the calibre of these minute vessels. If protoplasm could only be proved to exist in the walls of the capillaries, the presence and action of the nerve fibres would be accounted for. They would cause the protoplasmic substance to contract. But as the capillary

parietes are not composed of protoplasm, but consist of a passive permeable membrane, such an explanation is inadmissible.

Next, I must ask the reader to consider with me whether it is probable that these nerves are connected with nutritive or secretory operations. Now, although many high authorities still hold to the opinion that nerves do act directly upon the nutritive process, many considerations render it at least doubtful whether the action of cells is directly influenced by nerve fibres in any case. Nutrition and growth are carried on at a rapid rate in living structures which are destitute of nerves, and at every period of life. In disease the most active nutritive changes occur in tissues which appear to be wanting in nerve fibres. For example, the formation of pus from epithelium and the formation of tubercle, can hardly be attributed to the influence exerted by nerves, seeing that the phenomena occur in parts to which nerve fibres do not reach. Nutrition and growth are most active in all living beings at a period of development anterior to that when nerves are formed. Secretion is very active in glands which receive a limited supply of nerves, and in those parts of glands to which very few nerves are distributed. Contrast, for example, the multitudes of fine nerve fibres distributed to the surface of a sensitive mucous membrane, with the few that can be traced around the uriniferous tube, or followed to the follicles of the sebaceous glands, or to those of the salivary glands. Pflüger's statements on this point have not been confirmed by other observers, and from my own observations I am convinced that if, as Pflüger asserts, nerve fibres are distributed to secreting cells, they are arranged in a manner very different from that represented by him in his drawings, and the character of the fibres is different. In those cases in which nerves can be followed near to secreting cells, it is more probable that their function is afferent as regards the nerve centre governing the nearest vessels, than that they are directly concerned in the actual secerning process. This I believe to be due rather to the properties and powers of the bioplasm of the cell, than to any mysterious, and at this time purely conjectural, influence of nerve force.

What, then, is the probable office of the nerve fibres which are so freely distributed to many of the capillary vessels? Upon many sensitive surfaces the fine nerve fibres running very close to the capillaries are indeed so numerous that we can hardly avoid coming to the conclusion that these very nerve fibres must be concerned in sensation. The capillaries of the mucous membrane of the frog's palate, for example, are surrounded by a complete network of very delicate nerve fibres, which lie immediately beneath the epithelium. In this situation there are no *papillæ*, nor is there any form of special sense organ to which nerves are distributed. By experiment we know that this surface is eminently sensitive, though there is no reason to think that it is concerned in the sense of taste. At

any rate, upon the tongue of the frog are very highly elaborate nerve organs of a special structure, and having a very large number of nerve fibres distributed over a small space quite at the summit of the papilla.* If the frog possesses the sense of taste, the delicate papillæ above referred to are doubtless the organs concerned, and not the general surface of the palate.

But the nerve fibres near the capillary vessels may take part in general sensation. Doubtless it is through the agency of the beautiful little "tactile corpuscles" in the papillæ of the skin that we are able to distinguish those very slight differences of quality in fabrics when the tip of the finger is gently drawn across them. But if every tactile corpuscle were destroyed, we should still be able to *feel*, and the skin of the finger would still be differently acted upon by hot things and cold things. According to the same reasoning, we are justified in supposing that the nerves which I have shown are distributed to the capillaries of voluntary muscle, act as sensitive fibres, and are perhaps concerned in conveying to us the sensation by which we are enabled to form a conception of the exact degree of contraction which has been effected in the muscle under different circumstances. And in certain forms of disease in which there is no loss in the power of contracting the muscle, but in which the mind does not form an accurate idea of the degree of contraction which has been induced, it is probable that these nerve fibres, or the centres with which they are connected, are the particular parts of the nervous system which are involved. The cold feeling, the chill which precedes a cold or an attack of fever, results, I believe, from a change effected in these fibres distributed to the capillaries, caused probably by disturbance in the capillary circulation and the consequent phenomena induced immediately outside the minute vessels.

Few questions are of higher importance than the determination of the relationship of the nerves distributed to the capillaries to those ramifying in such great number on and amongst the muscular fibre cells of the small arteries which divide into branches from which the capillary vessels spring. I can adduce direct anatomical observations in favour of the view which I have been led to accept. In the frog I have succeeded in actually tracing nerve fibres from a ganglion to a nerve trunk, and from the trunk to the capillary vessels; and in the bladder of the frog I have been able to follow fine nerve fibres from the ganglion both to arteries and capillary vessels. It is surely justifiable to infer that a similar disposition exists in the higher vertebrata and in man. But such an inference is almost irresistible if we bear in mind the vast number of ganglia existing in the submucous areolar tissue of the intestinal canal and the course taken by the nerve fibres from these, and the fact of the great alteration frequently taking place in the vascular turgescence

* See 'Phil. Trans.,' 1864.

of the mucous membrane. But there are certain physiological experiments which have a very important bearing upon the question under discussion, and to these I will now direct attention.

Physiological Experiments.

Many years ago, when I possessed two living specimens of the Proteus, which I brought home from the cave at Adelsberg, I often observed the change which instantly took place when a bright ray of light was suddenly thrown upon the vessels of the exposed branchiæ. The little arteries of the gill suddenly contracted, and the entire volume of the vascular tuft was reduced by at least one-third. The circulation through the vessels was instantly retarded, the diameter of the capillaries was sensibly reduced, and the flow of the blood stream distinctly checked. The ray of light, I conclude, acted directly upon sensitive nerve fibres distributed around the capillaries, which carried an impression to the nerve centre, and through the vaso-motor nerves which were connected with this centre the muscular fibre cells of the artery were made to contract.

Another experiment which may be easily tried upon the web of the foot of the living frog is almost as conclusive, although it is open to the objection that the fibres which carry the impression to the nervous centre are not those of the capillary vessels, but the sensitive fibres distributed to the cutaneous surface. It must, however, be remembered that there are no special tactile organs in the part of the web experimented upon, and that the capillary vessels with their nerve fibres lie immediately beneath the thin cuticular covering of the web.

The experiment is performed as follows:—The foot of a young living frog is well arranged for observation and covered with thin glass, so that the circulation in the vessels can be well studied with a quarter of an inch object-glass. The illumination must be good. The tube of the microscope is then lengthened by eight or nine inches. By this proceeding we greatly augment the magnifying power. Next, a small artery is brought well into the field of the microscope, and carefully focussed. While the observer is looking intently at the artery the surface of the web near the spot under examination* is very gently touched with the point of a needle, which should have been already mounted in a handle made of a piece of light wood. Instantly the artery begins to contract, and in a few seconds its cavity is so narrowed that not a single blood corpuscle can pass. The contraction is not quite even, for the outlines of the vessel appear more or less wavy. After a few seconds an undulatory movement of the coats is noticed, and the artery gradually dilates to the same degree as before the irritation of the web.

* The experiment often succeeds when the most distant parts of the web are touched.

The results of these experiments, considered in connection with the facts discovered by careful anatomical investigation, prove, I think, conclusively the function of the fine nerves distributed to the capillary vessels, and indicate the precise manner in which they act. In many tissues and organs in which the circulation is readily disturbed by peripheral irritation the nerve fibres distributed to capillary vessels are exceedingly numerous.

Arrangement of Self-acting Mechanism by which the Supply of Blood to Tissues is regulated.

We are now in a position to consider the probable arrangement of the reflex nerve mechanism by which the flow of blood in the capillaries is regulated, and the equable distribution of nutrient fluid to the tissues outside the capillaries ensured in a state of health. We shall also see how this elaborate mechanism is rendered self-acting, self-regulating, and self-adjusting during the healthy state.

The nutritive operations of man and many vertebrata have to be carried on with comparatively little alteration under *very variable and continually changing external conditions.* A limited range of variation is permitted, but if the limits within which this self-adjusting apparatus acts perfectly, be overstepped in either direction, as not unfrequently happens under the very variable and artificial conditions to which civilized man and domestic animals are exposed, the range of the capacity for self-adjustment is exceeded, and a part of the mechanism is temporarily damaged or actually destroyed. Unfortunately repair can only be imperfectly carried out, and the mechanism restored to its normal state in cases in which the derangement is comparatively slight. So complex is the apparatus and so widely distributed are its interdependent parts that renovation after its destruction is complete is impossible, and I doubt if its re-formation in the adult has ever occurred, or is indeed possible.

In the diagram appended I have put together the facts I have ascertained by anatomical investigation, and have represented the several parts of which the self-regulating mechanism is, I believe, made up. I have also indicated what I believe to be their true relationship.*

* The artery *a* is surrounded by muscular fibre cells which are supposed to be contracted, and the capillaries near the artery over the figure 2 are also represented as they would appear when very little blood was traversing them; while those to the left of the drawing, over figure 1, are as they would appear if distended with blood. It is obvious that in the first case an interval would exist between the nerve fibres and the capillary vessels, while in the latter, where the capillaries are distended, the external surface of their walls would be almost in contact with the nerve fibres and the particles of bioplasm connected with them. A dark line on each side of the artery indicates the diameter of the vessel when its walls are relaxed, and similar dark lines within the dilated portion of the capillary vessels, over figure 1, show the diameter of these vessels when the flow of blood through the artery is checked by the contraction of its muscular coat.

The arrangement represented we shall find will act as a self-regulating mechanism of the most perfect kind. Suppose for a

MECHANISM GOVERNING THE DISTRIBUTION OF BLOOD IN THE CAPILLARIES.

Diagram to show self-regulating mechanism connected with the minute arteries and capillaries. *a*, artery with muscular fibre cells; the dark lines show its diameter when dilated. *b*, small vein. *c*, capillary network. Over No. 1 the capillaries are dilated, and over No. 2 they are contracted. *d* is a ganglion cell with at least two sets of nerve fibres connected with it, one of which, *e*, divides and subdivides, giving off nerve fibres which are distributed to the artery *a*, while the other, *f*, is continuous with the plexus of nerve fibres ramifying close to the capillary vessels. Nerve fibres are also distributed to the small vein, *b*, but these are not represented in the drawing. The bioplasm of the vessels and nerve fibres is represented.

moment a tissue is receiving more nutriment than it appropriates, the capillary nerve fibre will be at once disturbed, a change instantly transmitted to the nerve centre, and the afferent nerves distributed to the artery participating, will cause the muscular fibres to contract. The arterial tube is thus narrowed, and less blood consequently sent to the tissue. In an opposite state the phenomena would be

reversed—the arterial walls relaxed and the capillaries distended with blood. Or suppose that any noxious materials or living germs of any kind were making their way from without into the blood, it is obvious that if the nerve fibres distributed to the capillaries were healthy they would be instantly affected by the contact of the foreign body, and the vaso-motor nerves of the arteries would as instantly respond to the disturbance excited in the ganglion cell, and the contraction of the arteries would succeed. This would be followed by the narrowing of the capillary vessels, and a corresponding increase of thickness in their walls, a condition likely to prevent or at least to retard the ingress of particles to the blood. The foreign body thus kept from entering the current of the circulation might remain till it was destroyed or altered by the fluid, and thus rendered harmless, or, in the case of living matter, till its death occurred. In this way it may be that the capillary nerves of a person in perfect health protect him and are directly instrumental in preventing the access to his blood of substances which are with great difficulty changed, or which must infallibly produce disease if they find their way into the circulating fluid. The fibres distributed to the capillaries are probably those which are irritated or paralyzed by certain substances which after being absorbed by the blood transude through the delicate walls of the vessels, and are thus brought into contact with the nerve fibres just outside. These are the nerve fibres which are primarily affected in cases of sudden death which result from the presence of certain poisons, such as hydrocyanic acid, for example, and which are influenced in the production of those slower changes which arise from other poisonous matters introduced into the blood.

Influence of the Veins.

In regulating the flow of blood through the capillary vessels, it must not, however, be forgotten that the veins perform an important part. The muscular fibres are spread over irregularly and ramify in all directions upon the external surface of the vein, instead of simply encircling the tube, as in the case of the arteries. The thin muscular coat of the smaller veins is abundantly supplied with nerve fibres, which ramify upon and amongst the muscular fibres in greater number than in arteries. By the reduction of the calibre of the vein the circulation of the blood in the capillaries would be retarded; and as a given degree of contraction of the muscular fibre cells can be preserved for a certain period of time, it is clear that the veins share with the little arteries the office of regulating the flow of blood through the capillaries, and form an important part of that mechanism through the instrumentality of which the distribution of blood to the

tissues and organs of man and the higher animals is constantly regulated and controlled. By the contraction of the muscular fibres of the small veins the lateral pressure of the blood upon the capillary walls may be altered from time to time, and the rapidity with which the blood traverses the capillaries caused to vary.

The smallest veins, like the capillaries, are destitute of muscular walls, but are remarkable for the great number of oval bioplasts connected with their coats and projecting into the interior of the vessel. In many of the smaller veins the united area of the bioplasts would considerably exceed that of the membranous portion of the wall of the vein. These bioplasts are the agents concerned in the absorption of nutrient constituents from the blood and their distribution in an altered form to the tissues. It is these bodies which, by removing and taking up certain of the substances dissolved in it, promote the onward flow of the blood. Thus the *vis-à-fronte* action, which has been so frequently referred to, may be accounted for and explained.

Action of the Nerve Fibres of the Capillary Vessels in Inflammation.

It is not possible that nerve fibres lying so very close to the capillary walls should be uninfluenced when the volume of blood in the capillary is increased, as in congestion. By the long-continued stretching or pressure to which these delicate nerve fibres would be subjected, partial paralysis would be induced.

In inflammations and in the long-continued feverish condition, the growth of bioplasm external to the vessels and resulting from the multiplication of bioplasts which had passed through the capillary walls in the stage of congestion, must seriously impair the action of these nerve fibres; and there is good reason to think that some of the phenomena that ensue are the direct consequence of the nerve disturbance. Let us therefore consider very briefly the changes occurring in one of the slightest and most familiar forms of inflammation—a common flea-bite.

The tissues which are perforated by the cutting instrument of the flea as it penetrates, would be, first, the epithelium; next, that modified connective tissue with its "corpuscles" (masses of bioplasm) of which the papillæ and the most superficial layer of the true skin consist. Embedded in this connective tissue are certain vessels and nerves. The following tissues would be more or less damaged by the passage of any sharp instrument through the different layers of the skin,—cuticle, connective tissue, the capillary vessels (lymphatics in some instances), and nerves; and these are the only tissues which can be affected under the circumstances alluded to. Now the capillaries, as we know, receive blood from the arteries; and the

calibre of the latter vessels, and therefore the quantity of blood traversing these and the capillaries, is governed by nerves.

Now it will be noticed in a flea-bite or other inflammation of skin that there is a certain blush which is very deep in colour immediately around the wound, but extends a considerable distance beyond the point actually injured. This inflammatory blush too, as can be easily proved, may be completely removed by pressure. If I press the skin, the tint of the inflamed portion will become of precisely the same hue as that of the surrounding healthy skin; the whole is perfectly pale. When I withdraw the pressure, and the blood is permitted to return, the capillaries of the normal skin are distended to precisely the same calibre as before, and the dilated vessels in the seat of inflammation are again dilated, and to the same degree as before the blood was pressed from them. This simple experiment proves not only that there exists some mechanism by which the calibre of the vessels may be not only increased and reduced so as to allow a larger or smaller quantity of blood to flow through the capillaries in a given time, but that the increased quantity of blood which flows through these tubes in inflammation is fixed and regulated by the degree of contraction of the muscular fibre cells of the small arteries. This can be determined only through the instrumentality of nerves and nerve centres in which a certain temporary change has been established and is maintained for a time. Such an arrangement as that described in page 59 would enable us to explain the facts above referred to.

In the early stages of inflammation probably more blood actually flows through the capillaries of the inflamed part than through those of the healthy tissues; but after a time, as is well known, the circulation becomes slower, the capillaries are distended, and at last the blood ceases to flow. The self-regulating mechanism fails to act, and, unless relief speedily follows, may be destroyed.

Inflammation, as it occurs in the tissues of the higher animals, is an exceedingly complex process, in which vessels, nerves, and many other structures take part. But other changes are occurring during inflammation which produce an influence upon the nerve fibres outside the capillary vessels though less directly. If the inflammatory change is not quickly succeeded by a return to the normal state, partial or complete paralysis of the little arteries results; the capillaries necessarily become distended, and their walls so attenuated, that the passage of fluids through them from the blood is very much facilitated. There is increased *exudation*. Now the exudation that escapes is not simply a clear fluid from which particles are deposited after the manner of crystals, and aggregated so as to form "cells," but at the time it is poured out there are suspended in it numerous living particles. These particles are found in considerable number, and are probably composed of matter like that

which enters into the composition of the white-blood corpuscle. Moreover, the small particles detached from the white-blood corpuscles make their way through, and divide, and subdivide, and give rise in a short time to multitudes of minute bioplasts which can only be distinguished by the aid of very high magnifying powers. This was referred to in a paper which was presented to the Microscopical Society in 1863, and published in the 'Transactions.' No doubt then, in many cases where the capillary vessels are distended, living particles of bioplasm pass through. These being surrounded with nutrient matter, grow, and divide and subdivide very freely. In this way are produced the multitudes of bioplasts commonly found immediately surrounding the capillary vessels in the earlier periods of inflammation of complex textures. Wherever fluid in which are suspended living particles stagnates, the conditions are favourable for the absorption by the living bioplasts of an increased quantity of nutrient material. Not only may this increased absorption of nutrient material occur outside capillary vessels, but it may proceed in the substance of a clot. The white-blood corpuscles entangled in the clot have been known to multiply to such an extent as to give rise to the formation of multitudes of bioplasts which resemble a drop of pus. Pus has been formed in a clot of fibrine. It is possible that pus may be formed in the same way in a clot of blood.

In this way I would venture to explain also the changes which ensue in the serous or fibrinous exudation which occurs in the inflammation of certain special tissues. As is well known, serous membranes are subject to what is known as adhesive inflammation, which is supposed to be distinct from the suppurative. But pus may be formed upon the surface of a serous membrane. When we consider the structure of the serous membrane, and bear in mind how exceedingly thin it is, and how close the vessels are to its free surface, it is easy to conceive that minute particles of bioplasm would pass through, collect upon the free surface, grow and multiply, and give rise to the enormous quantity of fibrinous material frequently seen, for example, upon the surface of the pleura and pericardium in inflammation. In this way is the "recent lymph" probably produced. And we know that pus may be formed if the process goes on. As nutrition increases, the little bioplasts multiply exceedingly; the "lymph" is found to consist almost entirely of bioplasts, which take up the fibrinous matter, and at last "pus," which, as I have shown, consists of multitudes of rapidly-growing living bioplasts, results.

It is not possible that such changes as those described could take place just outside capillary vessels without seriously implicating the nerves themselves, leading thereby to disturbance of the nerve centres, and by reflex action to the extensive and widely-distributed nervous derangement which usually accompanies severe inflam-

mations and general fevers. In both classes of diseases the characteristic general disturbance is due entirely to changes in the capillary vessels and in the nerves of the capillary vessels which I described in the first part of this paper, published in the January number of the Journal.

Alteration of Nerve Fibres of Capillaries in Chronic Disease.

In many forms of chronic disease the nerve fibres distributed to the capillary vessels, as well as those ramifying upon the small arteries and veins, degenerate and are at length destroyed. So that the variation in vascular tension and blood distribution which occur in the healthy state through the agency of these nerves cannot take place. If therefore in consequence of the body being exposed to very adverse conditions the circulation be much disturbed, the derangement cannot be compensated or the injury repaired. It too often happens that the damage is increased until sufficiently great to render a fatal result inevitable.

In fatty degeneration of the capillary vessels and small arteries, the fine nerve fibres are also involved, and the mechanism for regulating the flow of blood through the small vessels is gradually impaired. In some cases the nerve fibres and the muscular fibres of the arterial coats have so completely degenerated that the distribution of blood is greatly impeded. The outline of the vessels becomes uneven, their coats thicker in some places than in others, and the lining membrane rough. The elasticity of the vascular walls is much impaired, and in not a few instances the tube is actually rigid. The nutrition of every organ in the body suffers partly from changes which affect the circulation of the blood, and in part from the altered composition of the blood itself, which is gradually induced by the deranged circulation occurring in the blood-forming and blood-changing organs.

The condition of *health* is dependent upon the integrity of the nerve fibres distributed to the capillary vessels. It is therefore of the utmost importance to do all we can to keep these delicate nerve fibres in an active state. By exercise, cold bathing, and external rubbing, that amount of activity of the nerve fibres and change in the capillary circulation, is ensured at least once in the twenty-four hours that is required in order that the nerve mechanism which has been described may be exercised and its activity preserved. These nerve fibres are, as it were, the sentinels which give the earliest information of the dangerous proximity to the vessels of matters the entrance of which into the blood might occasion serious disease or cause death. So long as these sentinels are active and in thorough good working order, warning is given in time for the performance of acts which prevent the ingress of deleterious substances; but if they

act sluggishly, or if the excitability of these nerve fibres is numbed by certain agents, time is allowed for the poison just outside the vessels to make its way into the blood; while if they are paralyzed, even temporarily, the state of things is far worse. No watch is kept, and the organism may be destroyed by an invading poison that under other circumstances might have been effectually excluded.

III.—*Remarks on the Nomenclature of Achromatic Objectives for the Compound Microscope.*

By Dr. J. J. Woodward, U. S. Army.

For some years past, while most of the Continental opticians have continued to give arbitrary designations to their achromatic object-glasses, such as No. 1, No. 2, &c., or System A, System B, &c., the English and American manufacturers, affecting a higher degree of accuracy, have undertaken to name the objectives they construct by their real or supposed agreement in magnifying power, with single lenses of specified focal lengths. We hear accordingly of inch, half-inch, and quarter-inch objectives, &c., by which we are expected to understand combinations agreeing in magnifying power with single convex lenses of the focal lengths named.

At first sight nothing could appear simpler or more exact than such a nomenclature; nevertheless recent articles in the journals would seem to indicate that the general plan is capable of considerable modification in its practical application, and that grave misunderstandings have hence arisen.

Under these circumstances, it appears desirable to give some account of the principles involved, and of the practical difficulties to be considered in their application, particularly as the microscopical text-books contain little or no information on the subject. In fact, the only scientific discussion of the matter with which I am acquainted is the paper of Mr. Charles R. Cross, "On the Focal Length of Microscopic Objectives."* This paper gives a reasonable formula for the approximate computation of equivalent focal lengths, and furnishes some other valuable information, but does not discuss all the points at issue. I shall have occasion to refer more than once to this excellent paper, which the reader would do well to examine in connection with the following remarks.

We learn from the elementary treatises on optics that when an object is placed in front of a single convex lens at a distance somewhat greater than its focus for parallel rays, a real image is formed

* 'Journal of the Franklin Institute,' June, 1870, p. 401.

on the other side of the lens, which may be received on a screen. This image will be larger, and formed at a point more distant from the lens, the nearer the object approaches to the focus for parallel rays; and two equations are given which express the relationship of the distances to each other, and to the magnifying power, *viz.* $\frac{1}{f} = \frac{1}{p} + \frac{1}{p'}$, and $\frac{p'}{p} = m$, in which f is the length of the focus for parallel rays, p the distance of the lens from the object, p' its distance from the image, and m the true magnifying power, that is, the size of the image divided by the size of the object; p and p' are termed the conjugate foci, and are variable quantities; f is termed the principal focus, and has an unchangeable value for each single lens.

If now we combine the above equations, representing $p + p'$ or the sum of the conjugate foci by l, we may deduce the formula $f = \frac{m\,l}{(m+1)^2}$, which represents in the case of any single convex lens the relationship existing between the length of the principal focus, the magnifying power, and the distance from the object to the screen. This formula, which I think rather more convenient than that of Mr. Cross, differs from it only in using $m =$ the magnifying power, instead of $n =$ the reciprocal of the magnifying power; it may be deduced from his by substituting for n its value $\frac{1}{m}$ and reducing, or it may be derived directly from the primitive equations. In either shape the formula yields the same numerical results, and if for any single convex lens m and l are given, the accuracy of the value of f resulting will be limited only by the degree of precision with which m and l have been measured.

If now there were any such actual equivalence between achromatic objectives and single lenses as the nomenclature assumes, it would only be necessary to set up the objective to be rated, in such a manner that the image of a micrometer should be focussed upon a white screen, using of course no eye-piece, to measure the distance from the micrometer to the screen, to determine the magnifying power by measuring the image of the micrometer, and substituting these values of l and m in the working formula to calculate the value of f. Unfortunately, however, if with any compound objective we repeat this operation several times, merely varying the distances, we obtain as many different values for f as there are distances used, instead of obtaining but one value for all distances as we do with a single lens.

Mr. Cross* has already pointed out this circumstance, which results from the fact that the modern achromatic objective has

* *Loc. cit.*, p. 409.

considerable thickness from its anterior to its posterior surfaces, and that it has, properly speaking, no true optical centre. He gives two examples, in one of which a change of 5·24 inches in distance corresponded to a change of ·0067 inch in calculated focal length; in the other a change of 6·10 inches in distance corresponded to a change of ·0029 inch in calculated focal length, the objectives used in the experiment being uncorrected ¼ths, so called. If, however, Mr. Cross had used for this purpose lower powers, or had made greater variations in the distances employed, he would have found much greater discrepancies. For example, by measuring the magnifying power first at 25 and then at 50 inches, and deducing the value of f by the formula of Mr. Cross, I obtained in two cases the following equivalent focal lengths:—For a so-called 1½ inch, at 50 inches distance, 1·2187 inch; at 25 inches distance, 1·2468 inch; difference, ·0271 inch. For a so-called ¼th, at 50 inches distance, ·1982 inch; at 25 inches distance, ·1893 inch; difference, ·0089 inch.

Moreover, since the achromatic objectives of different makers are constructed on different series of curves, and the component lenses placed at different distances apart, it will be found that if two achromatic objectives magnify the same at any given distance, they will no longer do so if the distance is materially changed.

Hence I am compelled to agree fully with the observations of Mr. Cross* that the nominal focal length assigned to an achromatic objective can only serve in any case as "a general appellation serving to group together objectives of *approximately* the same magnifying power," and must conclude, therefore, that the English and American nomenclature possesses no real claim to strict scientific accuracy, and especially that the comparison made by some with the case of the celestial telescope is not valid.

But besides the inevitable inaccuracy resulting from this source, there are in the case of the higher powers of modern makers two other sources of much more considerable error. The first of these involves the case of all those objectives which are provided with a screw-collar to correct for thickness of cover; the second involves the case of objectives with two fronts, one for wet and the other for dry, or those with but one front which can be used wet or dry by merely changing the correction given by the screw-collar.

The correction for thickness of cover is made, as is well known, by changing the distance between the front combination of the triplet and the posterior two combinations. As a consequence, the magnifying power of the objective at any given distance, or with any given eye-piece, is least when the objective is corrected for uncovered, and greatest when it is corrected for the thickest cover through which it will work. The ratio of this change is very

* *Loc. cit.*, p. 401.

different in different objectives. I select a few from my note-book in illustration, purposely omitting to name the makers :—

	Magnifying power at uncovered.	Magnifying power at covered.
No. 1	200	225
No. 2	250	275
No. 3	300	333
No. 4	350	500
No. 5	570	630
No. 6	900	1100
No. 7	975	1180
No. 8	890	1250

These values were obtained by throwing the image of a micrometer on a cardboard screen, using the objective without an eye-piece, and the distance from micrometer to screen in each case remained the same, the screw-collar and the focal adjustment of the objective being modified. Of course, for all intermediate positions of the screw-collar, intermediate values result. The distance used was 48 inches in some of the cases, 50 in the others. With shorter distances, the amount of the difference is diminished, but its ratio to the magnifying power at uncovered is not materially changed, as anyone can convince himself by comparing a stage micrometer, as seen by any corrected objective, with an eye-piece micrometer, first at uncovered and then with the full correction for cover.

Now it is evident that even if, by the formula of Mr. Cross, or otherwise, we could obtain accurate equivalent focal lengths for any one position of the cover correction, the result would not be true for any other position of the cover correction.

We are told by a recent writer that the practice of the opticians is to name the combination at its performance uncovered, that is, at precisely the adjustment least used. If, however, scientific accuracy in the matter is desirable for any purpose whatever, it is evident from the above that at least the maximum and minimum should be furnished by the maker.

It is also evident that the considerations here offered, aside from their bearing on the nomenclature of objectives, have a high practical value to all those who attempt to make micrometric measurements with modern high-power objectives; for the practice recommended in the text-books, and too generally pursued, is to give values to an eye-piece micrometer by comparing it with a stage micrometer at a fixed position of the draw-tube, and to use these values in subsequent measurements. Now as these values vary considerably with the cover correction, it is to be feared that the majority of the measurements made with high powers during the last twenty years are sadly inaccurate.

The difficulties in the way of a nomenclature based upon equivalent focal lengths have been still further increased since the introduction of immersion objectives. The compound objective is usually furnished with two fronts, one for wet and one for dry use. Of these the wet usually gives the greatest magnifying power at any given distance. I have also measured an objective which, when corrected for the thickest cover through which it will work dry, is just corrected for uncovered wet, and by approximating the posterior pair of combinations still nearer to the anterior, corrects for cover wet, thus increasing the magnifying power when the objective is in use wet precisely as if two fronts were used.

The difference in magnifying power resulting from the modifications given to make the objective perform wet is quite considerable, as may be seen by the following extracts from my note-book.

	Magnifying Power.			
	Dry.		Wet.	
	Uncovered.	Covered.	Uncovered.	Covered.
No. 1	225	250	250	275
No. 2	425	490	450	500
No. 3	700	900	900	1000
No. 4	770	910	900	1100
No. 5	790	930	975	1180

Of these objectives No. 1 had but one front, the correction for wet being made by the cover correction; the others had two separate fronts, one for wet, the other for dry.

Now it is just in connection with these complex objectives, with double fronts or other devices to correct for wet and dry, that the greatest diversity of nomenclature exists. In one quarter it is claimed that the system should have two names, one based on its magnifying power at uncovered wet, the other on its magnifying power at uncovered dry; in other quarters the practice has been to give the system but a single name, derived from the magnifying power at uncovered dry alone. It is evident, however, that neither of these plans has any pretension to scientific accuracy, and that if the makers will not give at least the maximum and minimum for both dry and wet, the purchaser must learn to measure for himself.

A similar remark applies to the angle of aperture as stated by the makers, for each objective sold. As a rule they give the greatest angle attainable by the combination, which is generally—not always—the angle at the correction for thickest cover, their usual practice in this case being the reverse of their usual practice with regard to magnifying power. Now I have seen objectives with an angle of 170° and upwards at full cover correction, which did not exceed 140°

at uncovered. It is evident, therefore, that the maker should furnish with each glass both the maximum and minimum angle, or the microscopist must measure for himself.

After a full consideration of all the circumstances, I am disposed to think that the best interests of both makers and purchasers would be consulted if the present nomenclature were abandoned altogether, and objectives named instead by their precise magnifying power without eye-piece at some selected distance. It would be well if all the makers could be brought to agree on some fixed distance; but until we obtain this happy uniformity, which perhaps is not to be anticipated, it is only necessary for each maker to state the distance he selects. By this plan objectives without correction for cover would be named by one number, objectives with correction by two, and those with two or more fronts or backs by two or more pairs of numbers.

Thus we should have objectives without cover corrections named precisely 2, 3, 4, 5, 6, 7, 8, 9, and so on up to 100 or more, the number indicating the exact magnifying power, say at 12½ inches from micrometer to screen. Objectives with cover corrections would be named 30 to 40, 35 to 46, 75 to 89, 125 to 140, &c., the numbers representing the minimum and maximum magnifying powers at the selected distance. Objectives with wet and dry fronts would require a separate name for each; thus 78 to 95 dry, 98 to 130 wet, &c.

By this plan the real magnifying power of the glass and its limits of variation would be accurately stated, whereas at present even those makers who are most careful about their nomenclature do not hesitate to call a glass an $\frac{1}{8}$th provided its power at uncovered approximates that of a single lens of $\frac{1}{8}$th of an inch focus more nearly than it does a $\frac{1}{4}$th or a $\frac{1}{16}$th. Hence glasses which differ materially in magnifying power at uncovered, receive the same name, and the changes in power produced by the cover correction are invariably ignored.

If the plan I here recommend be adopted, the precise distance from micrometer to screen which may be chosen does not appear to me to be of very great importance. Many persons would, I suppose, prefer 10 inches to 12½. I have selected the latter number because many of our larger stands have tubes too long to permit the convenient measurement of low powers with eye-piece and stage micrometers, in the manner I shall presently describe, if the distance be taken at 10 inches, whereas on most stands, with the help of the draw-tube, 12½ inches can be used conveniently with all powers from the 3-inch upwards. I do not, however, insist on any particular distance, but only that the distance selected shall be stated in each case until some uniform plan shall be generally agreed upon.

In this connection I may add that the actual plan of the Continental opticians is also unsatisfactory. In the matter of angle of aperture, when they give it at all, they also, as a rule, give only the maximum. In the matter of magnifying power, when they give any information, they attempt, as a rule, to give the magnifying power of the objective with each eye-piece as actually looked through when in use. But as the magnifying power of the combination under these circumstances involves the distance of distinct vision for each observer, it is evident that the figures thus furnished cannot have any practical value.

Pending the adoption of some such system as I have suggested above, it will be necessary for the microscopist to measure for himself the magnifying power of the objectives he uses, whenever he desires to be possessed of the real information concealed behind all the several systems of nomenclature at present in use. Mr. Cross, in his paper, gives two modes of doing this. I myself have been in the habit of using a dark room, and throwing the image of the micrometer on a white screen, by the direct rays of the sun. Very nearly equal accuracy may be attained, however, by any microscopist who possesses an ordinary glass eye-piece micrometer and a stage micrometer. The glass eye-piece micrometer, it will be recollected, is slipped through the eye-piece in such a manner as to be just in the focus of its eye-glass, that is, in the plane which is occupied by the image formed by the objective when it is seen most distinctly. If now the field-glass of such an eye-piece be removed, and the stage micrometer carefully brought into focus, a comparison of the divisions of the eye-piece and stage micrometers will give the magnifying power of the objective alone at the distance actually existing between the two micrometers. Thus, for example, if the stage micrometer is in $\frac{1}{1000}$ths and the eye-piece micrometer in $\frac{1}{100}$ths of an inch, five times the number of eye-piece divisions corresponding to one division of the stage micrometer will be the magnifying power of the objective at the cover correction employed, for the distance selected. We may then apply the following simple rule, derived from the formula of Mr. Cross.

Multiply the distance between the two micrometers in inches and decimals, by the magnifying power, and divide by the square of the magnifying power plus one; the result will be the equivalent focal length (for the given conditions) in decimals of an inch.

It will, moreover, be found in practice that for powers equivalent to those of a $\frac{1}{4}$th or shorter focal lengths a still simpler rule may be adopted, and that the distance between the two micrometers, divided by the magnifying power, will give very nearly the same results as are obtained by the more complex rule, but this of course is not true for lower powers.

It must, however, be constantly borne in mind, that the results

obtained in any case are true only for the cover correction and distance used.

For the convenience of those who may undertake such comparisons, I append a Table in which the real magnifying powers of single convex lenses are given for three different distances. In the treatises on optics, the magnifying powers of single lenses are sometimes stated at some given distance from the lens to the screen, but I know of no table which shows their powers at given distances between image and object.

This Table is calculated by substituting the numerical values of f and l in the equation $f = \frac{m\,l}{(m+1)^2}$, when m = the magnifying power remains as the only unknown quantity, and is easily computed. I have carried out the values of m to two decimal places only; but in practice the nearest whole number will be found sufficiently accurate.

TABLE OF THE MAGNIFYING POWERS OF SINGLE CONVEX LENSES.

Focal length for parallel rays.	Magnifying power at 12½, 25, and 50 inches distance from micrometer to screen.		
	12½ inches.	25 inches.	50 inches.
8 inches	1·50	6·17	14·59
2 ,, ...	4·00	10·40	22·95
1½ inch	6·17	14·59	31·30
1 ,, ...	10·40	22·95	47·99
½rds of an inch	16·69	35·47	72·98
$\frac{1}{10}$ths ,, ...	29·21	60·48	122·99
$\frac{1}{20}$th ,, ...	47·99	97·98	197·99
$\frac{1}{30}$th ,, ...	60·48	122·99	247·99
$\frac{1}{40}$th ,, ...	72·98	147·99	297·99
$\frac{1}{50}$th ,, ...	97·98	197·99	397·99
$\frac{1}{60}$th ,, ...	122·99	247·99	497·99
$\frac{1}{75}$th ,, ...	147·99	297·99	597·99
$\frac{1}{100}$th ,, ...	185·49	372·99	747·99
$\frac{1}{125}$th ,, ...	197·99	397·99	797·99
$\frac{1}{150}$th ,, ...	222·99	447·99	897·99
$\frac{1}{200}$th ,, ...	247·99	497·99	997·99
$\frac{1}{250}$th ,, ...	310·49	622·99	1247·99
$\frac{1}{500}$th ,, ...	622·99	1247·99	2497·99

Note.—Since writing the foregoing article, I have read with pleasure a paper on the same subject by Dr. R. H. Ward.* This paper contains much valuable matter, and should be read by all who are interested in this subject. I learn from it for the first time that the method of determining the magnifying power of objectives by removing the field-glass of the eye-piece and using eye-piece and stage micrometer as described above, has already been

* "Remarks on Uniformity of Nomenclature in regard to Microscopical Objectives and Oculars," 'American Naturalist,' March, 1872, p. 136.

used by Dr. J. J. Higgins, of New York,* to whom I hasten to give the credit due.

I am very glad to find Dr. Ward indicates the feasibility of the substitution of magnifying powers at a fixed distance as names for objectives instead of alleged equivalent focal lengths. I must, however, differ from him when he recommends that the distance should be measured from some part of the objective to the screen, instead of from the micrometer to the screen; for however desirable this may be made to appear, it is not feasible. Nor can I agree with him in regarding 10 inches distance as having anything special to recommend it. To the question he asks at the conclusion of his paper, "at what point of the screw-collar adjustment shall the objective be placed for rating its angular aperture and amplifying power?" I reply without hesitation, that for each purpose accuracy demands that the maximum should be given as well as the minimum; that the maker should state not merely one limit, but both.

Dr. Ward's paper also contains some interesting suggestions on the subject of the nomenclature of eye-pieces, a matter which will, however, I think, particularly in the case of the ordinary eye-piece, require further discussion. I agree fully with Doctor Ward that eye-pieces should be named by their magnifying power: but the question at once arises, how shall this be accurately measured? To this subject I may recur at some future time.—*Silliman's Journal*, June.

IV.—*Our Present Medical and Students' Microscopes.*

By R. H. Ward, M.D.

Those who use the microscope as an elegant and costly luxury will, of course, be guided in so doing by their general ideas of taste, economy, &c.; the few who use the instrument as medical experts or original investigators in science, will, at the same time, by years of practice, grow into the use and the possession of an instrument suited to their wants; but a larger class are those who use the instrument as an incidental though frequent aid in their daily work in various sciences or professions, who reasonably desire the simplest instruments consistent with real usefulness, and who, however eminent in other specialities, are often unfamiliar with the styles and prices of the various makers, and at a loss to know what available resources would best supply their wants. The following Tables are designed to be of use to buyers of microscopes, of the latter

* 'American Naturalist,' Dec. 1870, p. 628.

class, and to persons who desire information in a concise and convenient form, in regard to the progress thus far made in this department of microscopy. While the styles and prices will be subject to endless variation, the statements made will be sufficiently accurate to form a basis for selection and correspondence for a considerable time.

Only standard and available styles are given, by reliable American makers, and no such makers have been intentionally omitted. The selection of instruments and apparatus of the different makers is entirely the responsibility of the writer, and not of the makers. He has selected such sets, at the makers' prices, as he has been accustomed to recommend to students and others for the general purposes of students' microscopes.

The Table of Objectives is designed to be complete in regard to American lenses, and to present for comparison, and for the convenience of those who desire a variety of lenses by different makers, such European items as could be obtained at the time of writing. It is believed that the Table will be interesting to others than buyers, as showing the prevailing powers, prices, angular apertures, &c., in different parts of the world. Notwithstanding the general impression that the prices of American objectives are unreasonably high, it will be seen that they compare favourably with those of the most celebrated English makers. Most opticians prepare, very cheaply, mounted objectives of small angle, and usually second quality, which answer a good purpose where economy is imperative and difficult work is not required. Such lenses are not usually priced and sold separately by the American makers, and are therefore not included in their columns in the Table of Objectives.* They can be obtained, however, at prices proportioned to their angles and quality. The prices and angular apertures given in the Table are generally, though not in every case, official and up to date; but a reasonable allowance must be made for practical variation from the standard angles and powers. There is understood to be a screw-collar adjustment for all objectives of over 50° angular aperture, unless otherwise stated, though, from want of sufficiently distinct description by their makers, this rule cannot be applied to some of the English lenses of medium power and small angle. To persons without great experience in microscopy, the best "test" of an objective is the respectability of its maker.

The binocular microscope, though not especially applicable to medical microscopy, is of such general usefulness that it should be preferred in all cases where its price is not a positive objection. Binoculars are furnished, when ordered, even by those makers who do not manufacture them.

* This remark would also apply to a few of the English opticians.

TABLE OF OBJECTIVES, THEIR MAKERS AND PRICES.

a. See Table of Student-' Microscopes.
b. C. Baker, 243 and 244, High Holborn, London.
c. R. & J. Beck, 31, Cornhill, London, E.C.
d. John Browning, 63, Strand, London, W.C.
e. Charles Collins, 157c, Great Portland Street, Portland Place, London, W.
f. Henry Crouch, 51, London Wall, London, E.C.
g. J. B. Dancer, 43, Cross Street, Manchester.
h. Horne & Thornthwaite, 122 and 123, Newgate Street, London, E.C.
i. James How, 2, Foster Lane, London, E.C.
j. Murray & Heath, 69, Jermyn Street, London, S.W.
k. M. Pillischer, 88, New Bond Street, London, W.
l. Powell & Lealand, 170, Euston Road, London.
m. Thomas Ross, 7, Wigmore Street, Cavendish Square, London, W.
n. J. H. Steward, 406, Strand, London, W.C.
o. James Swift, 43, University Street, Tottenham Court Road, London, W.C.
p. Edmund Wheeler, 48, Tollington Road, Holloway, London, N.
q. E. Hartnack & Co., 21, Place Dauphine, Paris, France.
r. E. Gundlach, Optical Institute, Charlottenberg, near Berlin, Prussia, Germany.
s. G. & S. Merz, Optical Institute, Munich, Bavaria, Germany.
t. R. Wasserlein, 27, Schutzen Street, Berlin, Prussia, Germany.
u. Carl Zeiss, Jena, Germany.

Nominal focus in inches.	Angular aperture in degrees.	Grunow.[a] Price in dollars. a.	Miller. Price in dollars. a.	Queen. Price in dollars. a.	Spencer. Price in dollars. a.	Tolles.[b] Price in dollars. a.	Wales.[c] Price in dollars. a.	Zentmayer. Price in dollars. a.	Baker. Price in pounds and shillings. b.	Beck. Price in pounds and shillings. c.	Browning. Price in pounds and shillings. d.	Collins. Price in pounds and shillings. e.	Crouch. Price in pounds and shillings. f.	Dancer. Price in pounds and shillings. g.	Horne & Thornthwaite. Price in pounds and shillings. h.	How. Price in pounds and shillings. i.	Murray and Heath. Price in pounds and shillings. j.	Pillischer. Price in pounds and shillings. k.	Powell and Leland.[d] Price in pounds and shillings. l.	Ross. Price in pounds and shillings. m.	Steward. Price in pounds and shillings. n.	Swift. Price in pounds and shillings. o.	Wheeler. Price in pounds and shillings. p.	Hartnack.[e] Price in francs. q.	Gundlach. Price in thalers. r.	Merz. Price in thalers. s.	Wasserlein. Price in thalers. t.	Zeis. Price in thalers. u.
5	..	..	..	..	..	..	..	16½	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
4	7	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1 10	..	..	..	..	..	..	..	..
	8	..	..	..	..	18/	..	16½	1 5	..	..	..	..	..	..	..	..	1 8½	..	1 10	..	..	..	..	..	..	..	..	..
	9	..	..	..	..	..	12	..	..	..	..	..	1 0	..	..	..	..	..	1 10	1 10	1 10	1 10	1 0	..	..	..	..	..	..
	10	..	..	..	35/	35/	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	12	18	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
3½	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1 10	..	..	..	..	..	..	..	..	..	..	..	..	..	..
3	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1 13	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	6	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1 1	..	..	..	..	..	..	..	..	..	..	..	..
	8	..	..	..	..	..	..	..	..	2 0	..	..	..	..	..	..	..	0 15	..	..	..	..	..	..	..	..	..	..	..
	10	..	13½	..	..	..	..	..	1 15	..	2 0	1 10	..	1 0	..	..	..	..	..	..	1 5	1 0	1 10	..	..	..	..	..	..
	12	18	18	..	..	..	15	..	3 10	..	..	2 0	1 10	2 5	..	..	..	2 15	1 10	2 15	3 0	2 10	1 10	2 2	..	..	..	..	..
2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1 15	..	..	..	..	..	..	..	..	..	15	..	..	..	..	..
	10	..	..	..	..	30	..	..	..	1 10	0 18	..	..	..	..	1 1	0 18	..	..	..	..	..	..	..	..	..	..	..	..
	12	..	13½	..	15	..	..	..	1 10	..	..	1 10	1 0	1 0	..	..	1 8	1 10	..	2 15	..	1 5	1 0	1 10	..	..	..	..	..
	13	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	14	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..

	15	..	18	..	..	..	..	..	1 17½	..	..	2 0	1 10	2 5	..	2 10	2 15	2 10	..	3 0	2 10	1 10	2 2	..	..	..	..	..			
	16	..	..	..	..	..	..	..	..	..	2 0	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..			
	18	..	..	..	..	23	..	..	..	3 10	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..			
	20	18	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..			
	27	..	..	..	30	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..			
1½	12	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1 1	..	..	..	..	..	..	..	..	..	..	..			
	13	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1 5	..	..	..	..	..	..			
	15	12	..	..	..	..	..	..	..	2 10	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2	..			
	16	..	..	..	..	..	..	..	..	..	..	..	..	1 10[h]	..	..	..	..	..	..	..	..	..	..	..	..	..	..			
	17	..	..	..	..	28[i]	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..			
	20	..	22	..	..	..	..	..	1 17½	..	2 5	..	1 10	2 10[h]	..	2 15	2 17½	..	3 0	3 0	2 15	1 10	2 10	..	..	..	..	..			
	22	18	..	..	.	..	17	15	..	..	..	..	..	..	..	..	..	2 10	..	..	..	..	..	..	..	..	..	..			
	23	..	..	..	..	..	..	..	..	3 10	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..			
	37	..	..	..	30	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..			
1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1 15	..	..	..	..	..	..	..	..	20	..	..	..	..			
	15	..	..	..	..	..	..	..	1 10	..	0 18	..	..	..	..	1 1	1 0	1 10	..	2 0	1 5	..	..	..	..	..	..	..			
	16	..	13½	..	..	..	..	..	..	..	..	..	1 0	1 10	..	..	..	..	..	..	..	..	1 10	..	..	..	..	..			
	17	..	..	..	..	20	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..			
	18	..	..	12	..	..	..	..	..	..	..	1 10	..	..	..	..	..	..	..	..	..	1 0	..	..	4	..	..	..			
	20	..	..	..	20	..	..	..	..	..	..	..	..	2 10[j]	..	..	1 15	..	..	..	..	..	..	..	8	..	..	..			
	22	..	..	..	..	..	..	..	..	2 10	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..			
	23	..	..	..	..	..	..	..	1 17½	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..			
	25	20	22	..	..	23	..	..	..	..	2 10	2 0	1 10	2 10	..	3 3	3 0	2 10	..	3 10	3 0	1 10	2 10	..	..	..	..	..			
	27	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1 15	..	..	..	..	..	..			
	30	20	..	..	30	30	..	..	2 2	..	..	..	..	..	..	..	..	..	3 3	..	..	..	..	..	..	..	..	..			
	35	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2 0	..	..	..	..	..	..			
	40	..	..	..	40	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..			
	45	..	..	..	40	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..			
8–10	20	..	..	12	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2	..			
	32	..	..	..	..	..	..	18	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..			
3–4	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	25	..	..	..	..			
	40	..	..	..	..	30	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..			
2–3	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1 15	..	..	..	..	..	..	..	..	..	..	..	..	7			
	22	..	..	15	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..			
	25	15	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..			
	28	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2 10	..	..	..	..	..	..	..			
	30	..	22	..	..	..	20	..	..	..	..	2 10	1 10	..	..	..	..	..	..	..	..	1 10	2 10	..	..	..	..	..			
	32	..	..	..	..	..	..	..	..	3 3	..	..	..	..	..	..	..	..	3 10	..	..	..	..	..	..	..	..	..			
	35	22	..	..	..	..	..	..	2 5	..	3 0	..	..	3 0	..	..	..	..	..	3 10	3 5	1 15	..	..	..	..	..	..			

a. Mounts, to special order, objectives as high as 1-10 inch, to work well on any binocular stand.
b. All of 1–5 or higher, either dry or immersion. Both ways with same lens, or with extra front, at extra price.
c. Higher powers to order.
d. Improved arrangement for thick covering-glass, 1l. 10s. extra.
e. All of 1-12 (No. 9) and higher, immersion and adjustable.
f. Changeable, by reversing, to 2½ inches.
g. Changeable, by sliding tube, to 3 inches.
h. Divides to give 3 inches.
i. Has a front to give 1 inch of 25°.
j. Divides to give 2 inches.

TABLE OF OBJECTIVES, THEIR MAKERS AND PRICES—*continued.*

Nominal focus in inches.	Angular aperture in degrees.	a. Grunow.[a] Price in dollars.	a. Miller. Price in dollars.	a. Queen. Price in dollars.	a. Spencer. Price in dollars.	a. Tolles.[b] Price in dollars.	a. Wales.[c] Price in dollars.	a. Zentmayer. Price in dollars.	b. Baker. Price in pounds and shillings.	c. Beck. Price in pounds and shillings.	d. Browning. Price in pounds and shillings.	e. Collins. Price in pounds and shillings.	f. Crouch. Price in pounds and shillings.	g. Dancer. Price in pounds and shillings.	h. Horne & Thornthwaite. Price in pounds and shillings.	i. How. Price in pounds and shillings.	j. Murray and Heath. Price in pounds and shillings.	k. Pillischer. Price in pounds and shillings.	l. Powell and Lealand.[d] Price in pounds and shillings.	m. Ross. Price in pounds and shillings.	n. Steward. Price in pounds and shillings.	o. Swift. Price in pounds and shillings.	p. Wheeler. Price in pounds and shillings.	q. Hartnack.[e] Price in francs.	r. Gundlach. Price in thalers.	s. Merz. Price in thalers.	t. Wasserlein. Price in thalers.	u. Zeiss. Price in thalers.
6–10	52						30′		2 10		1 15	3 10′	2 5		2 12½		2 10′					3 0		30	4	8		12
1–2	64								3 0	2 10		2 5	2 10′	1 10		2 2	1 17½						2 5′					
1–10	30															4 4												
	35																3 3											
	38																	4 10										
	40												3 15				2 7½											
	55																		5 0									
	56																			4 0								
	60																				5 5							
	65																					5 5						
	70																						2 10					
	75																							3 15				
	80																											
	85																											
	90																											
	25																											
	50																											
	55																											
	64																											
	70																											
	75																											
	80																											
	85																											
	90																											
	95																											
	110																											
	135																											
	146																											
1–3	30																											

| | | |

TABLE OF OBJECTIVES, THEIR MAKERS AND PRICES—*continued.*

Nominal focus in inches.	Angular aperture in degrees.	a. Grunow.[a] Price in dollars.	a. Miller. Price in dollars.	a. Queen. Price in dollars.	a. Spencer. Price in dollars.	a. Tolles.[b] Price in dollars.	a. Wales.[c] Price in dollars.	a. Zentmayer. Price in dollars.	b. Baker. Price in pounds and shillings.	c. Beck. Price in pounds and shillings.	d. Browning. Price in pounds and shillings.	e. Collins. Price in pounds and shillings.	f. Crouch. Price in pounds and shillings.	g. Dancer. Price in pounds and shillings.	h. Horn & Thornthwaite. Price in pounds and shillings.	i. How. Price in pounds and shillings.	j. Murray and Heath. Price in pounds and shillings.	k. Pillischer. Price in pounds and shillings.	l. Powell and Lealand.[d] Price in pounds and shillings.	m. Ross. Price in pounds and shillings.	n. Steward. Price in pounds and shillings.	o. Swift. Price in pounds and shillings.	p. Wheeler. Price in pounds and shillings.	q. Hartnack.[e] Price in francs.	r. Gundlach. Price in thalers.	s. Merz. Price in thalers.	t. Wasserlein. Price in thalers.	u. Zeiss. Price in thalers.
1–7	130					55																						
1–8	135					65				5 5																		
	140					35/			5 5			4 4			4 4													
	100									8 8						5 0	6 0											
	70								6 6		7 0	6 10	4 10	6 10				4 12½			7 7	8 8						
	85															7 7		7 10	7 10									
	95																			8 9¼								
	100																					5 5						
	115																											
	120																											
1–9	125																											
	130																											
	135																											
	140	45	72		65	45	45																					
	145	45																										
	150				60/																							
	160				80/	75	80																					
1–10	175					80																						
	120																											
	100					45				6 0⅝																		
	125																											
	130	30/				80																						
	160	80/																										
	170					85	60																					
1–11	175																											
1–12	120					60																						

[a] Grunow's objectives are now made by Tolles.
[b] Tolles's objectives are now made by Grunow.
[c] Wales's objectives are now made by Zentmayer.
[d] Powell and Lealand's objectives are now made by Swift.
[e] Hartnack's objectives are now made by Wasserlein.

| | | |

SYNOPSIS OF STUDENTS' MICROSCOPES.

Model.

Must have joint to incline at various angles from horizontal to perpendicular.

Curved Bar.—"Jackson" (but not prolonged beneath stage): steadiest form for equal weights.

Transverse Bar.—Most convenient in some respects.

Weight.

Four to ten pounds.—A question of convenience *v.* firmness.

Height. (12 to 16 inches.)

Tall.—Much room below stage. Standard length of tube gives usual powers. Looks well, and is most convenient when inclined.

Short.—Easiest for vertical use. Short tube works lenses at too low power. Should be lengthened by draw-tube when inclined.

Diameter of Tube. (1 to 1 3-8 in.)

Small.—Looks best and is steadiest on small instruments.

Large.—Gives wider field with low-power oculars.

Material.

All Brass.—Looks best.

Iron Base.—Saves expense. Wears well unless broken by falling.

Coarse Adjustment.

Rack and Pinion.—Easiest; preferable, but costly.

Friction Pinion.—Said to be very delicate.

Chain Movement.—Fine motion. Easily repaired if necessary.

Sliding Tube, by hand.—Saves expense. Most delicate, but awkward. Wants expert hands, and often two of them.

Fine Adjustment. Essential.

Screw and lever moving nose-piece.—Best. Costly.

Screw and lever moving compound body.—Good for moderate powers.

Screw moving compound body.—Less delicate.

Screw against shoulder, moving nose-piece.—Less delicate.

Screw on nose-piece.—Little used.

Screw moving stage.—Cheapest in use in this country. Bad in theory and history; but quite satisfactory in use, as made at present.

Bar moving pinion of rack.—Not in this country. Good for low powers. May be added to instruments having no other fine adjustment.

Stage.

Mechanical.—Not required.

Lever.—Plausible, but not successful.

Magnetic.—Plausible, but not successful. (Insecure.)

Hand movement. (In two rectangular directions.)—Best substitute for mechanical, for high powers, and with Maltwood finder.

Glass, concentric.—Best for binocular.

Glass, sliding by hand.—Cannot be too highly commended for ordinary medical use.

Brass.—Sliding by hand (cloth-lined, or otherwise). A good and cheap substitute.

Plain.—Saves expense. Should have removable spring clips. Could have glass or brass sliding-stage added.

Mirror.

Must have lateral, vertical, and two rotary movements.

Lateral movement by cranked arm: not common in students' microscopes. Lateral movement by mirror swinging partly around the bar which carries it. Lateral movement by hinge joint at top of bar which carries mirror: obliquity entirely independent of distance of mirror from object.

Concave.—Indispensable. Should be nearly two inches wide.

Plain.—Very desirable, and costs little. May be smaller.

Diaphragm.

Fitting should be removable, for oblique light.

Disk with apertures.—Essential. Should be closed between diaphragm and stage to prevent accidental oblique light.

"*Graduating*" or "*Iris.*"—Incomparably best, but unnecessarily expensive.

Oculars (eye-pieces).

Negative (Huyghens') always meant, unless otherwise stated. Quality (and performance) varies greatly, though seldom suspected.

Two-inch.—(The A or No. 1 of most makers.) X* 5. Best for general use. Should always be negative.

One-inch.—(Often named B or C, or No. 2 or 3.) X 10. Use for micrometry, occasionally for observations, and generally as achromatic condenser. If willing to add $5 or $10, substitute Kellner's orthoscopic, by European or American makers (superb field), or Tolles' orthoscopic, by Boston Optical Works (field narrower, but peculiarly clear).

Objectives. (If extravagant anywhere, put it here.)

All above 50° angular aperture, should have adjustment for cover glass.

All above about 75° must have the same.

Screw collar.—Best.

Screw movement of outer tube.—Good for cheap lenses of small angles.

* Magnifies, diameters.

Sliding movement of outer tube, with oblique slot.—Good for cheap lenses of small angles.

One-inch.—X 10 (with lowest ocular, X about 50). Should be about 25° angular aperture.

One-fourth or one-fifth inch.—X 40 or 50 (with low ocular, X about 200 or 250). May be 75° to 85°.

One-sixth to one-eighth inch.—X 60 to 80. (With low ocular, X about 300 to 400.) Should be 120° to 140°. Best added after a few years' experience with the others.

Magnifying Powers.

Quality more than quantity.

Theoretical.—One inch X 10. Oculars and adjectives as above, X 50, 100, 200, 400, 800.

Practical.—Should approximate to this.

Accessories.

Essential, and should be furnished free.

A few glass slides and covers; stage-plates with ledge; pipettes; pliers; and needles mounted in handles.

Desirable, for medical and general use.

Compressorium, or *animalcule cage.*

Curved scissors (sideways).

Camera lucida, for drawing and micrometry.*

Stage micrometer.

Graduated draw-tube.

Cylindrical fitting, below stage, to use ocular for achromatic condenser.

Reagents. (In twelve one or two ounce vials.)

Turntable, and mounting materials.

Desirable, more for general than medical use.

Condensing lens, on separate stand, for opaque objects, and for parallel light, &c.; or *condensing lens* to limb or stage of instrument, for opaque objects; or *mirror on curved arm*, to swing above stage, for

* For occasional measurements the camera is entirely satisfactory. Where large numbers of measurements are to be made, this method is not sufficiently rapid and convenient. Dr. White's micrometer, a semicircle of thin glass, graduated at the straight edge, and lying upon the diaphragm in focus of the eye lens, is most used by the writer for this purpose. With low powers the object is easily brought up obliquely to the required position; for high powers the Jackson adjusting screw must be used, unless the stand have a mechanical stage. The simplest ocular micrometer, and best for general students' use, is a circular disk cut, to fit, from the centre of a rather thin stage-micrometer graduated to hundredths or to two-hundredths of an inch, and lying, only when used, upon the diaphragm in the ocular like Dr. White's form. This rather thick circle is more durable, more easily obtained and handled, and capable of giving unexpectedly good results. It should cost $2 or $3.

opaque objects; or *mirror removable* to stage or separate stand for opaque objects.

Spotted lens, or glass paraboloid, for translucent objects.

Stage forceps.

Maltwood finder.

PRICE.

Including walnut or mahogany case, £50 to £100. Varies with quantity and quality of work and reputation of maker.

Less, at present, unsatisfactory.

More, unnecessary.

About double, by substituting binocular stand.

TABLE OF AMERICAN

Name and Address of Maker.	Name of Instrument.	Model.	Height (in inches) with high powers.	Weight (in lbs.).	Inside diameter of tube (in inches).	Material (Brass except as stated).	Other Apparatus
Boston Optical Works.* Charles Stodder, agent, 66, Milk Street, Boston.	Tolles' Students' Microscope.	Jackson.†	15½	6	·96	Iron base and arm.	Sliding with spr...
Boston Optical Works.* Charles Stodder, agent, 66, Milk Street, Boston.	Tolles' Students' Microscope.	Jackson.†	15½	6	·96	Iron base and arm; $10 extra for all brass.	Rack ... pass.
J. Grunow, 410, Fourth Avenue, N. Y.	Students' Microscope (horse-shoe base).	Transverse bar.	12	5	1 1/16	Brass.	Sliding ...
J. Grunow, 410, Fourth Avenue, N. Y.	Students' Microscope (horse-shoe base).	Transverse bar.	12	5	1 1/16	Brass.	Sliding ...
T. H. McAllister, 49, Nassau Street, N. Y.	Students' Microscope.	Transverse bar.	12	4½	1	Iron base.	Chan... nos...
T. H. McAllister, 49, Nassau Street, N. Y.	Professional Microscope.	Transverse bar.	15	7	1 5/16	Iron base.	Chan... nos...
Wm. Y. McAllister, 728, Chestnut Street, Philad.	(No. 5374).	Transverse bar.	13	4½	1 1/16	Iron base.	Sliding ...
Wm. Y. McAllister, 728, Chestnut Street, Philad.	(No. 5374).	Transverse bar.	13	4½	1 1/16	Iron base.	Sliding ...
Miller Bros., 1223, Broadway, N. Y.	Students' Microscope.	Jackson.	16	5	1¼	Iron base and arm.	Sliding ... in ...
Miller Bros., 1223, Broadway, N. Y.	Educational Microscope.	Transverse bar.	15	4	1 5/16	Brass.	Rack ... pa...
B. Pike's Son, 518, Broadway, N. Y.	Students' Microscope.	Jackson.	15	5	1⅛	Iron base and arm.	Rack ... pa...
B. Pike's Son, 518, Broadway, N. Y.	Physicians' Microscope.	Jackson.	15	5	1⅛	Iron base.	Chan... nos...

* R. B. Tolles, superintendent.

† Arm expands to fill whole space between uprights, and gives, with conical steel pin, a very simple ... joint.

UDENTS' MICROSCOPES.

djust-nt.	Stage.	Mirror.	Oculars.	Objectives.	Range of Magnifying Powers.	Accessory Apparatus.	Price with case (in dollars).	Price of cheapest binocular stand only (in dollars).
to e.	Plain, with spring clips.	Concave, 1¼ in., with side movement.	1¼ in.	1 in. and ¼ in.	75 to 300	Mirror to separate stand for opaque illumination.	50	80, *extra* for binocular eye-piece.
with r to -piece.	Plain, with fitting for accessories.	Plane and concave, 1¼ in., with side movement.	1¼ in.	1 in. and ¼ in.	75 to 450	Mirror to separate stand for opaque ill., draw-tube, and camera lucida.	100	80, *extra* for binocular eye-piece.
w to pound y.	Plain, with spring clips.‡	Plane and concave, 1¼ in., with hinge movement.	Two.	½ in. 25°, and ⅛ in. 90°.	70 to 500	None.	85	None.§
w to pound y.	Plain, with spring clips.‡	Plane and concave, 1¼ in., with hinge movement.	Two.	½ in. 25°, and ⅛ in. 90°.	70 to 500	Camera lucida, stage micrometer, & compressorium.	100	None.§
w to ge.	Plain, with spring clips.	Plane and concave, with hinge movement.	B.	1 in. and ¼ in. (French).	50 to 400	Draw-tube and stage forceps.	50	None, at present.
ew with ver to se-piece.	Glass, sliding by hand.	Plane and concave, with hinge movement.	A. & B.	1 in., ½ in., and ¼ in.	50 to 600	Draw-tube and stage forceps.	100	None, at present.
ew with ver to mpound dy.	Glass, sliding by hand.	Concave, with hinge movement.	Two.	One set of French, dividing.	50 to 350	Condensing lens on stand.	50	None.
ew with ver to mpound dy.	Glass, sliding by hand.	Concave, with hinge movement.	Two.	1/10 in. 32°, and ¼ in. 120° (Zentmayer).	50 to 430	Condensing lens on stand, camera lucida, stage micrometer, and animalcule cage.	100	None.
ew to age.	Plain, with spring clips.	Concave, with hinge movement.	A. & B.	1 in. 16°, and ½ in. 75°.	37 to 310	Mirror to stage for opaque ill.	60	100
ew with ver to ose-piece.	Plain, with spring clips.	Concave, 1¼ in., with side movement.	A. & B.	1 in. 16°, and ½ in. 75°.	37 to 310	Stage plates and condensing lens on stand.	80	100
rew with ver to tage.	Glass, sliding by hand.	Plane and concave.	A. & B.	1 in. and ¼ in. (Gundlach).	50 to 600	Condensing lens on stand.	75	125
rew with ver to ompound ody.	Glass, sliding by hand.	Plane and concave.	A. & B.	1 in. and ⅛ in. (Gundlach).	50 to 960	Condensing lens on stand, and draw-tube.	100	125

‡ Diaphragm, sunk into upper surface of stage, so as to be close to the object slide.
§ Makes optical parts, to work with Zentmayers's binocular hospital-stand.

TABLE OF AMERICAN

Name and Address of Maker.	Name of Instrument.	Model.	Height (in inches) with high powers.	Weight (in lbs.).	Inside diameter of tube (in inches).	Material (Brass except as stated).	Coarse Adjustm
James W. Queen and Co., 924, Chestnut Street, Phil., and 535, Broadway, N. Y.	Students'† Microscope (No. 1665).	Jackson.	14	5	1⅛ (scant).	Iron base and arm.	Sliding
James W. Queen and Co., 924, Chestnut Street, Phil., and 535, Broadway, N. Y.	Students'† Microscope (No. 1665).	Jackson.	14	5	1⅛ (scant).	Iron base and arm.	Sliding
Chas. A. Spencer and Sons, Canastota, N. Y.	Students' Microscope.	Jackson.	15	6	1·18	Iron base and arm.	Sliding in rack
Chas. A. Spencer and Sons, Canastota, N. Y.	Students' Microscope.	Jackson.	15	6	1·18	Iron base and arm.	Rack a pinio
Chas. A. Spencer and Sons, Canastota, N. Y.	Standard Microscope.	Jackson.	17	11	1·18	Iron base and arm.	Rack a pinio
Wm. Wales,* Fort Lee, N. J.							
J. Zentmayer, 147, South 4th Street, Philadelphia.	Students' Microscope (round base)	Jackson.	15	5½	1⅛	Brass.	Sliding
J. Zentmayer, 147, South 4th Street, Philadelphia.	Students' Microscope (round base).	Jackson.	15	5½	1⅛	Brass.	Sliding

* Accessories of Beck's "popular" stand fits this.
† Makes optical parts only, to stands by any maker.

STUDENTS' MICROSCOPES—*continued.*

ne Adjustment.	Stage.	Mirror.	Oculars.	Objectives.	Range of Magnifying Powers.	Accessory Apparatus.	Price with case (in dollars).	Price of cheapest binocular stand only (in dollars).
crew with lever to nose-piece.	Glass, sliding by hand, with fitting for accessories.	Plane and concave, with hinge movement.	A.	$\frac{1}{2}$ in. (dividing).	100 to 300	Stage micrometer and condensing lens on stand.	60	None.
crew with lever to nose-piece.	Glass, sliding by hand, with fitting for accessories.	Plane and concave, with hinge movement.	A. & B.	1 in. 18°, and $\frac{1}{4}$ in. 80°.	50 to 500	Stage micrometer, condensing lens on stand, camera lucida, and compressorium.	100	None.
crew to stage.	Plain, with spring clips.	Plane and concave, $1\frac{1}{2}$ in., with hinge movement.	B.	1 in. 20°, and $\frac{1}{4}$ in. 60° (adj.).	85 to 325	Mirror on curved arm to swing above stage for opaque ill.	60	None.
crew with lever to nose-piece.	Plain, with spring clips.	Plane and concave, $1\frac{1}{2}$ in., with hinge movement.	B.	1 in. 20°, and $\frac{1}{4}$ in. 60° (adj.).	85 to 325	Mirror on curved arm as above, camera lucida and animalcule cage.	100	None.
crew with lever to nose-piece.	Plain, with spring clips.	Plane and concave, 2 in., with hinge movement.	B.	$\frac{1}{4}$ in. 60° (adj.).	325	Mirror on curved arm as above.	100	None.
crew to stage.	Plain, with spring clips, and tube for accessories.	Plane and concave, $1\frac{1}{2}$ in., with hinge movement.	A. & B.	$\frac{1}{10}$ in. 22°, and $\frac{1}{4}$ in. 85°.	50 to 430	Condensing lens to limb.	75	135
crew to stage.	Plain, with spring clips, and tube for accessories.	Plane and concave, $1\frac{1}{2}$ in., with hinge movement.	A. & B.	$\frac{1}{10}$ in. 22°, and $\frac{1}{4}$ in. 85°.	50 to 430	Condensing lens to limb, camera lucida, stage micrometer, & compressorium.	90	135

A Paper read before the Medical Society of New York, U.S.A.

PROGRESS OF MICROSCOPICAL SCIENCE.

American Desmids.—'Grevillea,' which is the title of the admirable little monthly record of cryptogamic botany issued by Mr. M. C. Cooke, M.A., the well-known fungologist, states that the seven new species of Desmids described by Dr. Wood in the 'Proceedings of the Academy of Natural Sciences,' at Philadelphia, are named by him—*Euastrum multilobatum*, *Euastrum ornatum*, *Arthrodesmus quadridens*, *Staurastrum minutum*, *Staurastrum Lewisii*, *Cosmarium suborbiculare*, and *Pleurotœnium breve*. Descriptions in Latin, with measurements, will be found in 'Hedwigia' for 1872, No. 1, p. 3.

The Microscopic Action of Quinine and Arsenic.—In a paper read before the Biological Section of the Academy of Natural Science, America, Dr. Joseph J. Richardson made some important remarks on the action of certain medicines in completely removing microscopic growths in the skin. He said in conclusion that it is not difficult to found a theory for explaining the probable mode of operation of quinine and arsenic—so long a problem in therapeutics—when acting as tonics upon the human system in many cases of disease: namely, that in part, at least, they serve their important purpose by rendering the blood less fit for the development of these lower organisms, and in this way *economizing* the supply of nutritive material in the circulating fluid. Many well-known peculiarities in the action of these two principal tonic medicines tend to confirm such a belief—as, for example, the facts: 1st, that quinine and arsenic, two substances which, although unlike in almost every other respect, resemble each other, as shown by the experiments of Dr. Binz of Bonn, and Dr. Dougall, of Glasgow, in being powerfully inimical to vegetable life, are in many cases our most reliable tonics. 2nd, that, as a general rule, three or four days must elapse before a decided invigorating effect is produced upon the system by these remedies—a period which, we may conclude, is requisite for sufficiently impregnating the blood (without disturbing the digestive organs) to render it an unfavourable medium for the growth of Bacteria. 3rd, that after a continuance of some weeks these roborants generally lose their invigorating power upon any particular patient, but seem to have regained it if recommenced after their administration has been for a short time interrupted—*i. e.* subsequent to the development of a new crop of Bacteria, which they again destroy. And 4th, that arsenious acid has so remarkable an effect upon the arsenic-eaters of Styria, producing such fat, vigorous, and pure-complexioned individuals when steadily continued, and giving rise to such intense suffering if omitted for a short time; results which can hardly be owing to the arsenical preparation supplying any necessary constituent of the human body, and which seem much more probably to be due to some action in preventing a waste of nutriment, and consequently of vital power.

A Minute Nostoc with Spores.—In Mr. Cooke's 'Grevillea,' July, appears an interesting paper by Mr. W. Archer, of Dublin, on

(probably) *Nostoc paludosum*. This species is a very minute one, though the dimensions of the subglobose or elliptic fronds vary much. It is rather common in moor and certain bog pools. On account of its small size, therefore, readily capable of compression, and its pellucid character, the elegant arrangement of its tortuously-twisted rather large moniliform filaments, is often nicely seen, and this causes it to be a very pretty and favourable illustrative example of its type for examination in its entirety under the higher powers of the microscope. Its minute size calls to mind *Nostoc minimum* (Currey), but in it the cells are described as quadrate with a sinus at each side, lending a crenate outline to the filaments, and the heterocysts are large, whilst here the cells are orbicular, or for a time slightly flattened at the junctions, and the heterocysts are but slightly wider, though longer than the ordinary cells. This plant is probably identical with *Nostoc paludosum* (Kütz.), though as regards anything to be deduced from the heterocysts Kützing is silent. But the interesting point connected with it is a single example of it having presented indubitable "spores," and precisely similar nature to those in Sphærozyga, &c., but with the peculiarity of these being always placed singly between two heterocysts. The pairs of heterocysts with the intervening spore occurred at just about the same intervals as in ordinary examples occur the isolated heterocysts; the spores large, broadly elliptic, about one-third longer than broad; their diameter more than twice the diameter of the heterocysts, about thrice the diameter of the ordinary cells; the "bright points" of the heterocysts not very conspicuous.

Development of the Skull of Salmo salar.—Our President, Mr. W. K. Parker, F.R.S., has just given the Royal Society a most elaborate and wonderful account of the development of the skull of this fish. We cannot be expected to do more than give an outline of the author's mass of facts so patiently and accurately worked out. His last subject, the frog, being fairly out of hand, Mr. Parker says that he set himself last summer to this "newer and more easy task—more easy by far, for the translucency of the young salmon contrasts most favourably with the obscurity of the embryo frog." He found that the two types at the time of hatching did not start fairly, but that the salmon had hastened to finish its *fourth stage* before emerging from the egg; this, however, is partly in consequence of the difference of the envelope in which the embryos are contained, for in the salmon this is a leathery "chorion," and in the frog a mere gelatinous bleb. Moreover, it soon became apparent that these two "Ichthyopsidans" are in nowise near akin to each other. In the very first stage, where there is an essential agreement, in one important particular they greatly disagree; for the embryo of the salmon has two arches in front of its mouth, while the tadpole has but one; there is also an additional gill-arch in the osseous fish. In the earliest stage of the salmon worked out by him he found a much more distinct condition of the parts than in frogs at the same stage; the differentiation of the latter is obscure as compared with the fish, and this not merely because of the quantity of *pigmentum nigrum* in the tissues of the former. Then, in addition to other causes of

obscuration, the mouth of the tadpole is strangely modified in harmony with its "suctorial" character and affinities (showing a remarkable affinity to the mouth of a lamprey), so that a whole system of cartilages has to be eliminated from the lips before the mouth (proper) can be understood. The labial system is slightly and slowly developed in the salmon, and its mouth is thus much more in harmony with that of the embryo reptile or bird than with that of the tadpole. After the simple stage is passed, the development of the facial arches is very different in the two types, as different, indeed, as in any two possible examples that could be given in the whole vertebrate group. The facial arches behind the mouth now undergo segmentation; first the hyoid, and then the mandibular. The hyoid is cloven from top to bottom, and also has a single distal piece separated off. "At this stage we get an explanation of what is seen in certain rays, where the hyoid suspensorium is permanently double; and also ascertain that this second postoral arch, which retains the anterior piece in relation to the skull as the great 'hyomandibular' pier, does not need the *axe* of the transcendentalist to put it into proper relation to its surroundings. Nature's invisible *wedge* has done what was needed, and the supposed *double rib* turns out to be *half* a visceral arch. On the whole, this second stage is extremely 'Plagiostomous,' for the details of which I must refer to the main paper." While in the egg the head of the embryo is flattened, and so twisted that one of the eyes (it may be the left or the right) looks upwards towards the "chorion," the other having a *visceral* direction. The facial bars, at first having all a simple sigmoid form, rapidly change towards the time of hatching, and, when the head gets free, the cerebral vesicles speedily swell, taking on the form so familiar to the embryologist; and the head now gains the "mesocephalic flexure." "After this an approach is made to the Teleostean type of structure; but this is not done at a stride. The intermediate condition is thoroughly 'Ganoid,' and, happily, comes in to explain the related structures of the *older* and *newer* 'Orders.' I am not aware that any stage of the heart or of the intestines shows either the many valves of the 'aortic bulb' or the intestinal spiral valve; this must be seen to; yet if these never show themselves in the 'fry' of the osseous fish, their absence does not affect the general skeletal morphology. The salmon amongst fishes, like the fowl amongst birds, never attains to the greatest degree of special class-modification; it remains *subtypical*, with a dentigerous maxillary, a ductus pneumaticus, a very *chondrosteus* state of the skull, and a very heterocercal tail. Yet, from an ichthyological point of view, this fish is an immense height above the Sharks and Rays, and is far in advance as a fish of the whole group of 'Ganoids.' The results of the *gradational* study of the fish-forms by the zoologist, and of their *secular* study by the palæontologist, are both in harmony with morphological facts. Although the light obtained is but as the first streak of dawn, yet it is a pleasant light, and quite sufficient to show each kind of worker where and how to renew his own special toil. I cannot close this brief abstract without remarking that my researches in these, the highest types of animals, seem to me to be in perfect accordance with

the results obtained by long study of the very lowest, the Rhizopods —namely, that they both yield increasing evidence in favour of the doctrine of Evolution. Researches of this kind show what the life processes can accomplish in the history of one individual animal, and also that the morphological steps and stages are not arbitrary, but take place in a manner in accordance with all that has of late been revealed to us of the gradation of types in the ages that are past."

Influence of Quinine on the White Blood Cells.—An article in a recent number of the 'Practitioner' deals with this subject. The paper is by Dr. Geltowsky, which derives additional interest from being the result of work done at the Brown Institution. The author confirms the statements of previous observers that the addition of a minute quantity of sulphate or hydrochlorate of quinine to drawn blood arrests the movements of the colourless corpuscles, either instantaneously or after a time, according to the quantity used; and he establishes that the action is due to the salt of quinine itself, not to its solvents; and that it is not shared by methyl sulphate of strychnia, urea, or chloride of sodium. Proceeding further from these data, he estimated the quantity of quinine that would be required to affect the whole mass of the blood in a given animal, and injected this quantity into a vein. In every instance either narcotism or death was produced; and the corpuscles, notwithstanding, remained wholly unaffected.

NOTES AND MEMORANDA.

Nomenclature of Objectives.—This controversy goes on still, and we are in hopes that it will prove of value to microscopists by rendering the terminology of the different countries at length identical. In the 'American Naturalist' for June, a writer signing himself C. S., who we doubt not is Mr. Charles Stodder, says of Dr. Ward's paper in the same journal that its author well says of the prevailing practice, "To call two lenses of identical magnifying power respectively one-fourth and one-sixth inch lenses, is just as indefensible as to call two houses of equal height, forty and fifty feet high respectively." An apt illustration of this is afforded by Dr. J. J. Woodward's paper in the April number of the same journal, where he cites an instrument invoiced $\frac{1}{30}$, which by actual measurement at the "open point" was only a $\frac{1}{18}$? But other objectives by the same maker called $\frac{1}{10}$, are known to be nearly as short focus as $\frac{1}{4}$ or less than $\frac{1}{8}$, showing that no system is used in the nomenclature. Other instruments from other makers have given similar results. With such discrepancies, and confusion, microscopists have nothing to depend on in ordering or comparing their instruments. The points from which the measurements are to be made is the question which is most obscure. Undoubtedly the best, and the true theoretic plan is to measure from the optical centre of the objective to the optical centre of the ocular, or the conjugate focus. This is the method of one maker of

objectives. Practically it is next to impossible to apply the rule with mathematical exactness. Mr. Cross says, "because the compound objective has no optical centre;" but this is an error; several lenses combined must have an optical centre as truly as a single lens; but the difficulty is to find its place among the lenses first, and then to find where that place is on the outside of the tube. But for high-power objectives it is so near the point between the front and middle combinations, that if that point is taken, there can be hardly an appreciable difference from the truth in the result. The optical centre of the ocular is the diaphragm, if that is in the place where it should be. But in the modern objective the optical centre is a movable point by the collar for cover adjustment. The nearer the lenses are brought together the more the combination magnifies. Shall the objective be named at least power, or some other? He says the least, because it is the fixed point, always to be found; because it represents the least the instrument can do, and the owner knows that the power increases from that point; and because it seems to have been adopted in theory by most of the best makers, as but few of their *medium* powers have ever been found "over-named." If they did not proceed on that principle, their objectives were still farther from what they were named than they have been found to be by that rule. It would certainly be desirable that makers should give the minimum as well as the maximum of focus for each objective. The practice of most makers seems to have been to make certain lenses by rule of thumb, *to be* called *e. g.* a fourth, put them together, give them the name, and sell them without any test of what their real focus should prove to be. Angular aperture varies also with the change of cover adjustment, and the practice is to give that at its maximum only; Tolles, now, however, gives and marks sometimes both maximum and minimum. Dr. Ward suggests that it should be given at the same point as the name. It is usually at the minimum there; but it may be anywhere else; Tolles can make it the maximum at open point. The question has been raised in regard to objectives with two front lenses, interchangeable, of different powers, as to what they should be called. Obviously they are practically two instruments, and should have the names proper to each front. This may be illustrated thus: Tolles will make an objective dry $\frac{1}{10}$, and to that put another front $\frac{1}{20}$ immersion, would it not be preposterous to call the performance of the immersion front that of a $\frac{1}{10}$th? His practice is to give two names; and as the originator of the plan of two fronts he may have the right to fix the rule of nomenclature.

The Biological Secretaryship of the Royal Society.—In this we of course are interested as well as the macro-biologists. The loss of Dr. Sharpey will be a very great one; for with his great and extended knowledge he possessed a most kindly and painstaking manner, which we ourselves have frequently experienced. We trust that his post may be filled by an equally worthy person, and assuredly we know of none so highly qualified for the office as Professor Huxley, whom we hope to see soon in Dr. Sharpey's place.

CORRESPONDENCE.

Brighton Meeting of British Association.*

To the Editor of the 'Monthly Microscopical Journal.'

Royal Pavilion, Brighton, *July* 19, 1872.

Sir,—I have the sanction of the General Manager of the London, Brighton, and South-Coast Railway, to state that he will be willing to extend to the members of your Society the same railway facilities as will then be given to the members of the British Association during their visit to Brighton in August next.

In a day or two you shall hear further from me.

I have the honour to be, Sir,

Very faithfully yours,

James Beck,
Secretary to Executive Committee.

Walter W. Reeves, Esq.,
Assist.-Secretary, Royal Microscopical Society.

The Measurement of Covering Glass.†

To the Editor of the 'Monthly Microscopical Journal.'

Rochefort-sur-Mer, *June* 7, 1872.

Sir,—In an article inserted in the last May number of the 'Monthly Microscopical Journal,' and entitled "Optical Curiosities of Literature," the Rev. S. Leslie Brakey points out an error in a note of mine published in August, 1871, in your estimable Journal. That note relates to the thickness of covering glass and of objects, and to the manner of measuring them. I remarked that error myself very soon afterwards; but as the measurement of thin glass and of objects was not the exclusive subject of the note, I did not think it necessary to make a correction of it, but left to each micrographist the care of detecting the error now pointed out by the Rev. L. Brakey, and which I admit in all humility. I admit in fact that the thickness of objects cannot be ascertained by a change of focus, owing to the law of the *transport des images*. The same is true respecting the thickness of thin glass, which cannot be measured by transparency, because the lower surface *appears* to rise about $\frac{1}{3}$th (?). This phenomenon occurs with every object placed under a thin plate, according to the indices of refraction of the thin glass, of the liquid, and of the object itself.

* Mr. Reeves has requested us to publish this letter, which we do with great pleasure. Mr. Beck deserves the thanks of Fellows of the Society.

† Some points in this letter are not particularly clear, but the translator is by no means to blame.—Ed. 'M. M. J.'

In any case I thank the Rev. L. Brakey for the courteous way in which he has pointed out the error committed.

As regards the application of my apparatus to the determination of vision, the chief object of my note, I think its exactness cannot be disputed, and up to the present the arrangement has appeared to me to constitute a perfect optometer.

Be pleased to receive, Sir, the assurance of my most distinguished consideration.

MOUCHET.

PROCEEDINGS OF SOCIETIES.*

BRIGHTON AND SUSSEX NATURAL HISTORY SOCIETY.

March 14th.—Ordinary Meeting. Mr. Hollis, President, in the chair. Messrs. B. Lomax, Mills, Pinker, and H. Verrall were elected ordinary members.

Mr. Wonfor announced the receipt for the Society's library of 'Catalogue of Cretaceous Fossils,' in the Brighton Museum; 'The Willett Collection,' from Mr. H. Willett; 'Geological Notices of North Hampshire,' by Dr. Stevens, from the author; 'Proceedings of the Eastbourne Natural History Society,' from the secretary; and for the Society's herbarium, from Mr. G. Davies, of a lichen new to Sussex, *Bacidia muscorum*, obtained by that gentleman a few days before at Patcham; and a moss new to Sussex, *Fissidens decipiens*, from the moist part of the Downs above Slindon. Though common in mountainous limestone districts, it had hitherto escaped detection in Sussex, being confused with *F. adiantoides*, the large lax round cells of whose leaves differed from the pale-margined, densely-areolated leaves of *F. decipiens*. This moss had been found by Mr. Davies.

Votes of thanks were given to the donors.

Mr. F. E. Sawyer then read a very interesting paper "On Rain, with special reference to the rainfall of Sussex, and how it is influenced by the South Downs."

A vote of thanks was given to Mr. Sawyer for his paper.

Mr. Sewell exhibited a series of fossils from the Whincopp collection, kindly sent down for exhibition by Mr. Charlesworth. Among the most striking were teeth of different genera of sharks, scales of fish and reptiles, coprolites, ear-bones of porpoise, dental palates and tusk of walrus.

* Secretaries of Societies will greatly oblige us by writing their report legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—ED. 'M. M. J.'

THE

MONTHLY MICROSCOPICAL JOURNAL.

SEPTEMBER 1, 1872.

I.—*On Euchlanis triquetra and E. dilatata.*

By C. T. Hudson, LL.D.

Plate XXVIII.

The genus *Euchlanis* belongs to one of the most fascinating and perplexing families of the Rotifers. The members of its various species are large, brilliantly transparent, elegant in form, and remarkable in structure; so that it is impossible to find more beautiful objects for the microscope than these living atoms. But, on the other hand, the species have been badly described and wofully confounded together; several of the genera are hopelessly involved one with another, and even the family itself is threatened with extinction by a more modern classification than that of Ehrenberg.

Indeed Ehrenberg's system has so broken down under the weight of new discoveries, that I need make no apology for setting it aside while attempting to show how the rotifers may be roughly grouped, until some really good system of classification has been hit upon.

The most obvious difference of habit among these creatures is that some spend the greater part of their lives attached to a plant or other foreign body, while the rest generally move freely about from one spot to another. This difference would at once separate the Tube-formers from the Free-swimmers.

The "*Tube-formers*" subdivide at once into two groups, *viz.* 1st, those in which the mouth is symmetrically situated with respect to the body, lying in its longitudinal axis, or the *Floscules*; and 2nd, those in which the mouth is asymmetrically placed, lying on the ventral surface, or the *Melicertans*.

The Free-swimmers break up into two well-marked groups; 1st, those that both swim and creep like a leech, or the *Philodines*; and 2nd, those that only swim; and these latter again subdivide into a loricated and an illoricated family, to wit, the *Hydatines* and *Brachioneans*.

Now, according to the above classification, Ehrenberg's family

of the *Euchlanidota* would disappear; for as all its species consist of free-swimming loricated rotifers, it would rank with the *Brachionæa.*

But the reasons that induced Ehrenberg to make a separate family of the *Euchlanidota* are worth the considering, as they will naturally lead me to speak of some of the curious peculiarities of the family as a whole, and especially of the two species of the genus *Euchlanis*, which more properly form the subject of this paper.

Ehrenberg's imperfect instrument led him to believe that he could divide the trochal disks of the rotifers into four well-marked groups, *viz.* 1st, those with a single ciliated edge; 2nd, those with many ciliated protruberances; 3rd, those with two equal ciliated circlets; and the first was again subdivided into single ciliary edges with some of the cilia wanting, and perfect single ciliary edges.

Now it may be said briefly that the whole of this scheme is worthless. The modern instruments have shown not only that the trochal disks are often of a highly-complicated structure, and that they possess subsidiary lines of minute cilia, of the existence of which Ehrenberg was quite unaware; but also that the forms of the disks pass so gradually from the one into the other that it is hopeless to attempt to ground any system of classification upon them. I may add to this also that it is the most difficult thing in the world to determine in almost any rotifer (except the Tube-formers) what is the real form of the trochal disk. In confirmation of this statement I have only to call attention to the drawings of even such excellent observers as Leydig, Cohn, and Gosse, in which very frequently a row of lines drawn at random follows the outline of the head of the rotifer without the least attempt having been made to depict the real arrangement of the cilia.

Now, according to Ehrenberg's notions, the *Euchlanidota* had the trochal disk of the *Hydatinea*, while themselves encased in loricæ like the *Brachionea:* so he formed them into an independent family, and at the same time pointed out that the great majority of the typical genus *Euchlanis* had a lorica slit longitudinally down the ventral surface.

As to the trochal disk of *Euchlanis*, I have spent no little time and patience in endeavouring to understand it, and I *think* that the disk of *Euchlanis triquetra* is correctly given at (*a*), Fig. 1, Plate XXVIII. If so, it does not much resemble that of *Hydatina*, in which there are, broadly speaking, two horse-shoe shaped rows of cilia one within the other.

If anyone would like to test at once the quality of his temper and his microscope, I strongly recommend him to study the trochal disk of one of these creatures from a living specimen, and with the full determination of thoroughly understanding it.

What a severe trial it will prove to both will be readily seen when it is understood that *Euchlanis* is incessantly in motion, now rolling over as it swims, now catching hold with its pincers, and jerking itself backwards and forwards; and that the instant it is held with the compressorium it begins to thrust its head in and out of its lorica, and continues to do so, without intermission, till it is released.

It is useless, of course, to use high powers on a rapidly-moving object, and yet with powers less than $\frac{1}{8}$ inch very little can be made out. The most successful method of attacking the puzzle is the employing of the dark-field illumination, especially that with Wenham's paraboloid.

It will be seen from Fig. 1 that if I have rightly interpreted the disk, the cilia on *Euchlanis triquetra* are divided into several groups, two at the sides, like whiskers (*b*, *b*), two parallel rows (*c*, *c*) running down to the mastax (*d*), and performing the office of conducting to the mouth and jaws the food brought by the coarser cilia.

There is another row (*e*) along the forehead, while from the disk itself spring up four papillæ (*f*, *f*), (*g*, *g*), the former of which have curved motionless setæ on them, while the latter (*g*, *g*) are evidently perforated tubes, and probably contain some tactile organ, which, however, I have never seen protruded. There is an obvious tactile organ (*h*) on the medial dorsal line and just above the disk.

Before I describe the internal organs I will take the lorica, which Ehrenberg affirms to be slit underneath longitudinally right down the middle of the ventral surface. Here I am quite positive that the great naturalist is in error; the peculiarity he describes does not exist, while he omits one quite as striking.

If *Euchlanis triquetra* be carefully watched with the dark-field illumination, the flashing of the light from its extremely transparent but highly reflecting lorica will reveal its true form. It will be best understood from Fig. 2, which is a cross-section of it through the middle of the back, *viz.* at the point (*k*) in Fig. 1.

The lorica is something like a glass dish-cover, rising into a very high ridge, and placed on a dish which it overlaps.

Fig. 2 shows the upper portion or cover (*a*) and the lower flat plate or dish (*b*), with a connecting incurved rim (*c*) lying between; though whether this portion be chitinous, like the lorica itself, or only a soft membrane, I have been unable to discover. That there is a connection may be easily shown by drying the animal on a slip of glass, when on again dropping water on it, the connecting portion may be seen to bulge out.

Fig. 3 is a similar cross-section of *Euchlanis dilatata*; which animal also, in my opinion, has no medial division of the under portion of the lorica: it *has*, however, a raised portion (*a*, *a*), the

edges of which, when the animal is held by the compressorium, give rise to two sharp lines, which appear to be the bounding lines of the under surface of the lorica, and exactly give the impression of there being a naked gap at (*b*), leaving the body unprotected. This is better seen in Fig. 4.

To return to *Euchlanis triquetra*. Of its internal organs, its enormous brain (*l*) claims the first notice. Anyone unacquainted with the subject may well smile at the idea of crediting such an atom with such a brain; but the reasons which lead those who study the rotifers to say that the organ is a nervous centre are, I think, convincing. To lessen the marvel I may mention that the contents of the brain sac (*l*) seem to be of a very fluid and transparent nature, with the exception of the hinder portion (*m*), which appears to be nucleated and more dense.

Immediately under the brain is the red eye-speck (*n*) and the mastax (*d*), while above it and close to it is the tactile organ (*h*); this arrangement is pretty general among the rotifers.

The passage through the mastax leads by a short tube to the stomach (*o*), which is generally dotted over with oil-globules, and often in quicunx fashion.

Two muscles (*p, p*) cross the surface of the stomach, imbedded in its surface, and constantly twitching it from side to side. The stomach is usually subdivided into an upper and lower portion, as in the figure; the lower portion being densely ciliated. But this subdivision is not permanent in the same individual.

There are the usual convoluted tubes (*q*) on either side of the body, bearing on an average four or five vibratile tags on each side; and at (*r*) is the contractile vesicle, into which the tubes open, and from which a discharge is kept up every ten or twelve seconds into the vent.

The ovary (*s*), containing the embryos (*t, t*) and the full-developed egg (*u*), opens also into the same place.

I have only to add that the pincers (*v, v*) exude, apparently, a viscid substance, like that of a spider's web, by means of which the animal adheres to any substance it pleases. I have never seen them *pinch* anything, but I have seen them adhere by their extreme tips to the glass; and I think I have on more than one occasion seen traces of this viscid secretion on the glass itself: the power of secreting such a fluid is, I believe, common to the majority of the rotifers.

The figure of *Euchlanis dilatata* (Fig. 5) is so like that of *E. triquetra*, that it requires no further explanation.

II.—*On some Methods for preparing the Tissues of the Frog Tadpole's Tail.*

By R. L. Maddox, M.D., Hon. Fel. R.M.S.

Of the various methods employed by me lately to try and render more or less transparent some of the tissues of the common frog tadpole's tail without inducing a too great granularity, and yet give distinct views of the relations and paths of the different sets of nerves, cutaneous and deeper seated, I beg to offer the following, thinking the plans may be of interest to readers of the Journal, and believing they embrace some novelty in the treatment. The tissues are not stripped, hence the relations are not destroyed; but the epidermis is removed so as to permit a clearer view into the thin parts of the tail, in fact completely through the textures. It is not worth while to do more than briefly recount some of the numerous methods, and those which have yielded excellent results, more in detail.

Methods 1, 2, and 3.—After removing the epidermis, as described farther on, chloride of gold and chloride of cobalt solutions mixed in various strengths and various ways were tried.

Methods No. 4 and 5.—Chromic acid solution followed by tincture of logwood and chloride of zinc solution; also the chloride of zinc replaced by chloride of tin solution (these two reagents having been employed by me in 1868, when working on the structure of the papillæ of the frog's tongue, and found to beautifully stain the nerves of a blue or blue-violet colour); and these plans used in different ways more or less successfully on the tadpole's tail, but did not equal the methods following.

No. 6 Plan.—Living specimens, also tadpoles that had been dead at least for twelve hours, were placed in chromic acid solution, 3 drams (strength ¼ per cent.) with 20 drops of spirit of sweet nitre, for five minutes or a little longer; then washed in pure water thrice, changed for ten minutes; now immersed in a few drams of ammoniated water (strength 4 drops of the strong liquor of ammonia to 3 drams of pure water) for about three minutes or until the epidermic epithelium was softened into a sort of mucoid film, then *quickly* removed into pure water in a Wedgwood evaporating dish, and gently brushed under water with a soft long camel-hair pencil; now rewashed for ten minutes, changing the water three or four times, and replaced in fresh chromic acid and sweet nitre mixture for twenty minutes; then rewashed in several changes of pure water for ten minutes, and placed in diluted tincture of logwood for five minutes (strong tincture diluted with pure water until of a golden or sherry colour); again well washed and

placed for three minutes in the usual iron developer with acetic acid employed for developing photographic negatives; again washed with several changes of water, and mounted in a nearly saturated solution of acetate of potash slightly acid with acetic acid.

This plan furnished beautiful results. The reason why the iron developer was used arose from finding that if the specimens after the logwood tincture were washed with our ordinary well water, which contains a little iron, they were greyer in tint than if washed in pure water.

The same plan was modified by placing twice alternately after removal of the epithelium, into the chromic acid and sweet nitre mixture and diluted logwood tincture, without using the iron developer, and which gave fair specimens.

No. 7 Method.—The same as No. 6 plan, only keeping the tadpoles in the chromic acid and sweet nitre mixture for forty minutes before removing the epithelium by the ammoniated water, and after its final removal very gently by the brush, rewashing and placing the specimens in the tincture of logwood first for five minutes; then well washing and replacing in fresh chromic acid and sweet nitre for ten minutes; again washing and mounting in the acetate of potash, as above. The specimens did not quite equal No. 6 plan.

No. 8 Method.—Living tadpoles were placed for fifteen minutes in the chromic acid mixture, and well washed; then the epithelium softened by the ammoniated water and removed under pure water, as above described; again well washed and placed in "Mordan's purple ink" diluted with two-thirds water for ten minutes (ink supposed to be made from aniline), well washed in water weakly acidified with acetic acid and mounted in acetate potash solution.

This also gave beautiful preparations.

The attempt to hurry the effects of the reagents by increasing their strength and diminishing the times did not yield equal results. The mixtures and the ammoniated water to be used fresh each time. Dead specimens, in which the fine nerve threads might be supposed to have considerably changed, did not under the microscope appear to have suffered in any way, save that the epithelium could be removed after a very *brief immersion* with ammoniated water.

This plan may be found useful for some living structures, or such as are only recently dead. Instead of stripping the sides from each other, the central muscular portion of the tail, snipped off *after preparation*, is carefully removed by scissors under water, then the thin portions lifted out on a thin silver spatula, drained from the edge by blotting-paper, and gently pushed off into the mounting medium on the slide. If steel instruments be used they very quickly tarnish by the acetate potash and acetic acid, get slightly roughened, the delicate specimens cling to the surfaces, often become torn or doubled over, and thus they are somewhat difficult

to spread out again. Glycerine was tried for the mounting medium, but the appearances were more natural in the acetate potash medium. More than a hundred specimens were treated in various ways, too long to enumerate here, the preference being given to the plans fully detailed.

III.—*On the Employment of Coloured Glass in Microscopy.*

By M. Mouchet.

I believe that the use of coloured glasses in microscopical studies may facilitate the examination of many objects which would otherwise remain drowned in a flood of light; I believe also that the use of them must remarkably spare the eyesight. It is many years since Ch. Chevalier first employed tinted glasses, which he placed above his lenses in order to temper the intensity of the light. By the use of those glasses, or of silvered glass, sunlight may be made use of, and may thus replace all the other systems of illumination which are intended to supply the brightness that powerful objectives tend to destroy. Now, what position should these different glasses occupy? We can count four: before the mirror; under the object; between the objective and the eye-piece; finally, above the eye-piece itself. To place them before the mirror necessitates the employment of glasses of a certain size; to place them under the object increases the thickness of the plate and prevents a very oblique light from passing through; to fix them in the body of the instrument causes trouble and a loss of time, for the microscope must be taken asunder. Above the eye-piece appears to me the most suitable place. For this purpose, I have constructed an apparatus, consisting of a kind of revolving diaphragm pierced by nine openings, each one centimètre in diameter, and placed exactly opposite the eye-piece. Seven of these openings are fitted with coloured glasses corresponding to the colours of the solar spectrum; another with a silvered glass, according to Foucault's process; and the ninth with a very finely roughed glass, which may be removed at will, and thus leave one opening free. This revolving diaphragm is attached to a ring placed on the tube which receives the eye-piece, and is fixed to the tube by means of a *vis de serrage*, as in certain "*chambres claires*" (camerælucidæ ?). The apparatus has some analogy to that which Mr. Collins calls "light corrector," and which he places on the stage of the microscope.

It is evident that when coloured glass, as, for example, cobalt blue glass, is used, we have not a pure monochromatic light; but this glass replaces, if not with advantage, at least without in-

convenience, the vessel of ammoniacal sulphate of copper patronized by Count Castracane.

At first, I wished to construct, without any modification, Mr. Collins's accessory plate, which for the optical effect may be placed in the microscope more advantageously than my apparatus; but to keep the chief plate of the least possible thickness is so powerful a consideration, that I decided on adopting the plan which I have just described.

In order to substitute one glass for another when examining an object, I have no need, as with Mr. Collins's arrangement, to change my position; and I can observe, without losing sight of the object, what is the most favourable colour for the perception of certain details peculiar to it.

However, I am not yet sufficiently acquainted with the advantages to be derived from those glasses. They will, perhaps, become useful auxiliaries to micrographists, by enabling them to make more frequent use of solar light, without experiencing the least fatigue.

IV.—*Note on Silicious Incrustations from the Geysers of the Yellowstone River, Wyoming Territory.*

By Prof. Arthur Mead Edwards, U.S.A.

The specimens are of two characters. First, we have a mass of white stony material, arborescent in appearance and looking as if it were some species of vegetation, as a moss, converted into a mineral. The others are rounded masses, smooth on the exterior, white, and evidently consisting of the same substance as the arborescent specimen. These were given to me by Mr. P. T. Austen, of the School of Mines, who has made a chemical analysis of them with the following result:—

SiO_2	87·23
Fe_2O_3 and Al_2O_3	1·13
CaO	·55
MgO	·46
PO_5	1·48
HO	9·09
	99·94

For my own part, being interested in the subject of the mode of formation of concretions of the character of these rounded masses, I determined to sectionize one of them and examine it by means of the microscope, to ascertain, if possible, what kind of nucleus it had. This matter is one of importance on account of its bearing upon the

mode of formation of pearls and the rounded masses which, united together, constitute the dentine of teeth, and which in that substance are often so loosely adherent one to the other as to leave cavities known as "interglobular spaces," and the existence of which is by some observers denied, the appearance of them in tooth sections being considered "optical illusions." I now exhibit one of these silicious balls thus sectionized or cut down to a thin slice which runs directly through the centre of the mass, and viewing it by means of a pocket-lens will show plainly how it has been formed. At first, the silicious matter has been deposited from solution upon the sides of the geyser in the vegetation, like masses shown. Although they seem pretty strong, yet they are liable, on account of their porous nature, to be broken so that at some time a small mass has been broken off and rolled and tossed about in the basin until it has gradually come to be covered over with a smooth coating of the same material. Thereafter it has rested for awhile somewhere within the basin, so that it has been again encrusted with rough material, and dropping down into the tube of the geyser, has again been thrown about until a smooth coat has formed over the rough one, and so on alternately rough and smooth coats have been deposited. It will be seen that the specimen I exhibit has several of these alternate compact and porous coatings, so that to the naked eye it almost seems to have an organic structure.

These specimens were brought home by the exploring party under the command of Dr. F. V. Hayden, United States' geologist, who has for some time been engaged in the examination of the Yellowstone Park, in Wyoming Territory, under the direction of the Department of the Interior; the survey being known as the United States' survey of the Territories. Dr. Hayden has brought home many specimens of the Diatomaceæ from the geysers, as well as several earths containing the silicious remains of those organisms, and these he has put into my hands for examination and report. Further collections will be made, and the results of the examinations published hereafter. In the meantime, Dr. Hayden has kindly consented to my making known here and elsewhere the results of my examinations, which I shall do from time to time. —*Read before the Lyceum of Natural History, New York, March* 25, 1872. Sent to 'M. M. J.' by the author.

V.—*On Angular Aperture of Immersion Objectives.*

By Robert B. Tolles, Boston, U.S.A.

The following table gives measurements of angle of aperture for several objectives—taken indiscriminately, not selected for the purpose—in balsam, in water, and in air. Several of them were made years since :—

Objectives.	Angle in Air.	Angle in Water.	Angle in Balsam.	
1/8″ Single Front	170°	100°	81° (+)	Sun.
1/8 Single Front	170°	120°	87°	„
1/8 Single Front	170° (+)	110°	83°	„
1/8 Compound Front	147°	97°	82½°	„
1/10 Compound Front	175°	117°	93° (+)	„
1/16 Single Front	175° (+)	113°	95°	Lamp.
¼ Compound Front	178°	120°	97°	Sun.
¼ Compound Front	155°	106°	88°	„
¼ Four Systems	175°	127°	110°	„

This table of measurements of angle in balsam, &c., furnishes proof enough of the correctness of my position so positively denied by Mr. Wenham, first in 'Mon. Mic. Jour.,' No. xxxii., p. 84; again, *ibid.*, No. xxxvi., p. 292; and thirdly, No. xl., p. 272, of the same. The criticism bestowed in these articles induced the measurements. Still, if nothing of attempt to deny the significance of that first experiment as described in this Journal, No. xxxii., p. 36, had appeared, the above results of actual measurements would have been interesting as data of practical angle of "immersion" objectives when water or any medium of ref. index not higher than 1·549 that of balsam is used. But *somebody is mistaken*, and I must necessarily be "personal" in pointing out how I am not, and others are, in respect to what I have set forth on angular aperture. In the first place all agree that in the case of a balsam-mounted or dry-mounted object-slide, if of common glass in use, the light incident at the first outside surface of that slide at the extremest obliquity will be so bent in entering the glass as to make the angle of the pencil after this first refraction less than 82°, the extreme rays of the pencil having approached the glass at an incidence of more than 89°, and the pencil angle more than 178°. Not to

include the case of a slide of dry-mounted objects, it is safe to state that with whatever kind of *objective* used, when the slide and covering glass are by balsam between fused into one (nearly) homogeneous transparent piece, this first refraction limits the angle of transmitted pencil to less than 82°. The barrier evidently is at the first refracting surface of the slide. The little (or large) hemispherical lens cemented to the outer surface of the slide * gives the case I first gave substantially. It is evident that as the light, say a ray for simplicity, is carried from the axis round to the extremest obliquity, the path of the ray will be a straight line to the centre of curvature of the plano-convex lens. This figure illustrates plainly that the *hemispherical* lens gives access for the largest angular pencil to the balsam-mounted object, say 179°. Here the lens is not fully a hemisphere, but *nearly* of the requisite thickness. The diagram previously given † shows the full pencil admitted.

In giving my first note and illustration I assumed as evident enough from what *had already appeared* that with a balsam-mounted object-slide an immersion objective would have more angle for rays *radiant from the object* in the balsam than a dry objective could have. But it is denied. And yet it is safe to say that with *air* between the immersion-front and slide-cover, the latter being of glass commonly used, the "well-settled laws of refraction" limit the pencil, *emergent*, at the outside surface of the "cover" to 82°,—*within* the glass, *before* emergence; but with water between the cover and front surface of the immersion objective it would be *larger* according to the difference between refraction of covering-glass in water and covering-glass in air. Whether the immersion objective would be competent to transmit the whole angular pencil or not is another question. That it could be made to do so did not seem to me to admit of doubt. Mr. Wenham's $\frac{1}{12}$" 170° air angle, 100° water angle, probably would not transmit or "collect" to exceed 82°. "Mr. Tolles is" not "unable to perceive this." All of my own save one, so far as tried, *do* take up more than 82° when immersed in balsam. That exceptional one has the same angle in air and in water as given by him as pertaining to his $\frac{1}{12}$ in. When I "seized" upon his own experiment as proof that my experiment was valid, it was only because his result agreed exactly with my result—objective 170° in air, 100° in water. *All* that his or my experiment proved was that more than 82° could *get out* of the balsam through the cover into *water*, unless I include that my plan showed that in air all but 82° of pencil was *turned back*, which his did not! Now again, "100° is surely more than 82°"! Perhaps Mr. Wenham "is unable to perceive this, and so let it remain."

* See 'M. M. J.,' No. xxxix., p. 117, Fig. 3. † 'M. M. J.' for Nov. 1871.

For persons not "familiar with optical science," and for others, Mr. Wenham's tank method furnishes a ready means of *test* of angle in balsam, thus ascertaining the degree of fitness of any objective for viewing objects balsam-mounted. It proves, moreover, by simple experiment what many persons would not take the trouble to study out from a diagram and description, and the *experiment* is conclusive, at all events as to the point at issue between Mr. Wenham and myself. The measurements given at the head of this article for angle of objectives in water and in balsam were made strictly according to that tank method as contributed by him, only sunlight was used in most cases. The differences in the results are plainly referable to differences in the objectives as to plan of construction. There is no need to set forth the manner in which the result is produced in one case or another; the fact remains of comparatively large angle for immersion objectives over what is possible for "dry." In proof that image-forming rays were concerned, I name the fact that every objective recorded in the table *readily shows the true test, A. pellucida, by uncondensed light of a common petroleum lamp flame.* In this connection let it be noted that most of the objectives in the list have the *compound front* of crown and flint glass, while some of them of equal angle have the single lens front.

The $\frac{1}{5}$" of 110° *in balsam* has four systems substantially on the plan figured in 'Mon. Mic. Jour.,' No. xxxix., p. 127, Fig. 1. It is easy to carry the angle on this plan much beyond 110°, but that breadth of angle, anything above 82° of course, is enough to dispose effectually of this extraordinary assumption and *defy*. "I challenge . . . . . anyone to get, through the object-glass with immersion front, a greater angle, or any portion of the extraneous rays that would in the other case be totally reflected, as no object-glass can collect image-forming rays beyond that limit."*

One word about Fig. 1 (dotted lines) and Fig. 2, p. 117, No. xxxix., this Journal. Mr. Wenham says they are grossly erroneous. He is at fault. In the text, this in qualification and explanation, is stated as follows:—"What is intended is to increase the refraction of the front by sharper convexity or higher refractive material or *both* to the extent necessary to make up for the diminution at the front surface which takes place according to the refractive power of the medium flowed in" between the front and covering glass. Higher refractive material involves *refraction* at the front plane surface, of *course and necessity*.

As to the practicability of the *first case* illustrated by the same figure, involving *four* systems in the objective, that seems to be admitted. It is not denied. I understand Mr. Wenham to admit it, in effect, in two of his three replies. The $\frac{1}{5}$" in the table, of

* Mr. Wenham, in 'M. M. J.,' No. xxvii., p. 118.

balsam angle 110°, is, as before stated, made on that plan, but not strictly according to what the figure represents, for *that* is an *extreme* case, to help apprehension, and the refraction is all accomplished by the three inner systems. In this $\frac{1}{5}$″ (of the table) the front lens is *thin* and has *considerable* action to bend the rays towards the axis, and the objective has a good range of working distance. To get the highest *practical* balsam-angle on this plan, of course the action of the two lenses at the front of the objective is so apportioned between the two as to favour convenience in use as well as efficiency. In this diagram, moreover, the three inner systems are identical with those of Mr. Wenham's $\frac{1}{8}$, as he represented them, and I thus limited the case, and *all the question*, to the added fourth lens. Therein was the reason his diagram was copied. *That part* he would not impeach. *I never used* such a set of combinations. Would not select it as choice for any purpose.

In conclusion, one point may as well be noted, as of some practical importance, *viz.* that with a *water* immersion objective in use, a water immersion condenser of the same balsam angle will give closely the full dimension of *illuminating pencil* that *can* reach the water-immersed objective above, and be transmitted by it. About 125° balsam angle, for the condenser would cover *all* cases of use of *water* immersion objectives. I pointedly suggested this in my first communication.

Boston, *July* 23, 1872.

VI.—*The Minute Anatomy of Two Cases of Cancer.*

By Assist.-Surgeon J. J. Woodward.

Plate XXIX., from the Author's Photographs.

Opportunities for the study of the histology of morbid growths frequently occur at the Army Medical Museum. Specimens are sent, not merely by medical officers of the army, but also by civil practitioners. It is often possible to examine these growths while quite fresh, and thus to make observations which aid in the interpretation of the appearances found in those which are not received until they have been hardened by immersion in alcohol or other reagents. By methods described in former reports, it is possible to preserve thin sections of very many of these growths in a permanent form for examination with the microscope, and a large collection of such preparations has already been formed.

These preparations are frequently studied by medical officers

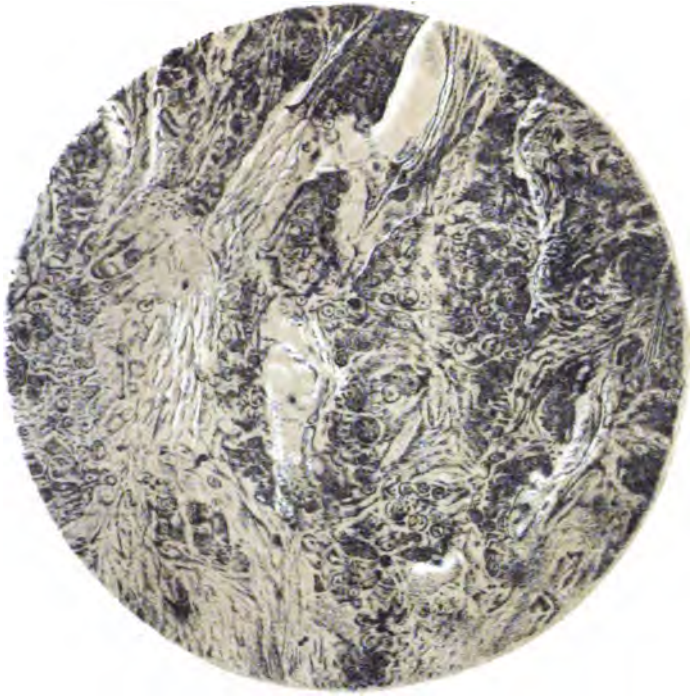
and others who visit the Museum for that purpose, but they would be rendered more generally useful if it were practicable to publish from time to time brief descriptions of the more important cases, accompanied by suitable illustrations.

The usual methods of representing such microscopical preparations by etchings on copper, lithographs, or woodcuts, are open to many objections. Especially it may be indicated that the production of the drawings which must serve as the basis of such representations is laborious, and consumes too much valuable time if the investigator himself endeavours to make them; and if the work is entrusted to a professional artist the essential features of the image are very generally imperfectly shown, or altogether misrepresent the original. Even if the microscopist himself has the leisure and the skill to make his own drawings, the finished work will too often represent rather his theoretical views than the severe facts of nature, for in any case the artist finds it impossible to represent all that he sees in the microscope, and only attempts to reproduce so much as he conceives to be of importance.

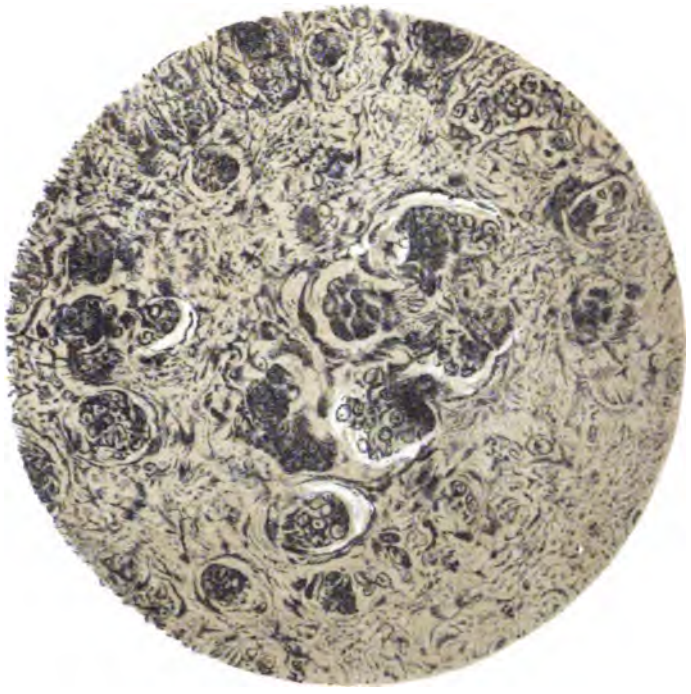
Much more truthful representations can now be made by the process of Photo-micrography as improved by my experiments; but the cost of making silver prints, the fact that they are not permanent, and the circumstance that they require to be mounted on tolerably stiff cardboard, prevent them from being generally available as book illustrations, especially where editions of several hundred copies or more are desired. Under these circumstances I note with pleasure the rapid improvements lately made in the direction of photo-mechanical processes of various kinds, having for their object the reproduction of photographic negatives in inks of a permanent character. These processes are indeed as yet far from perfect, but several of them have already reached such a degree of excellence as to be very suitable for the reproduction of photographic negatives of microscopical images.

In the United States two such methods may be said to be in successful operation, *viz.* the Woodbury process, and the Albertype process. A detailed account of these processes would be out of place here, but a few brief remarks are required.

The Woodbury method consists essentially in the production from the negative, by the action of light, of a relief surface of gelatine, from which a metal "intaglio" is produced by pressure. In this a series of gelatine films coloured by any suitable permanent pigment are formed by mechanical means, and these constitute the prints. Like silver pictures the Woodbury prints require to be mounted on sheets of cardboard or stiff paper, and this of course adds to the expense. Moreover, if the mount is thick it is ill adapted to binding. Of late, however, films of great flexibility have been produced, which are supported on mounts not much



Mammary Cancer.



Cancer of the Liver.

The figures are reduced [illegible] from [illegible]

stiffer than ordinary plate paper, and therefore quite suitable for book purposes.

In the United States this process is employed by the American Photo-relief Printing Company, No. 1002, Arch Street, Philadelphia, under the supervision of Mr. John Carbutt, who has reproduced three of my negatives of microscopic objects, *viz.* a photograph of a section of ovary magnified 400 diameters, of which five hundred copies were printed, mounted on stiff cardboard, and used among the illustrations of my report on photographing histological preparations by sunlight; a photograph of *Amphipleura pellucida*, in illustration of a paper published in the 'American Naturalist' for April, 1872, and a photograph of two frustules of *Triceratium fimbriatum*, in illustration of a paper published in the Chicago 'Lens' for April, 1872. Large editions of the last two prints were struck off for the journals named. They were mounted on plate paper.

In quality these prints were almost fully equal to fine silver prints, which they closely resembled, but the editions were much more uniform than would have been possible had silver prints been used. The cost, moreover, was considerably less than that of silver prints, and the negatives were returned to me wholly uninjured, while they would have been much damaged if not quite destroyed had an equal number of silver prints been made from them.

The Albertype process is employed in New York by the Photo-Plate Printing Company, under the supervision of Mr. E. Bierstadt, No. 932, Broadway. In this process a printing surface (not a relief) is produced on a gelatine film by the action of light, through the negative, on certain chemicals contained in the film. The surface thus produced, when properly inked, yields in the press an impression on paper, in which the details of the original negative are very well preserved. The prints may be made on either plain or enamelled paper; in the latter case they closely resemble silver prints on albumen paper, in the former they are like silver prints on plain paper. In either case they give a clean white margin, and therefore do not require to be mounted as Woodbury prints do, but are at once ready for binding. Moreover, proper descriptive lettering is readily added to the plate. If suitable ink is employed the prints are as permanent as lithographs or other engravings.

Two of my negatives were placed in Mr. Bierstadt's hands for reproduction, and the illustrations of the present paper are the results. These plates very fairly represent the original negatives. One of them is on plain, one on enamelled paper. The latter more closely resembles an albumen print from the negative than the former does, but the enamelled paper has a certain glare, and I observe that it easily cracks if crumpled. It is a question how

far these disadvantages counterbalance its advantages. I place the two prints side by side, and should be glad to hear comments on their respective merits.

I may add that the prints of the edition of these two plates are not so uniform as the Woodbury prints above alluded to. Some are much darker, some much lighter than the standard desired. I cannot say, however, that this irregularity is as great as is usually the case with silver prints. In cost the Albertype prints, paper included, were rather cheaper than the unmounted Woodbury prints.

In selecting a subject for this report, which it is to be hoped may be regarded as the first of a series of short essays on morbid growths, I determined upon a case of multiple carcinoma, consecutive to the ablation of a primary cancer of the female breast. With this I have associated a brief description of a primary mammary tumour of similar structure.

The microscopical anatomy of these growths appeared of interest in connection with the recent study of Koester on the development of carcinoma and sarcoma.*

Koester has been led to the opinion that carcinomatous growths very frequently have their origin in a proliferation of the endothelium of the lymphatics of the parts involved, and that this transformation plays probably a great part in the development of most cancers and sarcomas. His work, so far as published, is confined to the examination of epithelial cancer of the skin, and alveolar *colloid* of the stomach (alveolarer Gallertkrebs des Magens), and he has postponed for future parts the detailed examination of other varieties. His views have been received with much favour in various quarters; I may especially mention Rindfleisch, as having given prominence to them in the last edition of his text-book.† I shall have occasion to refer further to the views of Koester in the sequel.

The following is briefly the history of the case selected for consideration:—

Case. Martha J. H——, a widow, forty-three years old, who had resided for twenty years in the District of Columbia, most of the time in East and South Washington, where she had frequently suffered from intermittent fever, first noticed a tumour in her right breast in June, 1865. The whole mammary gland was removed November 23, 1866, by Professor Johnson Eliot. The tumour, which had attained the size of a hen's egg, was hard, uneven, and somewhat painful. No microscopical examination was made.

The patient speedily recovered from the operation, but never

* 'Die Entwicklung der Carcinome und Sarcome,' von Dr. Karl Koester Erste Abtheilung. Wurzburg, 1869.

† 'A Text-book of Pathological Histology.' By Dr. Edward Rindfleisch. American translation. Philadelphia, Lindsay and Blakiston, 1872.

regained full constitutional vigour. In March, 1867, her health began to give way, and during April her strength failed rapidly, so that she was confined to her bed. Loss of appetite and vomiting were prominent symptoms.

After a time there was some temporary improvement, the vomiting ceased and her appetite returned, becoming in fact excessive. A tumour now appeared in the left breast, and the attention of her medical attendant was drawn to a decided enlargement of the spleen. No great pain was complained of, but the patient emaciated constantly, lost strength progressively, and died March 22, 1868, the tumour in the left breast not having yet ulcerated.

Autopsy by Dr. D. S. Lamb. The body was extremely emaciated. There was a small, hard, nodulated tumour of the left breast. The lymphatic glands of the axilla, neck, and groin were enlarged and indurated. The thoracic and abdominal cavities were filled with straw-coloured serum. The lungs were coated with lymph, but there were no adhesions. The lower lobe of the left lung was congested and somewhat œdematous. There was some little serum in the pericardium. The heart and pericardium, liver, spleen, and internal organs of generation were removed, together with the tumour of the breast, and sent to the Army Medical Museum for study, by Dr. George Rice, the attending physician.

These specimens are preserved in the Medical Section. The following is a description of their characters:—

No. 955, Medical Section. The left mammary gland, which is transformed into an oval, flat, somewhat nodulated scirrhous tumour, three and a half inches long by two and a half broad, and about an inch thick in the centre.

In thin sections of this tumour individual lacteal alveoli and ducts, stuffed with large cells, derived evidently from the glandular epithelium, were but sparsely scattered, and the greater portion of the gland was replaced by a peculiar scirrhous tissue, in which its scanty atrophied remains were imbedded. This peculiar tissue consisted of a firm, delicately-fibrillated connective-tissue stroma, containing numerous small cells and many fine elastic fibres, and of a plexus of irregular, varicose, nucleated cylinders of protoplasm, corresponding in many respects to the "cell cylinders" of Koester, which intimately interlaced with the connective-tissue stroma, filling all its meshes except those occupied by the vessels and the atrophied remainder of the gland. These nucleated cylinders did not appear to be composed of separate cells. They seemed rather to consist of a granular protoplasm, in which innumerable nuclei, $\frac{1}{2500}$th to $\frac{1}{2000}$th of an inch in long diameter, or even larger, were imbedded side by side, without any evidence of limiting cell-walls separating the protoplasm surrounding each nucleus from that belonging to its neighbours. On scraping or teasing the sections, moreover, the

nuclei escaped abundantly into the "juice" in a quite naked condition. Very often, however, some of the nuclei appeared in the "juice" thus obtained surrounded by a more or less irregular mass of protoplasm, resembling thus "the typical cancer cells" of the older writers.

The nucleated cylinders usually varied from the $\frac{1}{1000}$th to the $\frac{1}{500}$th of an inch in diameter, but in some parts attained still greater dimensions. In places where the section passed transversely through the cylinders they appeared as round or somewhat oval masses of protoplasm, stuffed with nuclei, which might readily pass for the "mother-cells" of the books. In glycerine and balsam preparations the protoplasm of the cylinders was often shrunk away from the connective-tissue stroma, the margin of which being clear and transparent might almost be taken for a limiting membrane.

The nuclei of the cylinders were transparent or but slightly granular when fresh, and usually contained a single large nucleolus. In the balsam-mounted sections they appeared more decidedly granular. Elongated nuclei with two nucleoli and other indications of commencing division were but rarely observed. The nuclei of the connective-tissue stroma were generally rather longer than those of the cylinders, but much narrower. Occasionally their position was occupied by little rows of two, four, or more nuclei, which might be supposed to be the progeny of the connective-tissue nuclei. Such rows were generally surrounded by a small quantity of protoplasm.

Sections taken from the peripheral portions of the tumour generally presented a certain amount of more or less altered adipose tissue. The quantity of the matrix between the fat-cells was much increased, so that instead of the individual cells of each fat-lobule being apparently in contact, they were separated to a greater or less extent (often the $\frac{1}{100}$th of an inch or more), by a delicately-fibrillated matrix, in which were imbedded many oval nuclei which were both larger and more numerous than normal. The connective-tissue septæ between the individual fat-lobules were also greatly thickened, and in many places contained cell cylinders quite like those of the body growth. (See Microscopical Section, Nos. 4615 to 4631.)

No. 958, Medical Section. A portion of the right lobe of the liver. The liver was nearly normal in size, but presented on section numerous little hard whitish nodules, varying from the size of a pin-head to that of a pea, or even smaller. These were invariably situated in the course of the portal vessels, *i. e.* in the interlobular spaces. In structure they closely approximated the scirrhous tissue of the breast tumour as above described, simply the meshes of the plexus of nucleated cylinders were less elongated, and in the sections, therefore,

these more frequently appeared as round or oval forms (the cylinders having been cut more or less obliquely), and less frequently as continuous cylinders of some length, such as were common in the breast sections. Hence, on superficial observation the sections of these cancer nodules of the liver appeared at first sight as a nucleated connective-tissue stroma, in which numerous large, round, oval, or elliptical mother-cells, containing numerous nuclei, were imbedded. On carefully focussing, however, it was easy to perceive that these apparently separate masses of protoplasm formed in fact portions of a complete network of nucleated cylinders, which had been divided by the section. As in the case of the mammary tumour, the cylinders appeared to consist of a mass of granular protoplasm with numerous nuclei imbedded, actual cell walls being nowhere distinguishable.

Nowhere did it appear that the hepatic cells had contributed by their multiplication to the formation of the cancer nodules; on the contrary, the actual structure seemed quite clearly to contradict any such inference. For some little distance around each little cancer nodule the cut end of single nucleated cylinders were frequently observed imbedded in the very substance of the hepatic acini. They always occupied, however, the vascular spaces or the meshes of the network of hepatic cells, and these appeared quite normal except in the immediate vicinity of the cancer nodules, where the chains of hepatic cells were often flattened somewhat, as though by the pressure of the growth. At the margins of the growths, in many places, nearly all the meshes formed by the compressed hepatic cell-chains were occupied by the nucleated cell cylinders of the cancer, the atrophied secreting tissue of the liver here taking a position corresponding to that of the connective-tissue stroma of the more central portion of the cancer nodules. The cancer nodules then would appear to have grown by a continuous extension of their nucleated cylinders into the adjacent hepatic parenchyma, the secreting cells of which seem to perish by atrophy without contributing to the substance of the growth. (See Microscopical Section, Nos. 2389 to 2393.)

No. 959, Medical Section. The greatly hypertrophied spleen. The spleen was nine and a half inches long, five and a half broad, by three thick in the middle. It weighed when fresh seventy ounces. Its margins presented several deep fissures (lobulated spleen). Beneath the peritoneal coat were a number of small cancer nodules. Sections of these cancer nodules showed their structure to be quite similar to that of the cancer nodules of the liver, except that the meshes of the nucleated cylinders were more irregular and the cylinders themselves were in many parts somewhat thicker. The relations of the margins of these nodules to the hypertrophied splenic

tissue, so far as it could be made out, was essentially similar to what was observed in the liver. (See Microscopical Section, Nos. 2642 to 2655.)

No. 960, Medical Section. The uterus and its appendages. Each ovary is transformed into an oval somewhat nodulated scirrhous mass, an inch and three-quarters in long diameter. In sections no trace of Graafian follicles or ovules could be discerned. The scirrhous mass everywhere consisted of a tissue very similar to that above described in the breast. Simply the meshes of the nucleated cylinders were longer, so that when the section passed parallel to them they could sometimes be traced running parallel to each other for some distance; moreover, the connective-tissue stroma was more conspicuously fibrillated than in any of the other growths. (See Microscopical Section, Nos. 2449 to 2467.)

No. 956, Medical Section. The heart. No. 957, Medical Section. A portion of the pericardium. On the surface of the heart and of the parietal pericardium were a number of flat white thickenings, which were at first supposed to be carcinomatous. They proved on section to be composed of an imperfectly-fibrillated matrix, in which numerous small corpuscles were imbedded, and probably are to be referred to some previous inflammatory process, and not to the carcinomatous disease. (See Microscopical Section, Nos. 4690 to 4693.)

On reviewing this case we cannot fail to be struck first with the great similarity of the minute structure of all the morbid growths, although seated in quite diverse organs. Everywhere the normal tissue characteristic of the part affected appeared to be replaced by the morbid product, which was everywhere similar in texture.

This similarity of texture has long since been frequently observed in multiple carcinomatous growths and is the anatomical basis of the doctrine of a cancerous dyscrasis regarded as the constitutional cause of the local affections. It will readily be understood that if we could believe in a special modification of the blood giving rise to peculiar organizable exudations, we should expect the tissues thence derived to be everywhere characterized by some common structural features, the consequence of their common *origin*. Such a view as that of Koester, however, would equally well explain the circumstance referred to. For if the nucleated cylinders are, everywhere, transformed lymphatics, they must be expected to have very many features in common, combined with some minor differences in the calibre of the cylinders, the size of their meshes, &c., consequent upon the local variations in the character of the finer lymphatic nets.

Koester describes the network of cylinders in epithelial cancer as "cell cylinders" and was able by silver imbition to map out cell

walls and to trace the continuity of the cancer cylinders with normal lymphatics lined by their characteristic epithelium. At the time the foregoing case was investigated his monograph had not yet reached me; since reading it I have caused sections of several other carcinomatous tumours to be treated with silver, by Dr. E. M. Schaeffer, one of the assistants at the Museum. In several instances results were obtained approximating those described by Koester, but they have not yet been sufficiently complete to justify me in forming a definite opinion. I hope to make this matter the subject of a future report. In the meantime, however, I cannot but admit the close correspondence between the outlines of the plexus of nucleated cylinders and the network obtained when the lymphatic capillaries are injected. And I may add that the general features of the morbid growths, above described, *viz.* a network of nucleated cylinders, interlacing with a connective-tissue stroma, are to be observed in quite a number of the sections of carcinomatous growths preserved in the Museum. In none of these cases, however, have I as yet been able to satisfy myself with Koester of the existence of a lumen in the central part of the nucleated cylinders, nor am I convinced that the normal lymphatic capillaries, lined by an epithelium, are alone capable of being transformed into the cancer cylinders. Much more probable does it appear to me that all the lymphatic spaces of the connective tissue are susceptible of this transformation, which would at once account for the voluminous character of the pathological network.

The presence or absence of cell walls in the protoplasm, through which the nuclei of the cancer cylinders are distributed, does not appear to me an essential point. Marked cell walls can be observed in the cylinders of several epithelial cancers in the Museum collection. On the other hand, Koester himself mentions that at times he found cylinders in which the nuclei were imbedded in a granular protoplasm in which no cell walls could be observed.

In the present state of our knowledge we may perhaps regard the cell wall as an indication of a comparatively advanced stage in the history of the individual cell, which at first consists merely of a nucleus surrounded by a mass of protoplasm. There seems then no difficulty in agreeing with Koester to regard such nucleated cylinders as I have described above as composed of cells too young to be possessed of walls, and placed in such close juxtaposition that no line of demarcation can be observed between the protoplasm of the several elements.

As to the mode in which the cells of these cancer cylinders arise, however, I find greater difficulties. I am not satisfied either by my own examinations or by the study of Koester's paper that they can be justly considered the progeny of the lymphatic endothelium. A

view which should regard them as transformed white corpuscles, accumulated in the lymphatic passages, appears to me, if anything, a more probable interpretation. This would harmonize with the general absence of any demonstrable lumen, as well of demonstrable cell walls for the separate elements, at least in the earlier period of the history of individual cylinders; it would also harmonize with the close agreement generally observed between the elements of the youngest cylinders, *i. e.* those in the most peripheral parts of the growth, and ordinary granulation tissue.

I am not willing, however, to commit myself very warmly at present to the advocacy of any detailed views as to the mode of origin of the cancer cylinders. I desire simply to call attention to the probability, now almost a certainty, that they are conditioned in their form and direction by the pre-existing lymphatic passages, and am disposed to postpone any opinion as to their genesis till further facts have been accumulated.

In the case which has been described the primary growth was not received by me for investigation. I have therefore added a short account of a primary tumour of the breast, in which the structure was very similar to that observed in the secondary cancer of the breast in the case above described.

No. 5905, Surgical Section, is a scirrhous tumour of the female breast removed by a surgical operation in the spring of 1870. No particulars are recorded, and the subsequent history of the patient has not been reported.

A section through the piece shows that the mammary gland is represented by a small somewhat pyramidal mass, the apex of which terminates in the retracted nipple. The borders of the mass extend irregularly into the surrounding adipose tissue which constitutes the bulk of the specimen.

The case represents that very common variety of scirrhous in which the mammary gland is transformed into a small irregular cancerous mass, while the surrounding adipose tissue remains normal in bulk or even may actually increase in quantity. Nos. 3489, 3490, and Nos. 4616 to 4623, Microscopical Series, are sections of this tumour.

In these sections, as in the secondary mammary tumour in the former case, more or less modified gland lobules were but rarely encountered, and the greater part of the carcinomatous tissue appeared to consist simply of a network of nucleated cylinders imbedded in a connective-tissue stroma. In this case, however, the cancerous mass was imbedded in a more bulky mass of adipose tissue, the peripheral parts of which were quite normal. The separate fat-cells were much larger than in the first case, being from ·002 to ·004 inch in diameter, while in the first case the

limits were ·001 to ·002 inch. This would appear to depend simply upon the degree of emaciation which existed in the first case at the time of death. At all events the anatomy of that part of the adipose tissue which adjoined the morbid growth was essentially the same in both cases. Extension of the cancer cylinders into the connective-tissue septæ between the fat-lobules, and the development of a more or less abundant nucleated matrix between the individual fat-cells of those lobules nearest the scirrhous tissue, were characteristic conditions in both cases.

The first of the plates which illustrate this paper represents a portion of preparation, No. 3489, Microscopical Section, which is taken from this second case, magnified 400 diameters. It exhibits a side view of a part of the network of nucleated cylinders. The granular character of the protoplasm, the imbedded nuclei and the manner in which the nucleated cylinders interlace with the connective-tissue stroma, are faithfully represented. The nuclei of the connective-tissue stroma are not so obvious, but few of them lying in the optical plane selected for representation.

The second plate represents a portion of one of the liver nodules in the first case. It is copied from preparation No. 2393, Microscopical Section, magnified 400 diameters. The section divides several of the nucleated cylinders in a direction nearly perpendicular to their course, and they appear hence, as above described, not unlike so many multinucleated or mother-cells.

Both photographs were made from carmine-stained preparations, mounted in balsam, and in both, therefore, the nucleated cylinders appear shrunk away from the connective-tissue stroma in which they are imbedded.

In conclusion, I must refer briefly to another interpretation of the nature and origin of such nucleated or cell cylinders as I have above described, which has been received with great favour for some few years.

Thiersch in his work on epithelial cancer explains the cell cylinders in that form of carcinoma, especially of the skin, as outgrowths from the lower layer of the epidermis and from the epithelium of the glandular apparatus.* Such an interpretation implies, of course, the possibility of demonstrating that the cell cylinders are continuous with the epithelial structures from which they are supposed to have budded forth. Such a continuity Thiersch believes himself to have observed in many of his sections. He admits, however, that it is often impossible to make it out, and explains this by supposing the connection to have originally existed but subsequently to have disappeared, pointing, in justification of the hypothesis, to the history of the normal development of the

* 'Der Epithelial-Krebs.' Von Dr. Carl Thiersch. Leipzig, 1865. Page 58.

embryo as affording many examples of epithelial formations, which proceed originally from the epithelium of the surface, but become disconnected with it at a later period of their growth. Thiersch did not fail to observe the fact of the frequent anastomoses of the cell cylinders, which he has very well figured in fig. 1, Taf. IX, but this circumstance did not shake his confidence in the view he had propounded.

Billroth maintains similar views to those of Thiersch, not merely for cancer of the skin, but also for mammary cancers, and in fact for cancer generally.* In the last edition of his lectures he mentions the view of Koester as "enticing," but thinks that "all his evidence in favour of this view is not tenable." †

I would call attention, however, to the fact that the manner in which the cell cylinders anastomose in at least many mammary cancers, points rather to the lymphatics than to the mammary-gland tissue. Moreover, when a mammary cancer returns after extirpation, or when secondary growths develop in distant organs, as in the case reported in this paper, the new formations are generally constructed upon the same structural plan as the original growth. To reconcile these facts with Billroth's views, we *must* resort to far more complicated and improbable hypotheses than those of Koester.

Billroth was well acquainted with the anastomosing cell cylinders in certain forms of mammary cancer. He describes such growths as the "tubular form." According to him, mammary cancer in general almost always begins "with a coincident enlargement of the small, round, epithelial cells in the acini, and with *small* celled infiltration of the connective tissue around them." He admits that it is "difficult to make out the further fate of the glandular acini," yet he has no doubt that in the tubular form "the acini do not maintain their form, but grow into the connective tissue as very thin cell cylinders, while it becomes infiltrated with cells." I mention with interest that fig. 144, page 643 of his work (*loc. cit.*), agrees precisely, so far as a woodcut can represent the *microscopical* appearances of a morbid growth, with portions of some of the sections of the two cancerous breasts described in this paper. In it, as in them, certain figures which undoubtedly represent more or less modified gland acini and ducts, lie side by side with the cell cylinders, which are variously shaped in accordance with their relation to the plane of the section. Both are imbedded in the same connective-tissue stroma, but in his figure, as in the Museum preparations, no anatomical connection between the two is shown. They are separated always by connective tissue.

* 'General Surgical Pathology and Therapeutics.' By Theodore Billroth. Translated by Charles E. Hackley, M.D. New York, 1871.

† *Loc. cit.*, p. 631.

Nevertheless I am not prepared to deny that transformations of the true gland tissue of the parts involved, play a certain rôle in producing the texture of cancerous growths. But just how far they are atrophied and perish, how far at times a formative activity may lead to their transformation, are matters as to which it is difficult at present to form a judgment, and to which I hope to return at some future time.

Meanwhile I cannot avoid calling attention to two more of Billroth's woodcuts—fig. 148, page 648, and fig. 150, page 651 (*loc. cit.*)—the first representing the "extension of cancerous tumour into the fatty tissue about a lymphatic gland," the second "cellular infiltration of the fatty tissue in the periphery of a hard cancer of the breast." Either figure might have been drawn from the altered fat in the sections of the two breasts described in this paper, and the appearances are such that, as Billroth well remarks, "we can scarcely avoid thinking that in these cases also, white blood-cells escaping from the vessels cause the cellular infiltration." With this suggestion I heartily agree; but the real and difficult point in the analysis of cancerous tumours, as of so many pathological transformations, is to define with precision the exact part which the emigration of white blood-cells plays in the process, how far it is the principal, how far merely the accessory alteration. This report is offered as a preliminary contribution to the study of this question, now become so important in reforming our pathological doctrines.

I may mention, in conclusion, that the microscopical sections referred to were all made by Dr. E. M. Schaeffer, and that they are permanently preserved in the Microscopical Section of the Museum, where they may be examined by anyone interested in the subject. — *Report to the Surgeon-General of the United States' Army.*

ARMY MEDICAL MUSEUM, MICROSCOPICAL MUSEUM,
April 29th, 1872.

VII.—*The Theories of Cell Development.* By I. N. DANFORTH, M.D., Pathologist to St. Luke's Hospital, Chicago, U.S.A.

FROM a very early period in the history of medicine the idea prevailed that there must be some anatomical unit, some elementary part or particle, which, whatever might be its form or size, must be regarded as the true seat and home of vitality. Without the aid of

optical instruments, or even of the dissector's scalpel, and at an age of the world when philosophy was but a tangled maze of fancies; when men, with curious persistency, shunned the plainest truths and blindly groped in darkness, even then the conviction seems to have forced itself upon many that, underlying the various tissues and organs of the body, there must be some vital element common to them all, and from which they were all originally differentiated. And this idea was not so much the product and result of close and vigorous analytical *thought* as it was the outgrowth of an inevitable necessity, namely, the necessity for a *somewhere* and a *something* which should serve as a biological starting-point. Hence we find Fallopius (1523–1562), Borellus (1656), Haller (1757), and others, constructing their rude, and to us preposterous, theories in answer to demands which were irresistible. But these early theories have for us little value or interest, save as antiquaries, since none of them clearly recognize the cell as the ultimate anatomical element. To the histologist of the present day it seems strange, and indeed almost incredible, that so many years should elapse between the real discovery of the cell and the comprehension of the one great fact that this little body is the starting-point of all development. There seems to be little doubt that Borellus actually saw the cell as long ago as 1656, since, as Dr. Tyson remarks, "he describes pus-corpuscles as animalcules, and even says he saw them delivering their eggs."* In truth, it is more than probable that what Borellus believed to be the act of "delivering eggs" was really the process of cell development by "budding" or "gemmation," so that this very important discovery was made two centuries before it was announced.

The first attempt at the construction of a systematic theory concerning the cell and its origin, seems to have been that of Wolf, about the year 1759. From this time, therefore, the cell may be said to have a connected history; although it is quite common to regard cell history as commencing many years later, with Schleiden and Schwann. Although the name of Wolf was well-nigh forgotten, and would have so remained but for the magic touch of the brilliant but erratic Huxley,† his doctrines have exercised no little influence upon the thoughts and writings of many of his followers. We shall presently see that Wolf is the *real*, while Schleiden is only the *putative*, father of that wayward child, free or spontaneous cell development.

If we carefully analyze the various cell theories which have appeared from the time of Wolf until the present—and which for convenience we may style "modern" theories—we shall find, first,

* 'The Cell Doctrine,' p. 16.

† 'British and Foreign Medico-Chirurgical Review,' October, 1853.

that they are all hinged upon different conceptions concerning cell development; and secondly, that these different theories may be reduced to four classes, namely:—

(*a.*) Theories which recognize free cell development.

(*b.*) Molecular or globular theories.

(*c.*) Theories which locate developmental power outside the nucleus.

(*d.*) Theories which confine developmental power to the nucleus alone.

No sooner was the cell recognized as the true and universal agent of development, than the importance of understanding its mode of development was seen; hence we at once encounter theories of cell development, and for many years these followed one another in rapid succession.

(*a.*) *Free Cell Development.* — This doctrine has at least one positive merit, namely, boldness. Commencing, as I have already intimated, with Wolf in 1759, it has encountered various *ups* and *downs*, until, at the present time, it is really the keystone of the doctrine of "spontaneous generation," so vigorously defended by Dr. H. Charlton Bastian. According to Wolf, "every organ is composed at first of a mass of clear, viscous, nutritive fluid, which possesses no organization of any kind, but is at most composed of globules."* Subsequently cells are developed, but they are "mere cavities, and not independent entities; organization is not affected by them, but they are the visible results of the action of the organizing power inherent in the living mass, or what Wolf calls the *vis essentialis*" (Huxley). This simply amounts to saying that the cell is merely a passive, not an active body; that it is only matter thrown into a convenient form for building material, as clay is thrown into the form of bricks for the same purpose, while the real builder is the so-called "*vis essentialis*" resident in the "clear, viscous, nutritive fluid." Twenty years ago it was necessary to combat this doctrine, but at the present day it does not even merit the compliment of criticism.

We encounter the next phase of free cell development in connection with the doctrines of Schleiden. This observer—the first to comprehend the fact that the cell is the true and ever-present basis of developmental action in vegetable tissues—acknowledged two distinct methods of cell growth; one of which, the so-called "*exogenous free cell formation*," must be regarded as a true creative act, while the other ("*endogenous cell formation*") is a mere continuance of the process, or cell multiplication. In the midst of a perfectly structureless, clear, transparent fluid (*blastema, cytoblas-*

* Huxley, *loc. cit.*

tema), granules are first seen, "some of which increase in size and assume the form of a minute vesicle, the nucleus of the future cell. On the addition of water to this, granules become apparent in its interior, and one of these, larger than the rest, appears to be the nucleolus. *Around* the nucleus, as is generally stated, the cell membrane is developed, and thus the cell is completed." * Virchow's account of Schleiden's theory does not exactly agree *in terms* with the one quoted above, although the practical difference amounts to nothing; it is as follows: † — "According to the view which was put forward in the first instance by Schleiden, and accepted by Schwann, the connection between the three coexistent cell constituents was long thought to be on this wise, — that the nucleolus was the first to show itself in the development of tissues by separating out of a formative fluid (*blastema, cytoblastema*), that it quickly attained a certain size, that *then* fine granules were precipitated out of the blastema and settled around it, and that about these then condensed a membrane. That in this way a nucleus was completed, about which new matter gradually gathered, and in due time produced a little membrane (the celebrated 'watch-glass' form)." In the one case, granules are first produced, and, by their aggregation, the nuclei are afterwards formed; in the other, the nuclei take the precedence, the granules being a secondary product. Seeing that the nucleus was discovered several years previously (1833) by Brown, of Edinburgh, and that Schleiden had conferred upon it the name of "cytoblast" or "cell-bud," thus directly implying his belief in its importance in relation to cell growth, it is more than probable that Virchow's account is the more correct one. Schwann, whose observations seem to have been confined chiefly to animal structures, adopted the views of Schleiden concerning the development of vegetable cells, and applied them to the genesis of the cells of animals. He "was of opinion that the extracellular formation of cells—that is, their development *in* free blastema—was the most frequent mode of their production in animals." ‡ At this period, therefore, the theory of free or spontaneous cell development was generally accepted by histologists; and, seeing that Schleiden and Schwann gave to this notion the support of their great authority, it is by no means a matter of surprise that it found general favour.

Passing by the more recent advocates of spontaneous cell-growth, especially Bennett, of Edinburgh, Carpenter, and Todd and Bowman,§ with the remark that they are simply recasts of *the*

* Peaslee's 'Human Histology,' p. 120. † 'Cellular Pathology,' p. 35.

‡ Stricker's 'Histology,' vol. i., p. 34.

§ 'The Physiological Anatomy and Physiology of Man,' edition of 1857. It is proper to observe that in the last edition (1866), now issuing in parts under the editorship of Dr. Lionel Beale, the well-known cell theory of the latter is presented.

views of Schleiden, we come to that phase of spontaneity which, if established, fully demonstrates the possibility and indeed the *constancy*, of the spontaneous origin, not of individual cells alone, but of living beings.

It does not come within the limits or scope of the present article to review the history of the long, and as yet unsettled, contest concerning the spontaneous evolution of cells, or of those simple aggregations of cells which form the lowest types of living things. For a very complete review of this subject, I take great pleasure in referring to the essay by Professor J. C. Dalton, in the 'New York Medical Journal' for February, 1872. The doctrine of spontaneous generation was generally believed in by the ancients; was disproved, in all except certain "exceptional" cases, by Regnier de Graaf (1672) and Von Baer (1827); was revived by Needham (1748), and again controverted by Spallanzani (1775); was vigorously assailed by Schultze and Schwann (1836 and 1837), and for twenty years after the period last mentioned was generally abandoned by scientists as untenable. "The production of living beings without parents was a theory admitted to have no reasonable basis for its support, and was regarded simply as a curious relic of antiquity."*

But in the year 1858 the doctrine of spontaneous evolution was revived by Pouchet, of France, as a sort of buttress to support the conclusions to which he was forced by the revelations of modern geology. Pasteur, however, and several other members of the French Academy, repeated Pouchet's experiments with entirely negative results, and again this troublesome ghost seemed upon the point of being laid, when Professor H. Charlton Bastian published his series of experiments, and announced himself as "content" and "justified" in "believing that living things may and do arise *de novo*." According to Bastian, various unicellular organisms are pretty constantly developed in boiled and hermetically-sealed infusions of hay, turnip, &c., and also in certain saline solutions treated in like manner. The reader is referred to Dr. Bastian's work, the 'Beginnings of Life,' recently published, for an extended account of his very interesting experiments, and the accompanying illustrative figures. He claims to have demonstrated that monads, bacteria, torulæ, vibrios, leptothrix filaments, fungus spores, and various unclassified organisms of cellular structure, are developed from solutions of organic and saline substances absolutely destitute of living germs; hence they must originate *de novo*. This, of course, is the very essence of free cell development, or rather it is something more than that, since it confers upon any solution of organic or saline matters containing the chemical elements of protoplasm, the power not only to generate cells, but to spur them

* Dalton, *loc. cit.*

forward to that subsequent differentiation which results in the (not *lawful*, but) lawless creation of living beings. It carries us back to the days of Wolf, and substitutes for specific germs, endowed with vital force for a specific and unchangeable purpose, a something (which we may as well call a "*vis essentialis*" as anything else) by virtue of which dead matter is brought to life, boiled hay is made a creator, and solutions of salts forget the laws of crystallization and take to themselves the power of the genesis of new and unheard-of *living* (!) beings. For my own part, I cannot accept the doctrine of spontaneous cell formation in any form or to any extent, or the doctrine of heterogenesis (which is the same thing in different dress), until, like Pouchet, I shall be blessed with that boundless faith which sees in spontaneity the very beginning of animated existence, and in the higher order of beings, even up to man himself, only the result of "the ascending development of organized beings upon the globe."

(*b.*) *Globular or Molecular Theories.*—The globule theory, according to Virchow, was partly the outgrowth of the reaction against the fibre theory of Haller, partly the result of optical illusions from the unskilful use of microscopes which, at the very best, were infinitely inferior to those of the present day. It is difficult to understand the precise relation of the globule or molecule theory to the cell in the minds of the earlier writers, for the following reasons: First, several observers, following Leuwenhoek, believed that the tissues were developed directly from globules, without the intervention of cells; secondly, the terms "globule," "molecule," and "granule" were, and indeed still *are*, so interchangeably used, especially among the Germans, that neither term seems to have, or to have *had*, any very definite meaning; and thirdly, it is, to say the least, very probable that cells were frequently described as globules, and globules were sometimes described as cells. Although the terms "globule," "granule," and "molecule" are now understood to refer to bodies which are morphologically, and possibly chemically, distinct, they usually seem to have indicated to the earlier, as well as to some recent, writers, bodies smaller than cells, but having to cells a developmental relation.

The writings of Milne-Edwards gave general currency to the globule theory; but he does not seem to have deemed it necessary that globules should be converted into cells before proceeding to form tissues; but, as pointed out by Tyson, "there is little doubt but that many of these so-called globules described by Edwards were really cells, seen with indifferent instruments, and further distorted by the glare of direct sunlight."*

On the other hand, Arnold and Baumgärtner recognized the cell

* 'The Cell Doctrine,' p. 24.

as an intermediate stage of development. "A cell was considered to be produced by the globules arranging themselves in a spherical form, so as to constitute a membrane, within which other globules remained, and formed contents."* But they also believed that fibres were formed of elementary granules drawn up in a line.

Concerning the so-called "investment" or "cluster" theory, which for a time was pretty generally accepted, Virchow says "the upholders of this theory imagined that originally a number of elementary globules existed scattered through a fluid; but that, under certain circumstances, they gathered together, not in the form of vesicular membranes, but so as to constitute a compact heap (*mass, cluster,—Klümpchen*), and that this globe was the starting-point of all further development,—a membrane being formed outside and a nucleus inside by the differentiation of the mass by apposition or intussusception."† This theory is a most singular conglomeration of truth and error: of truth, in that it concedes what we now know so well, namely, that the membrane is not a necessary part of the *living* cell; of error, in that it practically recognizes free cell development.

Probably the most strenuous and persistent advocate of the globular (or, to adopt his own terminology, the molecular) theory at the present time is Professor Bennett, the eminent pathologist of Edinburgh. His theory is best given in his own words:—"The ultimate parts of organization are not cells nor nuclei, but the minute molecules from which these are formed. They possess independent physical and vital properties, which enable them to unite and arrange themselves so as to produce higher forms. Among these are nuclei, cells, fibres, and membranes, all of which may be produced directly from molecules. The development and growth of organic tissues is owing to the successive formation of histogenetic and histolytic molecules. The breaking down of one substance is often the necessary step to the formation of another; so that the histolytic or degenerative molecules of one period become the histogenetic or formative molecules of another."‡ On the succeeding page he adds, "As to development, the molecular is the basis of all tissues. The first step in the process of organic formation is the production of organic fluid; the second, the precipitation in it of organic molecules, from which, according to the molecular law of growth, all other textures are derived either directly or indirectly,"—that is, with or without the intervention of cells.

It will readily be seen that the theory of free cell development, as originally stated by Wolf and modified by Schleiden, and the globular or molecular theory, are, practically, one and the same

* Virchow, 'Cellular Pathology,' p. 53. † *Op. cit.*, p. 53.
‡ Bennett's Lectures, p. 118.

thing,—the difference being in *terms*, not in results. Both mean spontaneous cell formation. Both alike deny that a cell is necessary to the development of a new cell; but, while the former comes squarely to the point and *says* what it means, the latter makes use of the molecule as a sort of sugar-coat for that unpalatable dose, spontaneity.—*The Lens*, July.

VIII.—*Draw-tubes* versus *Deep Eye-pieces.*

By M. A. Prazmowski.

The magnifying power of any microscope depends upon the three following conditions:—

1st. The focal distance of the objective (equal to the principal focus of a small equivalent lens placed in the optical centre of objective combination).

2nd. The magnifying power of the eye-piece.

3rd. The distance between the optical centre of the objective combination and the eye-piece.

By varying the last two conditions, for instance, a long tube and low eye-piece, or a short tube and a high eye-piece, we may obtain results identical, so far as magnifying power is concerned. In other words, designating by L and l the longer and the shorter tubes, and by O and o the stronger and feebler eye-pieces, $\frac{L}{O} = \frac{l}{o}$.

For the sake of convenience, we leave out of this statement the slight difference in the power of the objective produced by the movements of the collar for the purpose of obtaining the best definition with the two eye-pieces.

It is a practical fact that, with a given magnifying power, the image obtained by a short tube and high eye-piece is more perfect, clearer, and brighter than that obtained by the use of a long tube and low eye-piece.

Any observer may verify this fact by employing successively the two methods for the purpose of augmenting, or perhaps we may say exaggerating, the power of a given objective beyond its best working limits. It will be found that the richness of detail is better preserved with the short tube and high ocular than with the long tube and low ocular.

By a careful study of the conditions present in the relations of the eye-piece and objective, I think I am able to show that the

different results above noted do not depend upon any defect in the correction of the spherical or chromatic aberrations of the objective combination, but are produced by the variable amount of interference of the rays of light in the two given conditions, the fringes of interference becoming larger and more injurious to the image as the tube is lengthened and the luminous cone becomes more acute.

It is well known that the capacity of an objective to display the details of a surface depends largely upon its angle of aperture; the larger this angle, the finer the objective. We make the single remark, that the magnitude of the angle of aperture has reference only to those rays that enter into the formation of the image. If the aberrations are not corrected through the whole extent of the angle, those rays that find entrance through the uncorrected portions, unless for the formation of the image only, serve to flood the field with light, in which the image is lost, drowned. The larger the angle of aperture, the larger the surface of the back lens of the objective. The circle formed by this back lens, which receives all the light introduced into the objective, is the base of a cone, of which the summit is found in the conjugate focus of the objective, at the point where is formed the image seen by the eye-piece. In objectives of high power the base of this cone, as compared with the distance separating it from the eye-piece, is very small; the cone is very pointed, and the rays intersecting one another at its summit make an angle necessarily very acute, and more acute in proportion as the summit is removed from the objective.

We know that rays of light crossing one another at very acute angles do give rise to this phenomenon of *interference*, and that the fringes are larger in proportion as the angle of intersection is diminished. Applying this principle to the problem under consideration, we find that the rays passing near the centre of the objective must give rise to this phenomenon, and that as the rays pass from the centre the fringes of interference will be less, and the definition consequently finer and sharper. This improvement in the definition will continue until the rays approach the border of the lens, when, from imperfect corrections of *aberrations*, or too great an angle of incidence, they no longer enter into the formation of the image.

We may say, for the sake of simplicity, that the extreme rays nearest the borders of the objective form a cone, the base of which is upon the objective and the apex in the focus of the eye-piece. In proportion as we increase the length of the tube of the microscope, or, what is the same thing, the distance between the objective and eye-piece, the luminous cone becomes more pointed, for the base of it is unaltered, while the height of it is increased. The angle of intersection of the rays at the summit is diminished, while at the

same time the fringes of interference rapidly increase in width, and, mingled with details of the image of the object, they only serve to mask it. The image produced at a less distance from the objective is less affected by these fringes when magnified by a higher ocular, and will be found, although enlarged to the same extent, to be more sharply or clearly defined, and more exact in all its minute representations of real structure.—*The Lens*, July.

PROGRESS OF MICROSCOPICAL SCIENCE.

The Structure of the Rods of Corti.—Dr. Urban Pritchard has communicated a short but important paper on this subject to the Royal Society, May 30th, from which we may give the following account of these curious rods:—In a general view of the rods from above, they appear similar to two rows of pianoforte hammers, rather than like the keys of that instrument, to which they have been likened. In a lateral view, these two rows of rods are seen sloping towards each other, like the rafters of a gabled roof. The rods consist of a shaft and two enlarged extremities, but the two rows differ considerably in form; the inner rods are attached by their lower extremities to the membrana basilaris at its junction with the lower lip of the limbus, and just external to the spot where the nerve filaments emerge; they are directed outwards and upwards, with a slight undulation to meet the outer rods. The lower extremity is enlarged and rounded, gradually tapering to the shaft, which is cylindrical; the upper extremity is somewhat cuboid in form, but the outer surface is deeply concave, and the upper lip of the concavity is prolonged into a process. The outer rods are attached to the membrana basilaris by a broad base, which also gradually tapers to a cylindrical shaft. Their upper extremity is less cuboid in form, and presents a convex internal surface, which articulates with the corresponding concavity in the inner rods just mentioned; from the outer and upper part there extends outwards a slender process. One of the most important features with regard to these rods is their relative length. Most authors state that there is very little difference in the length of the two rods; in this, however, they are much mistaken; for not only do the two sets of rods differ in this respect, but the length of each varies according to its position on the cochlea. Thus, at the base, the outer rods are as nearly as possible equal in length to the inner; but proceeding upwards, both rows increase in length with great regularity, although not in the same ratio, the outer increasing with much greater rapidity, so that near the apex they are twice the length of the inner. It was generally supposed, *a priori*, that these rods were graduated so as to distinguish the most minute variation of tone, but no one until now has been able to demonstrate this. The rods, therefore, vary in length from about $\frac{1}{500}$ to $\frac{1}{300}$ of an inch. The number of rods in each row is not the same, there being about three of the inner to two of the outer; and, according to calculation, there are about 5200 inner rods and 3500 outer in the whole cochlea. Most authors, with the exception of Deiters, describe nuclei situated in various parts of these rods, principally in the lower extremities; but although seen from above this appears to be the case, on closer observation these so-called nuclei of the rods are found to be nothing more than the nuclei of cells surrounding them.

The Early Stages of the American Lobster (*Homarus Americanus*).—These are the subject of an able paper of some length in the June

number of the 'American Journal of Science,' to which we refer our readers for more details. The stages are all those after the lobster has left the egg, as those within the ovum are well known already. The author, Mr. S. J. Smith, makes four stages in the post-ovum development of this animal. These he then minutely describes and figures, and he considers that the fourth is the adult stage. He thinks, too, that there is probably another stage between the third and fourth.

A Microscopical Examination of the Skin in Impetigo contagiosa.—An interesting paper on this subject appears from the pen of Dr. H. G. Piffard in the 'New York Medical Journal,' June, 1872. The author says that the first examination was made March 7th. By this time the potash had effected an almost complete solution of the crusts, with the exception of a few small, whitish masses, which remained undissolved. One of these was transferred to a glass slide, a little glycerine added, and a thin covering glass applied. Upon examination with a $\frac{1}{5}$ (Grunow) little was found to attract attention except a few collections of very transparent epidermis, without visible nuclei, many oil-globules, and some granular matter. The first half-hour yielded, with the exception of the above, but negative results. At this time a slight modification of the method of illumination brought to view a minute circular body whose exceeding transparency had hitherto rendered it difficult of detection. Applying a higher power ($\frac{1}{10}$ Grunow), and carefully focussing, the body exhibited a peculiarity of form which was exceedingly striking. The effect produced by depressing the objective was similar to that with which we are all familiar in the case of the red blood corpuscle, namely, the border first came into focus, as evidenced by the bright outline and dark centre, and then, upon further depression, the border became dark and the centre brighter. Reversal of the movement produced a reversed effect. The first impression was, that the body under examination was in reality a blood corpuscle, so thoroughly was its appearance counterfeited; but its minute size, and the fact that it had lain two days in a potash solution, precluded this possibility. Further search detected six or seven similar bodies in the same field, and prolonged examination of the preparation, and of others upon succeeding days, enabled me to determine the few following facts:—The bodies, which were unquestionably vegetable organisms, were generally circular in outline, a few being oval. Their average diameter was $\frac{1}{5000}$ of an inch. Their form, as determined by focussing, *appeared* to be the same as that of the red blood corpuscle, namely, a biconcave disk. The spores were usually isolated, but occasionally two or three, and in one instance four, were found together. In this latter case, three of them were circular, while the terminal at one extremity of the chain was oval. In another case the two-days-old crust was placed in a three-per-cent. solution of potassa fusa, while a small fragment of the older crust was macerated for a few minutes in a mixture of liquor potassæ and glycerine, and immediately examined with a $\frac{1}{15}$-inch objective (Wales). The field presented was extremely striking. Not only were the hæmo-discoid bodies, which we had previously encountered, present in abundance, but others exhibiting an almost endless variety of form. This unex-

pected and puzzling difference of form naturally excited the liveliest curiosity, and led to the most careful examination, with a view to account for it if possible. It seemed improbable that the little hæmodiscoids, which at first we supposed to be the type-form, could have developed into many of the irregular forms presented. This view had to be abandoned, and search was made for some other simpler form or type which would permit of the variations noticed. Among others we observed a body resembling a spermatozoon, that is, with a round or rather oval head, and a thin filamentary tail, slowly *moving* across the field. We say moving in contradistinction to *moved* (by currents in the fluid). This motion was accompanied and probably caused by slow undulatory movements of the tail, nothing like the quick, jerking movement of spermatozoa, but resembling those observed in many other protophytic organisms. In another instance the head was elongated and in others again the tail consisted of two parallel filaments united below, and directly continuous above with the dilated cephalic extremity (so to speak). In others there were one or more slight dilatations along the course of the tail. Evidence of apparently vital movement was not lacking in connection with some other individual spores upon the slide. One of the most striking and unmistakable instances of this was witnessed in the case of a body composed of a chain of three rounded portions, to one extremity of which was attached a filamentary prolongation, itself terminated by a minute rounded body. The whole outline of this curiously-shaped body could be distinctly seen, with the exception of a small portion upon one side, which was partially obscured by some granular matter. At this time, attention was diverted from the microscope for a few minutes, not more than ten, and upon returning to the instrument, instead of finding the object last observed, in its place was one of a somewhat different form, recognized, however, as the same body by its relations to the granular matter above referred to. Observation was continued, in order to detect further changes, if any should occur. In a few minutes the extremity of the tail curved slowly up, and attached itself to the main body, forming a loop. This movement was distinctly visible. The slide was now temporarily put aside, and attention was given to the crust which had been digesting in the potash solution for (by this time) about two hours. The crust was found to be converted into a viscid gelatinous mass, due to the action of the potash upon the pus of which it was in part composed. Portions of this jelly were placed upon slides and immediately sealed with Bell's cement. Examination revealed new shapes, among others an appearance such as would be produced by stretching a thin india-rubber ring. These elongated bodies varied greatly in size. Their breadth was nearly uniform, being about the $\frac{1}{10000}$ of an inch, but in length they measured from the $\frac{1}{40000}$ to the $\frac{1}{750}$ of an inch, as determined by an eye-piece micrometer. Another body appeared to be composed of two concentric rings. How these were formed was at first not very clear, but subsequently became easily explainable. The essential portion of the observation was finally completed by the discovery of certain fine filaments, not more than the $\frac{1}{30000}$ of an inch in breadth, but whose

length varied from the $\frac{1}{2000}$ to the $\frac{1}{300}$ of an inch. The presence of these filaments readily accounted for the development of the various forms hitherto noticed, from the simple hæmo-discoid to the more complicated varieties. No direct observation of the transformation of the filaments into other forms was witnessed, but it seems to me to be a fair inference that such change occurs. In addition to the well-marked bodies described, there were present epidermic cells, and a little oil, and besides many granular or rather molecular bodies, some slightly elongated, such as are found in most animal and vegetable infusions, and upon whose exact nature it would be somewhat hazardous to pronounce. These little bodies, as is usually the case, were in rapid motion. No connection could be traced between the epidermis and the spores; occasionally, to be sure, a spore would be found upon or beneath an epidermic cell, but the occurrence was probably accidental, and nothing like the intimate relationship which exists between the epidermis and the *microsporon furfur* was detectable. No hairs were visible in any of the preparations, and hence nothing can be said concerning them. Observation of the circular spores, especially the larger ones, with a $\frac{1}{16}$ immersion of Powell and Lealand upon their grand binocular stand, the property of my friend Prof. Arnold, leads me to suspect that their form is not that of a section of a cylinder with central depressions, but rather that they are rings. It is possible, however, that in the smaller ones the hole in the ring is occluded by subsequently-deposited material. The difficulty of deciding this point can be readily appreciated when the minute size of the organisms is borne in mind. The following conclusions seem fairly deducible from the observations above recorded: The bodies are unquestionably vegetable organisms. Their primitive form is probably filamentary. From the filament the others are developed. Their presence explains the contagiousness of the affection in which they are found, and brings it into the class of the Parasitic affections of the Skin.

Diseased Bone. Giant Bone-cells.—The 'British Medical Journal,' July 13th, gives an article on some important discoveries of Kölliker, relative to disease in bone. Robin has described under the name of *myéloplaxes*, and Virchow and other German histologists under the name of giant or colossal cells (*Riesenzellen*), certain multinucleated masses situated on the external part of the marrow of bone. These bodies have been subjected to careful research by Kölliker; the results of whose investigations, as communicated recently to the Würzburg Physico-Medical Society, point to an intimate connection between the presence of these bodies and the process of resorption of bone. Kölliker finds that, during resorption in healthy bones and teeth, the surface where this process is taking place presents fine lacunæ (commonly termed Howship's lacunæ); and that these lacunæ contain giant cells. Generally there is one cell in a lacuna, which it closely fits; sometimes a single cell fills two lacunæ; and in other cases there are several cells in a large lacuna. Considering these cells to be important agents in the removal of osseous or dental tissue, he designates them "osteoclasts" (or "ostoophages"). Lacunæ containing ostcoclasts have been found by Kölliker both in the interior

and on the exterior of bones. Internally, they exist in ossifying cartilage, just beyond the bone, where ultimately cancelli are formed, as in the bodies of the vertebræ and the ends of the long bones. They also are present on the walls of the larger medullary cavities in process of development; as in the diploe of the cranial bones, in the medullary canals of long bones; and the development of the frontal, sphenoidal, and other sinuses of the head and face, is intimately connected with their presence in these parts. On the external parts of bones they exist in the following situations: the dental grooves of the embryonic jaws; many parts of the bones limiting the cranial cavity, both at the base and at the sides and summit; on the walls of the vertebral canal and on the arches and bodies of the vertebræ; on the walls of the orbit and of the nasal fossæ; on the coronoid and condyloid processes of the lower jaw, and on the zygomatic process of the upper jaw; in the situations of the foramina and canals by which many of the bones are perforated, such as the foramen ovale, the foramen opticum, &c.; and on certain parts of the bones of the trunk and extremities, especially in the neighbourhood of the articular ends of the bones; as well as on the surface of the temporary teeth, where they are abundantly developed while these are undergoing absorption. Having described the situation where these osteoclasts are found, Kölliker proceeds to comment on their signification. Hitherto, he says, the normal resorption of bone has been attributed either to the chemical action of the fluids or of the medulla, or to some not very well-defined minute process. Thus Virchow supposes the osseous substance to pass into simple marrow-cells; Bredichin and Rindfleisch regard the "myéloplaxes" or osteoclasts as bone-cells set free by removal of the earthy matter; and Billroth believes that, in cases of disease, the bone is removed by the pressure of granulation-cells. Kölliker's experiments have led him to the conclusion that the resorption of osseous and dental tissue does not consist in a mere transformation of its cellular and other elements, but in a removal of them; and that the multinucleated cells or osteoclasts are in no way formed from pre-existent osseous or dental tissue. The proofs in support of this view are the following:—1. A bone or tooth, at the parts where it is studded with osteoclasts, shows no sign of transformation of its cells, such as enlargement, multiplication of nuclei, &c.; and it is extremely common to find, on the walls of the lacunæ which contain the osteoclasts, partial erosions in the tissue, which in other respects presents no change. 2. The osteoclasts can always be readily separated from the osseous tissue; there is no intimate connection or gradual transition between them. 3. After the absorption of the bone, small splinters not unfrequently remain; these contain no cells, and cannot be transformed into cells. 4. The occurrence of osteoclasts on the surface of the ivory of the milk-teeth shows that no elements of the dental tissue that is undergoing absorption can enter into their formation. 5. A most striking fact is the presence of osteoclasts on the surface of ivory pegs that have been driven into bone. Billroth has found that these pegs become eroded; and Kölliker has found them to present on their surface not only lacunæ,

but also osteoclasts. The osteoclasts, then, do not arise from pre-existent osseous or dental tissue; and Kölliker believes that they belong to the same category as the osteoblasts, which perform an important part in the formation of bones. His reasons are as follows:—First, transitional forms are observed. Secondly, most of the osseous surfaces, which at the time when absorption takes place are covered with osteoclasts, originally present osteoblasts; and the gradual intrusion of the former among the latter can be noticed by direct observation. Thirdly, osseous surfaces that have been undergoing absorption may again take on growth; and then there is a re-formation of osteoblastic cells, partly through the splitting up of the multinucleated osteoclasts. This is observed in the jaws. Before and at the commencement of the formation of the teeth, the lower jaw shows osteoblasts in the situation of the future dental groove. After this, when the dental sacs and teeth are being formed, the bony substance is removed, and a large furrow is formed in the jaw. Lastly, when the eruption of the teeth takes place, there is again a formation of bone at their roots, leading to the development of the alveoli. As to the manner in which the osteoclasts are concerned in the removal of bone, Kölliker has not arrived at a definite conclusion; but he considers the following points worthy of note:—1. The removal does not consist in an absorption of the earthy matter as in osteomalachia; it seems rather, that both the organic and the inorganic material are removed simultaneously. 2. The removal of the hard tissue is not preceded by crumbling, but takes place gradually, leaving the bone with well-defined and smooth edges and surfaces. 3. The surface of bones undergoing absorption, where osteoclasts are present, have not an acid reaction. 4. Two experiments, made in order to ascertain whether the osteoclasts yielded any slightly acid or alkaline material having a solvent action on the bones, were attended with negative results. 5. In many cases the osteoclasts present clusters of fine short hairs or filaments on the side in contact with the bone.

The Microscopy of the Blood in Syphilis.—We have already alluded to this subject. However, the 'Lancet' has taken the matter up also, and it gives (June 22) an excellent leading article upon it. Dr. Lostorfer, who, it seems, was specially involved in the matter, has supplied Stricker's 'Medizinische Jahrbücher' with the facts which have led him to the position he has now taken up on the affirmative side of the question. He was supplied, it appears, by Stricker, who appends a note to the paper, with a number of test specimens; and arrived, on the whole, at very correct conclusions. A drop of blood was taken from the skin, and as quickly as possible placed upon a clean object-glass, and covered. Twelve glasses, with drops belonging both to syphilitic and non-syphilitic patients, and properly numbered for subsequent identification, were then placed in a moist chamber, and were examined from day to day with a Hartnack's immersion No. 10, ocular No. 3. During the first day or two, vibrios, bacteria, and the early stages of development of sarcina, were commonly met with. On the third, or still more frequently on the fourth or fifth day, and occasionally even after twenty-four hours, small bright corpuscles made their appearance,

which were partly at rest and partly in vibratory motion; some of them exhibited a small process. By the sixth day these corpuscles had increased in size and number. Many of these larger ones exhibited the above-mentioned process; and this then distinctly appeared as a bud. In a few it was so large that it was very little less than the parent cell. During the subsequent days the corpuscles continuously augmented in size; so that a few soon attained the magnitude of shrunken red corpuscles, or even exceeded them. At the same time smaller ones of all sizes were present. Gemmation was frequently perceived, many corpuscles presenting not one bud only but several, which were in some instances pedunculated, and in others directly applied to the parent cells. Cases also occurred where the secondary buds appeared upon the surface of the primary ones. Most of these gemmæ were of irregular form, but some were spherical. About the eighth or tenth day a vacuole formed within the larger corpuscles, which gradually increased in size till ultimately its walls appeared to consist only of a thin lamina, presenting a double contour under the microscope. At this period the corpuscles had attained their final development, and underwent no further changes even when the test could be preserved for a month, which of course was only of exceptional occurrence. Dr. Lostorfer also examined the characters of the corpuscles with different fluids, as with solution of sugar, distilled water, and Pasteur's fluid—namely, one-half per cent. of solution of common salt and one per cent. of solution of acetic acid. These fluids, notwithstanding their diversity, produced similar effects during the earlier period of the development of the corpuscles, causing them to shrivel, and preventing any further growth. Even at the sixth day some shrivelling occurred on the addition of these fluids and dilute solutions of sugar; but when distilled water was added at this period the vacuolæ enlarged rapidly. Gemmation took place in the form of long tubules, which closely resembled the germ tubes of fungi. The number of the corpuscles varied to a very great extent. In one instance he counted fifty of them in the field of the microscope at one time on the fourth day; but he is unable to say whether any proportion exists between their number and the severity of the symptoms of syphilis present. Dr. Lostorfer states that long before the discovery of these corpuscles he had carefully examined the blood of healthy persons without finding anything similar to them; and that during the last three months he has systematically subjected healthy blood specimens to the same treatment, as well as blood taken from those suffering from gonorrhœa, diphtheritic ulcers, lupus, and from typhus patients, without in any instance observing their development. He therefore thinks he is justified in terming them syphilis corpuscles. He is of opinion that temperature is a matter of great importance affecting their development; for when the temperature fell below a certain point, not yet exactly ascertained, the results were negative. He gives the appearances in thirteen cases, which show that the corpuscles are not associated with any particular form or stage of syphilis, but manifest themselves both before and after the occurrence of the eruptions and in tertiary affections. He guards himself from saying

they in any way constitute the carriers of the infection; and, as regards their origin, it is uncertain whether they are introduced with the syphilitic poison, or whether the blood is so altered in syphilis as to permit their growth when derived from other sources. In the note appended by Stricker to Lostorfer's paper, he says that he tested Dr. Lostorfer's ability to diagnose syphilitic blood under the microscope on six occasions. On the first he sent him twelve proof specimens, of which Nos. 8 and 9 were taken from healthy persons, and the remainder from three different patients suffering from syphilis. In a few days Lostorfer replied:—"Nos. 8 and 9 non-syphilitic; two preparations spoilt; the remainder syphilitic." In the second list there were seven numbered proofs, and in the third nine proofs, and in each of these the syphilitic cases were correctly diagnosed. In the fourth list, however, no results were obtained; but this Lostorfer explained by stating that all the specimens had accidentally been exposed to a very low temperature (– 12° C.). The remaining tests, including one by Professor Hebra, seem to have been very fairly made out. Whatever may be the ultimate conclusion arrived at, enough has certainly been ascertained to lead to attention being paid to the subject; and as the field of observation is unfortunately only too common, it is probable that positive results will soon be obtained. High powers, immersion lenses, and some experience with the moist chamber, are indispensable for pursuing the inquiry.

Spontaneous Generation: Arguments against.—A very able and clearly-reasoned paper, though it is against the theory, is that of Mr. W. N. Hartley, F.C.S., of King's College Laboratory. It is to be found in full in the 'Proceedings of the Royal Society,' No. 132, 1872. The paper is of some length, and should be consulted by those interested in the matter. We merely give the author's account of the well-known Child and Wyman experiments. He says it has long been established by Pasteur, Payen, and other experimenters, that a temperature of less than 130° C. is insufficient to destroy all trace of life if the germs or spores are not immersed in a liquid; this is a fact admitted on all sides. In that case it is not difficult to understand how, in the experiments of Dr. Child and Prof. Wyman, organisms have been found in liquids to which air only which had passed through red-hot pipes was admitted. The former took a bulb with two narrow necks or tubes, and containing the experimental liquid; one tube was connected by a cork boiled in water with a red-hot porcelain tube filled with pumice, and connected with a gas-holder; the other tube dipped into sulphuric acid: the liquid was boiled for ten or fifteen minutes, and heated air was made to pass through the apparatus till the liquid and flask were cool. "When the bulb is quite cool, the necks are sealed by means of a lamp." In Wyman's experiments an apparatus of much the same kind was used; but the liquids in four cases were boiled for from five to ten minutes in a Papin's digester, under a pressure of from two to five atmospheres, or at a temperature of 120° to 150° C. Large flasks of 500 cub. centims., and even 850 cub. centims. capacity were used, containing 17 cub. centims. to 50 cub. centims., or thereabouts, of solution, so that in some cases

only $\frac{1}{25}$ of their capacity was occupied; the air admitted was passed through red-hot iron pipes filled with iron wires. There is a similarity between these two sets of experiments; the flasks were not entirely in contact with the hot liquid. We see also, from the few words quoted from the description of Child's experiments, that a sufficient space intervened between the red-hot tube and the bulb to allow of the heated air becoming cool before it entered the glass bulb; it cannot be said, therefore, that the entering air was so hot as to destroy whatever living thing might be attached to the glass. The precautions taken, then, were not sufficient to render the experiments trustworthy, more particularly in the case of Wyman's work, because there he had an immensely large surface untouched by fluid, and naturally he obtained more results in favour of the view of evolution than any other experimenter. Bastian's own experiments are open to the same objection; in fact, it seems that much work has been rendered faulty by this neglect of bringing every part of the interior surface of the containing vessels in contact with the heated liquid. We have positive proof that such is the case when, as Bastian himself states, "it has long been known that a boiled fluid extremely prone to change will not yield infusoria if the vessel in which it is contained is filled with the fluid;" the commercial method of preparing cooked meat depends upon this to a great extent.

The Power of various Substances to destroy Protoplasmic and Fungus Life has been inquired into by Dr. F. Crace-Calvert, F.R.S., who has published a paper on the subject in the 'Proceedings of the Royal Society.' His experiments consisted in adding to a solution of albumen swarming with microscopic life, one-thousandth part of the following substances, and examining the result after one and sixteen days. The experiments were begun on the 20th Sept., 1871, the solutions being kept at a temperature of 15°—18° C. In the standard solution the amount of life and putrescence increased during the whole of the time. The first class includes those substances which completely destroyed the locomotive power of the vibrios immediately, and completely prevented their regaining it during the time the experiments were conducted:—Cresylic acid. The second class contains those compounds which nearly destroyed the locomotive power of all the vibrios present when added, and afterwards only one or two could be seen swimming about in each field:—Carbolic acid, sulphate of quinine, chloride of zinc, and sulphuric acid. The third class are those which acted injuriously on the vibrios on their addition, leaving only a small number retaining the power of swimming, but which allowed the vibrios gradually to increase in number, the fluid, nevertheless, containing less life after sixteen days than the standard putrid albumen-solution:—Picric acid and sulpho-carbolate of zinc. The fourth class includes those substances which acted injuriously at first, but permitted the vibrios to regain their former locomotive power, so that the fluid after sixteen days contained as much vibrio-life as the standard putrid albumen:—Chloride of aluminium, sulphurous acid and prussic acid. The fifth class contains those compounds which acted injuriously at first, destroying the locomotive power of most of

the vibrios, but which afterwards permitted the vibrios to increase more rapidly than in the standard albumen-solution:—Bleaching-powder, bichloride of mercury, chlorine-solution, caustic soda, acetic and nitric acids, sulphate of iron, and the sulpho-carbolates of potash and soda. The sixth class contains those compounds which exercised no action on the animalcules, either at first or after sixteen days:—Arsenious acid, common salt, chloride of calcium, chlorate of potash, sulphate of lime, bisulphite of lime, hyposulphite of soda, phosphate of lime, turpentine, and pepper. The seventh class includes those substances which favour the production of animalcules and promote putrefaction:—Lime, charcoal, permanganate of potash, phosphate of soda, and ammonia.

Structure of Normal and Varicose Veins.—The 'Lancet' of July 13th states that Dr. S. Soboroff has lately undertaken a series of researches on the structure of healthy and varicose veins, the results of which possess considerable interest. He had at his disposal eight specimens of varicose veins, which, with only one exception, were accompanied by pathological changes in the vena saphena. These were all capable of being arranged in two groups: in both the lumen of the vessels was greatly enlarged; but in one the walls were thickened, in the other they were attenuated. The preparations seemed to have been made with great care, some being silvered to show the epithelium; others subjected to the action of chloride of gold, chloride of palladium, and chromic acid. The principal fact that he has definitely ascertained is that a dilated venous trunk, even when possessing almost perfectly transparent walls, contains colossal muscular fibre-cells. These are so large that when first observed they were regarded as fasciculi of unstriped muscle, but more careful examination, aided by staining experiments, showing the well-marked outline of the cells and the rod-shaped nuclei, clearly demonstrating that fibre-cells alone were under the microscope. The adventitia of the dilated veins was thin and but sparingly supplied with vessels, and the epithelial lining of these attenuated vessels was apparently continuous and unaltered. In the thickened and dilated veins, on the other hand, the adventitia was hypertrophied, and the vessels distributed to it were large and numerous.

A Fossil Hydractinia.—Professor George Allman, F.R.S., in a communication to the 'Geological Magazine' for August, says, that among the very few known instances of fossil hydroids, the genus *Hydractinia* must be included. Under the name of *Cellepora echinata*, M. Michelin has described a fossil from the Subapennine group of Asti, and from the Superior Fallunian of Bordeaux and Dax.* M. Fischer has drawn attention to the fact that the *Cellepora echinata* of Michelin is really a *Hydractinia* encrusting a *Murex* or a *Nassa*, while he has himself added another fossil *Hydractinia* from the Upper Greensand of Mans.† This he found in the collection of M. Alc. d'Orbigny, where it encrusted numerous specimens of *Natica tuberculata*, d'Orbig., from that formation.

* Michelin, 'Icon. Zooph.,' p. 74, pl. xv., fig. 5.
† Fischer, 'Bull. de la Soc. Géol. de France,' 2me sér., t. xxiv., p. 689.

M. Fischer has assigned to this species the name of *Hydractina cretacea*, while to Michelin's species he has given that of *Hydractina Michelini*. To the two examples thus noticed by Michelin and Fischer, Professor Allman is enabled to add a third from the Coralline Crag of Suffolk. It occurs among some Coralline Crag fossils in the collection of the British Museum. It was found encrusting two specimens of *Purpura lapillus*, one from Orford, and the other from Gedgrave, in Suffolk. It covers with a continuous crust the shell over which it spreads, and has a minutely alveolar structure, with its surface thickly set with short blunt spines. The original chitine of the common basal expansion is entirely replaced by carbonate of lime. There cannot be the slightest doubt of this fossil being a true *Hydractinia*, and indeed it is impossible to find any characters which can separate it from the living *Hydractinia echinata*. From the mere fossilized basis, however, which is, of course, all that has come down to us, we should not be justified in asserting its identity with the living hydroid.

The Epitheca in Fossil Sponges.—Dr. B. Holl in an article of some length explains that in the fossil sponges some portion of the surface, especially externally towards the base, is frequently observed to be either without pores, or they are so minute as to be invisible, and the sponge then appears as though covered by a more or less smooth or slightly-wrinkled membrane, which has been regarded by D'Orbigny, De Fromentello, F. A. Roemer, and others, as analogous to the epitheca of the *Zoantharia*; and the occurrence of this epitheca has been held to be an additional evidence of the stony nature of the sponge skeleton. When examined microscopically, by means of thin sections, however, it appears that this epitheca is due to the filling up of the interstices of the superficial parts of the sponge, which, in the situations in which it exists, is finer and more condensed than elsewhere. This greater density at the surface may be seen in many recent sponges, the superficial portions of the tissues being closer and finer than that of the interior, which was formed during an earlier and more active period in the growth of the sponge. But either from having arrived at maturity, or at a period when the growth was temporarily arrested—for in some sponges the growth is intermittent—or, as appears sometimes to be the case, from some local cause, the tissue at the surface assumes a closer arrangement. Thus in the common *Halichondria panicea*, the surface over greater or lesser portions frequently presents a condensed appearance with scarcely any visible interspaces, the outer superficial portion being made up of a densely-matted layer of spicula placed for the most part parallel to the surface; and the same is true of many fibrous sponges, as shown by Dr. Bowerbank. In some of the fossil sponges a similar modification of the tissue at the surface appears to have obtained, especially in certain cup-shaped and cylindrical sponges, and in the calcareous fossils in which this has been the case, the interstices, from their extreme minuteness, are more or less filled with carbonate of lime. Thus a species of *Cupulospongia*, common in the gravel-pits of Farringdon, frequently presents on its interior a smooth surface, described by the late Daniel Sharp as a membrane. But if a number of individuals of this species be ex-

amined, it will be observed that although it sometimes completely lines the interior of the cup, it more often occurs only in patches; and that, while some of the interstices are blocked up, there are others that remain open, and this not as the consequence of friction or weathering, but as the result of fossilization. It is, however, in some of the sponges of the Oolite that we see this infilling of the interstices most distinctly, in some of the cylindrical forms especially, the whole of the sponge, except its summit, appearing as though invested by a sheath, but which, were it really of the nature of a true epitheca, as in the *Zoantharia*, it would be difficult to comprehend how the functions of the animal were carried on. It is more than probable that this structure is nothing more than the cast of the impression or mould of the outer surface of the sarcode of the sponge, perhaps slightly thickened, but it is not constantly present even in the same species. For example, there is a small cylindrical sponge, not unfrequent in the Coral Rag, at Bullington Green, near Oxford, in which more or less of this so-called epitheca is met with in some individuals, while the greater number show nothing of the kind. It appears, therefore, that the epitheca is sometimes only a result of fossilization, and is sometimes probably the cast of the outer surface of the sarcode which has left its impress on the mould; that it is absent in the earlier and growing stages of the sponge, and is not constantly present in the matured individuals of those species in which it occurs; and, moreover, that it sometimes results from the contact of foreign bodies, in consequence of the increased density of tissue which such bodies are apt to produce. Its value, therefore, even as a specific character, is not great.

Microscopic Fossils in Irish Chalk.—At a meeting of the Royal Irish Geological Society, on June 12th, Professor Hull, F.R.S., exhibited two slices of Chalk from County Antrim, showing under the microscope its essentially foraminiferous structure. On comparing the forms with those in Ehrenberg's 'Mikrogeologie,' Professor Traquair and the author were able to identify at least five genera, namely, *Rotalia*, *Textularia*, *Planulina*, *Nodosaria*, and *Globigerina*—this last named being, as in the Atlantic mud, the most abundant; and, in fact, constituting the greater part of the mass of the rock. The cross-section of the spine of an *Echinus*, and other forms of doubtful affinities, were also observed. The shells were nearly all preserved in calcite, and bound together by impalpable carbonate of lime. The slices were made by Mr. Jordan, of the Museum of Practical Geology. Mr. Hull called attention to the fact that the Foraminifera of the Chalk of the north of Ireland had scarcely received any attention from palæontologists. This, he considered, might be partly attributed to the unusually indurated character of the stone, which rendered it almost impossible to extract Foraminifers by washing. This quality, however, was favourable to the preparation of thin slices for microscopic examination; and Professor Rupert Jones had kindly undertaken to examine slices of the Antrim Chalk for comparison with those forms now under investigation by himself and Mr. Parker.

Microscopic Organisms found on Inland Ice.—Dr. Berggren has communicated a notice of these organisms to the 'Geological Maga-

zine,' August, and promises a fuller account in the 'K. Vet. Akademiens Ofversigt.' The notice constitutes part of Professor Nordenskiold's paper. It seems that one of the species of Algæ met with on the inland ice occurred in such vast quantities, that the surface of the ice throughout larger or smaller tracts was tinted with a peculiar colour. Two others seemed exclusively to belong to the fine sand, which is found either in the form of a thin covering on the surface of the ice, or as a more or less thick layer at the bottom of the pipe-like holes that appear in the surface. The first-mentioned species, occurring copiously, does not require any such substratum, but is found principally on the sides of ice-hills, where the water from the melting ice filtered itself out between the little inequalities of the surface. The most copiously represented species has the form of a short thread, not spreading out in branches, but consisting of a single row of cells; the number of cells in each thread is 2, 4, 8, or at most 16. Threads of 4 and 8 cells are most common. The species very frequently appears only as a single cell. The threads are usually a little bent, sometimes, when the number of cells is 16, forming a complete semicircle. The number two or its multiples taken as the standard for the number of cells in the separate threads is accounted for by the regular continuous bisection of the cells, whereby their propagation proceeds The connection between the cells is the looser the older the partitions become, as the older membranes assume a looser consistence. In a thread of 16 cells, the connection between the eighth and ninth cells is soon broken, and in the two threads thus resulting the connection between the fourth and fifth cells is weaker than that between the second and third or the sixth and seventh. The threads therefore often lie bent at an angle. The diameter of the cells is 0·008—0·012 mm., and their length 0·016—0·040 mm. Individual cells may sometimes attain a length of 0·065 mm., and a breadth of 0·015 mm., whereas a great number of other single cells are met with of very small dimensions, from spherical forms of only 0·006 mm. diameter to those of ordinary form and size. As the ends of the cells, where they are joined together, are rounded, there is, of course, a contraction between them, which becomes more and more conspicuous as the connection between them is loosened by time. The membrane is thin and hyaline, and its outermost layer (the remnants of the membranes of the mother-cells altered after division) is of an almost slimy consistence, whereby the cells are for some time kept together. The contents of the cells are in part concealed by a dark purple-brown colouring matter, which in dried cells is immediately drawn out on wetting them. The centre of the cells is occupied by an oblong or cylindrical mass of chlorophyll, of somewhat irregular contour, in the extremity of which two kernel-formed rounded bodies are imbedded, which in general cannot be perceived by the eye till the colouring matter has been removed by means of reagents. We sometimes meet with four such bodies in one cell, sometimes but a single one: the former a result of accidentally checked division of the cells; the latter of such division having lately taken place. In the liquid of the cells a number of small grains are found, which are

for the most collected round the periphery of the cell or at its
"Judging from the construction of the cells, and the manner of
multiplication, the alga before us would appear to belong to the
jugatæ; but as I have not succeeded in discovering fructificatio
it, it would be rash to decide to which genus it is to be refe
The thread-like rows of connected cells agree with the *Zygnema*
whereas, on the other hand, an unmistakable similitude to the *De*
diaceæ, especially *Cylindrocystis*, and the nearly-related genera
indicated by the strongly-marked divisions into multiples of two,
by the tendency of the rows of cells to fall asunder, as far as
destructibility of the uniting cell-membranes permits, into parts
sisting of cells united in pairs, which however is seldom possibl
consequence of the greater energy possessed by the power of multi
ing the cells. As the above-mentioned small single cells, which o
in great numbers, are much less in diameter than those cells w
arise from the bisection of the threads, they have perhaps a diffe
origin from these latter, although the researches which I have hith
been enabled to devote to this subject have not furnished any il
tration of it. Were these daughter-cells arising from the divisio
the sporæ, if the above-mentioned supposition with respect to
systematic place of the species be correct, the stadium of copula
or sporæ, in some period of its development, ought to be found.
rare forms of peculiarly constructed cells perhaps ought not t
passed over in silence. I have sometimes found the extreme cell
thread considerably more swelled than the others, more ellipti
form, also provided with a thicker membrane, and with the cont
of the cell more coarse-grained. I once found one of the middle c
in a thread thus transformed, and on two occasions I have met
single cells of the same kind. I also once met with a cell of
peculiar construction. It had the usual form, but was unusually la
with a long mass of chlorophyll, as usual, in the midst, and
granular matter grouped rather towards the ends of the cell. I
were found about twenty larger or smaller spherical bodies. F
of these lay arranged at each end of the cell, and were almost enti
opaque, of a dark brown colour, and in appearance much resemb
the smaller cells of *Protococcus nivalis*. The others were transluc
with sharply-defined contours. As our knowledge of the natur
these bodies is confined to what is here stated, the fuller explana
of their significance must be reserved for a future investigation.
places similar to those in which this species occurs, and often in c
pany with it, *Protococcus nivalis* was met with. Amidst the
gravel upon the ice, but to a trifling amount, small green cells
found, sometimes united in little groups, sometimes isolated, w
appeared to belong to *Protococcus vulgaris*. *Scytomena gracile*, on
other hand, is everywhere met with in great profusion, wherever
gravel either lies in thinly-scattered grains on the surface of the
or forms more or less thick layers. The threads lie either alone
united in small bunches, as they join together at the lower part,
bend backwards higher up. They are pretty stiff, S-shaped, or fo
ing a curve of several undulations, and yellowish brown in col
Their length is very various, their breadth generally about 0·009 m

Observations on Collema.—These observations are continued by Mr. W. Archer in 'Grevillea' for August. The author concludes a paper of some length with the following three suggestions:—1. To suggest the possibility that, if we may conceive Dolichospermum, &c., excluded from the "gonidia question" as forming special fruit (that is, "spores"), so might we regard Nostoc as excluded, though its formation of spores be so extremely rare. Seemingly, indeed, the formation of spores by an algal species, supposed to become occasionally *lichenized*, is not a reason *against* the hypothesis as viewed by Schwendener—he only assumes that such an example of the alga surrenders, or leaves in abeyance, its tendency to the production of spores. 2. To suggest that there are veritable lichens which live submerged, and produce their apothecia. I presume, however, it might be replied that such may have received their inoculation by the parasite during some season of drought, when the *alga* lay "high and dry." 3. To suggest the possibility that the spores of Collema, if "sown" on some other gelatinous substratum, besides that of Nostoc —say, for instance, a Palmella or Mesotænium—might equally well germinate, penetrate therein, and develop a hypha. There seems, I venture to think, no *a priori* reason against this supposition—inside the Nostoc, the "reserve-stuff" of the spore being exhausted, and the chains of Nostoc filaments admittedly intact, the only next immediate source of nutriment for the growing hypha would, I imagine, in the experiment of Reess, appear to have been the "Nostoc-jelly." Now a "Palmella-jelly," or a "Mesotænium-jelly" (both aërial, that is, not under water), would seem in themselves to be possibly just as likely to afford the requisite *pabulum* for the germinating and growing Collema-spore. If this conjecture should be borne out, which I would indeed put with all diffidence, what would be the result of Reess's experiments, or, rather, what proven thereby? Such a combination (*if* capable) with a Palmella or a Mesotænium would not be "Collema," because it would not have "nostochaceous" gonidia, nor the characteristic periderm. If, indeed, we might for a moment assume that which direct experiment alone could prove, and a germination of spores and penetration of the hypha of Collema with a Mesotænium effected, such a "lichen-thallus" would be, I apprehend, unprecedented—a hypha *like* other lichen-hyphæ, no doubt (but *known* to be that of a Collema), with large elliptical or cylindrical "gonidia" containing a central "chlorophyll-plate," and which would probably (in free nature at least) go on and produce *zygospores!*

NOTES AND MEMORANDA.

Death of Dr. Stimpson.—We regret to learn from 'Silliman's American Journal' for June, of the death of Dr. William Stimpson, which occurred in the evening of the 26th of May last. He has recently returned from a dredging expedition across the Gulf of Mexico, and had been very ill since his return. He has for many years been well known as one of the leading zoologists of the country, and has devoted himself chiefly to marine invertebrata.

What Work can be done with the Microscope. — Mr. Charles Stodder writes to the 'Boston Journal of Chemistry,' June, to explain how an observer may study fermentation under, or rather over, the microscope. He says that the work of "breaking up organic compounds" can ever be understood, explained, or seen under the microscope, is very problematical, but as yet there is no evidence before the public that the research has been conducted with the best modern resources of the microscopist. With one instrument of investigation, the inverted microscope, the object will be not under the microscope, but the microscope under the object. With this instrument the evolution of the gas will interfere much less with the observation of the processes going on. Then, most of the published accounts of investigations of this kind, in France and Germany, appear to have been made with instruments of the construction of ten, fifteen, or twenty years ago, with powers of 400 to perhaps 1000 diameters. Now, *immersion* objectives of $\frac{1}{16}$ to $\frac{1}{50}$ of an inch focus, giving with ease powers from 1600 to 10,000 diameters, are in constant use. It is not known that such instruments have ever been applied to this class of studies. They have given remarkable results in other *investigations*, and until they have been applied to the yeast *plant*, "morbid or germinal growths," &c., we do not and cannot know all that may be learned about them. It is with such instruments of the *better* quality that these organisms will have to be studied. Interest in microscopy is rapidly growing in this country. Hundreds of young *men have* procured their instruments. Here is a branch in which almost everything is yet to be learned, and fame and honours will be the reward of those who add to human knowledge. Very few European *microscopists*, when publishing their results, ever describe the instruments they work with; as a general thing they merely say the object was magnified so many diameters, forgetting, or often not knowing, that 500 diameters with one instrument may be vastly inferior or superior to the same power with an instrument by another maker. Astronomers always record and publish the instrument *their* observations are made with. The microscopist ought to do the same. He should give the maker of his objective, its angular aperture, and *real focus*, not merely what the maker sold it for; for it is now a well-known fact that the instruments of some of the most renowned makers are really very different things from what the makers call them. In fact, in the

words of Dr. W. B. Carpenter, "$\frac{1}{4}$'s are often $\frac{1}{8}$," and in the words of Mr. F. H. Wenham, "$\frac{1}{3}$'s are often $\frac{1}{5}$ or $\frac{1}{10}$, and sometimes $\frac{1}{12}$!" Thus the makers may obtain a reputation for the performance of a low power to which they are not entitled.

The Hunterian Museum and its Microscopical Collection.—In his annual report to the Council of the Royal College of Surgeons, Professor Flower, the Conservator, states that the additions to the Pathological Collection for the past year have been eighty-eight in number, as compared with sixty-two during the previous year, and that the microscopical characters of all recent specimens sent to the College and thought worthy of preservation have been carefully described and delineated by Mr. Goodhart, the Pathological Assistant, who, by the great zeal with which he has performed the duties entrusted to him, appears fully to deserve the eulogium of the Conservator.

CORRESPONDENCE.

[By a mistake of the printers, Mr. Tolles' first letter was "crushed out." We now give it with his second one.—Ed. 'M. M. J.']

Angular Aperture of Immersion Objectives.

To the Editor of the 'Monthly Microscopical Journal.'

Boston, *June* 25, 1872.

Sir,—I wish to announce that I have lately subjected high-angled objectives to the tank method proposed by Mr. Wenham for ascertaining angular aperture, the objective being immersed (front surface dipped) in balsam. The result is very different from that which Mr. Wenham declared must happen.

Let this note appear, if possible, in your August issue proximo, and I will describe my results and method particularly in ample time for appearance in your Monthly for September next.

With my account I will send *an objective*, that my claimed results may be verified or disavowed.

Very respectfully,

Robt. B. Tolles.

To the Editor of the 'Monthly Microscopical Journal.'

Dr. Henry Lawson, Boston, *July* 15, 1872.

Sir,—In my note of June last, I declared my purpose to send an objective for test of angle in balsam.

Professor Markoe, of this city, sails to-morrow, per steamer 'Hecla,' from this port; and as an objective suitable to my purpose, made in 1869, has been very obligingly placed at my disposal by another gentleman of Boston, I venture to forward it to your care.

Without any wish to thrust the matter upon Mr. Wenham's further attention, I still would like Mr. W. to be notified that the objective is forwarded for the purpose of trial by him of its angle in balsam.

Adjusted at "$10\frac{1}{2}$" closed from open point (which is the intermediate point of the whole adjustment), I make the angle in balsam, using the tank as proposed by Mr. Wenham, 93°, or possibly 95° by sunlight.

With a view to help to a conclusion, perhaps you will have no objection to giving Mr. Wenham notice of receipt of the objective, and that it is for his use, for the purpose aforesaid, if he chooses to subject it to the test.

N.B.—I easily remove the balsam with a little cool oil, and no harm to lacquer or glass.

With much respect,

40, Hanover Street (Room 30). Robt. B. Tolles.

July 19th, 1872.

P.S.—Further remarks on account of experiments on angular aperture shall be forwarded per steamer of 23rd inst. from this port. —R. B. T.

[The Editor begs to say that he has not received any objective from Mr. Tolles up to the date of August 13th.]

PROCEEDINGS OF SOCIETIES.

Oldham Microscopical Society.

The annual meeting of the above Society was held at No. 2, *High* Street, in May last. The Secretary reported that last year the Society had 23 members, and a balance of 1*l.* 15*s.*, whilst this year it had increased to 43 members, and a balance of 6*l.* 10*s.* 6*d.* The librarian's report stated that the Society possessed 21 vols. of standard works on the microscope and the various scientific subjects to which it has been applied, and it was decided to expend the balance in the purchase of further standard works of approved excellence. Dr. Thomson was elected President for the ensuing year, and officers to serve under him were also elected. The Secretary submitted the following programme for the coming year:—Four excursions, available for members and their friends, during the months of June, July, and August, *viz.* to Tandle Hill, Marple, Littlebro', and one in connection with the Scientific Students' Society, of Manchester, place not decided. Papers on the following subjects, *viz.* "Pond Life," Mr. R. Horne; "The Microscope as Applied to the Detection of Disease," Dr. George Thomson; "Animal Tissues," Dr. Fawsitt; "Reproductive Organs of the Lichens," Mr. Charles

Walters; "Lichens," Mr. Brittain, of Manchester; "Fungi," Mr. Pullinger; "Fossil Botany," Mr. J. Nield; "Internal Structure of Fossil Plants," Mr. J. Butterworth; "Vegetable Tissues," Mr. J. R. Byrom; "Closing Address," the President. The retiring President, Mr. James Nield, read a very able *résumé* of the Society's proceedings during the past year, of which the following is an abstract:—After congratulating the Society on its position, financially and otherwise, and adverting to its excursions, he passed to what he described as the real work of the Society, namely, the useful series of papers delivered during the session, and which he characterized as being every way worthy of the Society over which it had been his honour to preside. Mr. Horne's very interesting paper, given in October, "On Infusoria," was a valuable acquisition, and invested those lowliest of beings which people our ponds in such myriads with an interest and importance which rendered the subject a most pleasant and profitable one to investigate. He would therefore pass on to that given in December by Dr. Fawsitt, "On Blood," illustrated by diagrams, and prepared slides of blood corpuscles, exhibiting many modifications of those disk-like living bodies as they occur in various types of animal life, and likewise of those singular objects, "blood crystals," which result from a breaking up of the red corpuscles, and which in the human blood take a prismatic form, and in other creatures lower in the scale of being, modifications of the prism. The paper was of an exhaustive character, as far as scientific research has hitherto enabled the student to advance. One of the results of this Society, numbering amongst its members several gentlemen holding high and honourable positions in the medical profession, was made manifest in this paper. Dr. Fawsitt brought his professional knowledge and experience to bear on the subject of his paper, adding a medico-physiological feature to its other important peculiarities. The legitimate work of the microscopist is, undoubtedly, practical and utilitarian in character. Yet the student can rightly claim to enter into the realms of theoretical speculation. Dr. Fawsitt prudently, I think, confined himself principally to ascertained facts. He made no attempt at generalization, or from an aggregation of accepted data to make abstract deductions. Yet the moment was propitious. And let me admit that during the progress of the reading, with the diagrams and prepared slides of blood and its crystals before me, the complete, or nearly complete, identity in form and chemical composition of the blood of all the vertebrata, and the intimate connection and relationship of all these varied animal organisms, seemed to me to be clearly established to the extent of the subject of the paper. When we look upon the colourless corpuscles of the human blood, and remember their chemical composition, their ever-changing form, and the purpose they serve in the economy of the human body, the conviction is forced upon us that man, the highest and grandest work of the Creator, is connected by these peculiar links in the chain of being with the humble and lowly rhizopoda, the latter finding congenial conditions in water, the former floating in the plasma of the human blood. The naked and protean amœba, and the sarcode clothed in ornate foraminiferous test, are but free members of a family, which are

aggregated and communistic in the sponge, and in the blood of man, and all intermediate creatures. "The Principles Involved in the Construction of the Microscope" was the subject of our next paper, and was dealt with by Mr. Waddington, in January. It seemed to me at the time that this was a very proper subject to bring before our Society, as many of our members, myself among the number, are insufficiently informed in the practical application of the microscope to the study of our department of natural history; still less are we acquainted with the optical principles involved in the construction of that all-important instrument. Mr. Waddington, who has made the microscope his study during many years, gave us the benefit of his extensive knowledge, and the result was a clear and instructive paper on the theoretical principles which find their expression in the microscope. There is, perhaps, no department of science that has made more important progress, in so short a space of time, as the study of the theory of light as involved in the manufacture of object-glasses and the actual building up of the instrument itself. The subtle intellect and practical genius of the various makers, both Continental, American, and English—and pre-eminently that of Messrs. Powell and Lealand, of London, aided by the wonderful manipulative skill of Col. J. J. Woodward, of America—have been concentrated on this labour, and the results have been the exploding of ideas and errors that have nothing to recommend them but their age. But a few short years ago the resolution of lines, now rendered easy of accomplishment by our cumulative knowledge and practical thrift, would have been considered, and were, beyond the limits described by our then knowledge of the principles of light. In February there was an unavoidable hitch in our programme, and no paper was ready or work recorded in the Secretary's minutes. In March, however, Mr. Walters gave us a very able paper "On the Reproductive Organs of the Fungi." This was very properly followed, in the month of April, by an equally valuable one "On Microscopic Fungi" by Mr. Pullinger. As may be seen by the titles of the papers, each gentleman directed his labour to a separate part of the same very difficult subject. The undertaking was a formidable one, and no doubt taxed their mental energies, and drew largely on their botanic lore for fungal histology. I may venture to say, without fear of contradiction, that their ability proved itself to be equal to their self-imposed task. The study of fungi is still a difficult one. Many facts in their life-history are yet wanting. Many phenomena exhibited by them remain unexplained, and much labour and patient investigation are required to elucidate their manifold wonders. Our vision is limited, the field is large, the horizon is clouded, and many phenomena are dim and ill-defined in outline. The neglect in which the study of these objects has been left is doubtless explained by their apparent want of those points of beauty of form and colour which render some other departments of botanic study so attractive. Many, too, are difficult to approach on account of their essentially microscopic character, yet botanists who have made these objects the subject of their earnest inquiry aver that they are most interesting histologically, and that they possess a fair share of beauty

as understood in an æsthetical sense. Apart, however, from the difficulty attending their study, and the repulsiveness and superstition with which ignorance has wrongfully invested them—apart from their want of beauty or otherwise—it is of vital importance to man, in an economical sense, that these hitherto obscure organisms should be understood, in order that they may be combated. "Their name is legion." Their power for good or evil is immeasurable. Between them and man the battle is chronic. The struggle for existence and ascendency is increasing as man becomes more civilized and varied in his tastes. No object of man's solicitude is safe from their attack. The cottage and the palace are equally in danger. The stores of priceless treasures that the latter contains form no ground of exemption from ruin. Man's choicest works of art, upon which his brain has laboured and his wealth been lavished, are marked by the fungi, as a sheriff's officer marks an insolvent debtor's goods, for destruction. Thousands of objects, whether placed high in the air or deep down in the soil, are still within the reach of this ubiquitous race. Some spread themselves over our fruits before we have time to gather and use them; others fill the cracks and crevices of our bread and cheese with the pellucid forests of pigmy trees, bowed down with the abundance of the fruit they bear. Our jams, pickles, and preserves, if not watched and defended with the greatest care, at some seasons are immediately stamped with their broad arrow as their own. "When our beer becomes mothery," says Dr. Badham, "that mother of mischief is a fungus. If pickles acquire a bad taste, if ketchup becomes ropy and putrefies, funguses have a finger in it all. Their reign stops not here, they even prey one on another. The closed cavities of nuts afford concealment for some species; others, like leeches, stick to the bulbs of plants, and suck them dry. Some pick timber as men pick oakum. More than one species has a fancy for the hoofs of horses and the horns of cattle, sticking to them alone." The viscera of our domestic fly is liable, in autumn, to break out into vegetable tufts of fungous growth. In spring I have myself disinterred numerous caterpillars with a species of clavarias larger than themselves growing from their heads. And the microscopist has often to lament that one or more of his once most valuable objects are, by the depredations of some species, rendered quite worthless. Thus we see that man cannot well treat with indifference these simple living structures. They are as varied in form as they are ubiquitous in habit. They are as dangerous from their numbers as from the insidiousness of their attack. Man's house, his clothing, and his utensils; the air he breathes, the water he drinks, and the food that he eats are claimed by fungi, in some shape, as their inheritance; nay, they even enter his mouth, and settle upon his teeth, thus boldly putting their heads into the lion's mouth. Mr. Walters and Mr. Pullinger are the Castor and Pollux of our Society, and, armed with the microscope, and that still more potent weapon, human thought, they have taken to the field against their numerous foe, and with twin-like unanimity they have undertaken to reconnoitre and examine the enemy's position and strength, to know and to conquer him. In concluding, let me thank

you, the members generally, for your regular attendance at the monthly meetings, and for your patient attention during business, which has rendered my duties as President light and easy; and let me thank the retiring officers specially for the manner in which they have fulfilled the duties immediately under their care, and assisted me in my pleasant labours, during the official year which this evening brings to a close.

Brighton and Sussex Natural History Society.*

February 15th.—Microscopical Meeting. Mr. Hollis, President, in the chair. Mr. T. H. Hennah read a paper "On the Palates of Mollusks."

Mr. Hennah prefaced his remarks by stating that he should regard the palates of mollusks as objects especially calculated to foster a true spirit of microscopical inquiry. They might be briefly described as membranes, studded more or less with teeth, found in the buccal cavity of univalve and shell-less mollusks. While the arrangement of the teeth was always symmetrical, the palates appeared to fulfil several functions, in some mollusks acting as a gizzard only, being supplemented by strong cutting mandibles, as in snails, cuttle-fish, &c.; in others acting both as a rasp and as a gizzard. In some the provision for wear and tear was prodigious: the palates of the limpet and periwinkle being longer than the animals themselves; while in other cases they were comparatively small and out of proportion, as compared with the animals. Thus the palate of the cuttle-fish was about the same size as that of the common snail.

As elsewhere, in the animal kingdom, specially-adapted contrivances were found, so also was the palate of the mollusk adapted, in its form, position, and arrangement, to the special habits of each species. In this variety, apart from its scientific interest, lay an inducement to go on with the examination when it was once begun. He well remembered dissecting out a palate of a common snail—his first dissection and almost his first microscopic preparation—and he could safely say he was grateful for the influence on his pursuits thus exercised by the silent tongue of this poor snail. Whelks, periwinkles, slugs, &c., were its successors, and though he found each had its palate covered with brilliant teeth, different methods of dissection were required for each; and thus a foundation was laid for a knowledge of the comparative anatomy of different species of the same family. He thought nothing gave a sounder teaching in Natural History than the thorough examination of any one organ, such as the "lingual membrane" of the mollusca. The variety was very great, but yet there was a family likeness running through the palate of many allied species.

Attempts had been made to base a classification of the univalve mollusca on the form and disposition of the teeth on the lingual membrane. Other characters of greater prominence had been found, however, to be inconsistent with its complete adoption, and although of

* Report by Mr. T. W. Wonfor.

much value in assisting the definition of species, having little external to indicate their difference, the dentition of the mollusca occupied a subordinate place in their classification. At the same time it was so characteristic, that any definition of species must be held to be imperfect without a figure or formula expressing the arrangement of the teeth. Instead of making excerpts from books, or merely quoting the systematic classification of lingual membranes which had been adopted, he would mention some books in the library containing the best information on the subject:—'Microscopical Journal,' vol. i., p. 170, 1853, "Teeth and Tongues of Mollusca," by Dr. J. E. Gray; 'Intellectual Observer,' vol. v., p. 67, 1864, "Dentition of British Mollusca," by Rev. G. Rowe; 'Transactions of Royal Microscopical Society,' July and October, 1868, "The Lingual Membrane in Mollusca, and its Value in Classification," by Mr. Jabez Hogg.

He strongly advised *dissection* as the means of obtaining the palates, as the experience so gained would be valuable in other investigations; but there was a shorter method, which he had been obliged to adopt with such small animals as *Balea fragilis* (about $\frac{1}{8}$th of an inch long), or with larger, when many had to be prepared. It was simply to cut off the head, or even to take the entire animal, and boil it in *liquor potassæ*, when all the soft parts would readily dissolve away, leaving the palate, easily to be recognized by its brilliant teeth, separate and entire, and requiring only to be well shaken with water in a test-tube to free it from extraneous matter. After well washing, the palate should be placed in diluted glycerine before mounting in glycerine jelly, the best medium for the purpose. He had brought down a good series of palates for examination, and would dissect for practical demonstration the palates of such mollusks as he had been able to obtain.

Mr. Wonfor, while thanking Mr. Hennah for his paper, was glad he intended giving practical instruction in dissection, for he was indebted to him for a valuable lesson in palate dissection some years ago; in fact, a common whelk, dissected on that occasion, and which he had brought with him, made for Darwin, as one tooth in each row had a double instead of a single crown. Some eight years ago he communicated Mr. Hennah's plan of boiling in potash to the 'Intellectual Observer,' giving him credit for the simple plan of readily obtaining palates; and was amused, some five years afterwards, to see it quoted from an American journal as a novel American idea. He mentioned this, for whatever credit was due for the suggestion belonged to Mr. Hennah.

The meeting then became a conversazione, when Messrs. Hennah, Glaisyer, and Wonfor exhibited numerous palates, including those of the whelk, limpet, periwinkle, snail, black slug, a vegetable feeder, testacella, a carnivorous slug, trochus, haliotis, cuttle-fish, aplysia, coris, clausilia, *Balea fragilis*, *Cyclostoma elegans*, &c.

During the evening Mr. Hennah gave practical instruction, by dissecting out the palates of snails, whelks, and periwinkles.

March 28th.—Microscopical Meeting. Mr. J. J. Sewell, Vice-President, in the chair.

Mr. Wonfor announced the receipt, for the Society's herbarium, from Mr. G. Davies, of another lichen new to Sussex, *Lecidea uestula*, found by that gentleman near Billinghurst, and previously found by the Rev. J. Crombie near Lyndhurst, New Forest; and for the cabinet, from Mr. T. H. Hennah, of the palate of *Tritonia Hombergii*, said by Forbes and Hanley to be rare. A couple were taken alive, inside a scallop, to Mr. Hennah, who supposed they were dredged up with the scallops, as they were said to be inhabitants of deep water, and to frequent scallop-beds.

Votes of thanks were given to the donors.

Mr. Hennah, remarking on the minutes of the previous meeting, said he wished to qualify what he had stated about the potash mode of obtaining palates, which was admirable when comparison or identification was the object, but for palates which were to be mounted, no plan was better than dissection.

Mr. Wonfor announced that their friend Mr. T. Curties had very kindly sent down for examination a box of slides, including some very choice diatoms, mounted by Möller, and which were from a collection of Möller's, from Baker's, Holborn. He had brought down for distribution a number of specimens of *Cyclostoma elegans* and *Clausilia nigricans;* a couple of packets of loam, one from above and the other below the Wealden building-stone, and sent to Mr. Onions by Mr. Grantham, who wished some of the microscopists to examine it for vegetable substance. That afternoon he had received a letter from Mr. Gwathin, and a bottle containing living creatures taken from the supply cistern of the kitchen boiler of Mr. C. Buckwell's house. It appeared "that since an alteration in the connecting pipe of the boiler and supply cistern, by which the temperature in the latter was reduced to lukewarm, these creatures from time to time made their appearance. The cistern was of cast iron, with tightly-fitting metal top. At first a little fine dust was seen on the surface, and, in a day or two after, the creatures themselves." Before coming down he had examined some, and found they were a species of Thysanuridæ (Spring-tails), which lived on the top of water. He fancied they were conveyed to the supply cistern from without. If the members present would look at one under the microscope, they would see the peculiar spring under the tail, whence the name. One evening in the previous week, while examining "battledore" scales, to see whether he could confirm Dr. Anthony's idea of the marking being tubercles on the ribs, he fortunately got some scales to stand on end, when he was able to see the tubercles standing out clear from the ribs. He had made a cork model of what he had seen, which illustrated their knobs, and pedicles, but not their bases. He was satisfied Dr. Anthony was right in the main; but scales taken from fresh-killed specimens should be examined, as in drying they had a tendency to become flattened.

Mr. F. E. Sawyer exhibited a block of indurated Portland stone, which was found by a mason in cutting a stone step, and pronounced by him to be a flint. He showed it to Mr. Wonfor, who considered it

indurated Portland stone. With a lens its oolitic nature could be easily made out. He had prepared some of the loose chalk-like particles, and had obtained portions of corals and sponges.

Mr. Hewell exhibited disintegrated granite, from the shores of Lake Erie, under the form of fine sand.

The meeting then became a conversazione, when, in addition to the objects mentioned above,

A number of very interesting objects were exhibited by Messrs. Sewell, Hennah, F. E. Sawyer, R. Glaisyer, and Wonfor.

Queekett Microscopical Club.

The Seventh Annual Meeting was held at University College, July 26, 1872, Dr. Lionel S. Beale, F.R.S., President, in the chair. The report of the Committee, read by the Secretary, congratulated the members upon the satisfactory condition of the club, detailed its progress during the past year, showed the number of donations made to the library and cabinet, with the number of members on the list; and referred to the publication of catalogues of the slides and books, the 'Quarterly Journal,' the excursions, the annual soirée, and the special services rendered by certain gentlemen named. The Treasurer's report showed a satisfactory balance-sheet, from which it appeared that the income from all sources had been 272*l.* 5*s.* 1*d.*, and that there was a balance in hand of 12*l.* 7*s.* 2*d.* Unanimous votes of thanks were passed to the Council of University College for the highly-valued privilege of meeting in the library of that building free of charge; to the President, for his valuable services during his two years' term of office; and to the Committee for their efficient conduct of affairs. The President then read the Annual Address, which was listened to with the greatest attention, and enthusiastically applauded by the meeting. The proceedings terminated with the election of officers and the installation of Dr. R. Braithwaite, F.L.S., F.R.M.S., as President of the club for the ensuing year.

Margate Microscopical Society.*

The June meeting of the Society was held at the Church Institute Margate, on the 4th instant, the President, Dr. Pittock, F.R.M.S., in the chair.

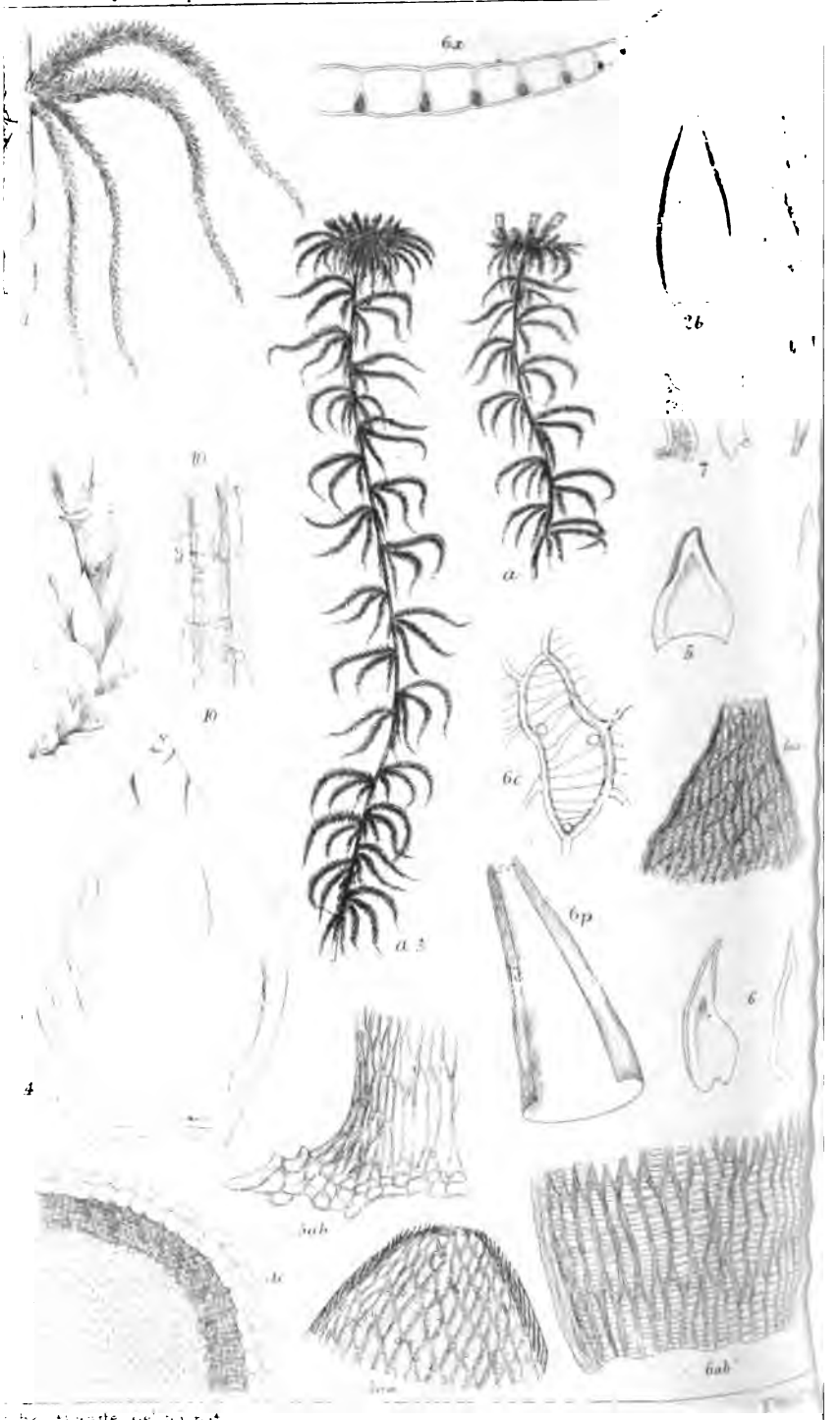
Some donations to the library and cabinet having been announced, and votes of thanks accorded to the donors, an animated discussion took place on the paper read before the Society by Mr. B. F. Kyngdon at the last monthly meeting "On Parasites," and this was followed by the reading of a paper by the President "On Pollen," amply illustrated by mounted slides and diagrams.

The members of the Society have recommenced their occasional excursions in the Isle of Thanet. A very pleasant one took place on the 8th instant, when some of the chalk rocks were visited, and many

* Report furnished by Mr. W. Lane Sear.

objects of interest collected. A pond collecting excursion to the neighbourhood of Stourmouth was announced for an early date. We understand that the President and members are at all times happy to meet with members of other microscopical societies who happen to be visiting at Margate, and any information as to excursions, &c., will be gladly afforded on application to Mr. W. F. Hunter, Hon. Sec., or to Mr. W. Lane Sear, Hon. Librarian, who has placed his drawing office (he is a local architect) at the disposal of the Society, and the members meet there weekly to work with the microscope and mount specimens.

The Margate Microscopical Society has been two years in existence, and numbers above 40 members.



Sph. neglectum.

THE

MONTHLY MICROSCOPICAL JOURNAL.

OCTOBER 1, 1872.

I.—*On Bog Mosses.* By R. BRAITHWAITE, M.D., F.L.S.

4. Sph. neglectum Ångstrom.

Öfver. Vet. Ak. Förhandl. XXXI, p. 201 (1864).

PLATE XXX.

Syn.—Lindberg in Journ. Lin. Soc. XI, p. 468 (1870). Hartman, Skand. Fl. 10th edit. v. II (1871). *S. subsecundum* C. Müll. Synop. v. II, p. 539; the American specimens from Sullivant. *S. contortum* Sulliv. Mosses Un. St. p. 11, No. 3 (1856). *S. curvifolium* Wilson MS.—Hunt, Mem. Lit. and Phil. Soc., Manchester, Ser. 3, v. III, p. 233 (1867).

Dioicous.—4–6 in. high, pale yellowish brown, the apex yellow-green. Stem solid, simple or forked, the *bark pale, of 2–3 layers of cells.* Branch fascicles more or less crowded, of 3–4 branches, of which 1–2 are divergent, the others pendent, but not appressed to stems, not much elongated, nor acute. *Cauline leaves small, ovate, cucullate at apex, at last minutely fimbriate,* patent or reflexed, the basal hyaline cells utricular, the middle and lateral very narrow, the apical rhomboid, with scarce any fibres or pores.

Ramuline leaves laxly incumbent, *subsecund, recurved at point,* especially on the lower part of the divergent branches, ovate, shortly and bluntly acuminate, the point with 3–5 teeth, margin incurved in the upper ⅓, rather broadly bordered; hyaline cells serpentine, elongated, filled with annular threads, pores many, small, arranged

EXPLANATION OF PLATE XXX.

Sphagnum neglectum.

a.—Female plant. *a* ♂.—Male plant.
1.—Part of stem with a branch fascicle.
2.—Catkin of male flowers. 2 *b.*—Bract from same.
3.—Capsule and peduncle. 4.—Peduncular leaf.
5.—Stem leaves. 5 *a a.*—Areolation of apex. 5 *a b.*—Areolation of base of same.
6.—Leaves from middle of a divergent branch. 6 *a a.*—Areolation of apex. 6 *a b.*—Areolation of base of same. 6 *p.*—Point of ditto. 6 *c.*—Cell from middle × 200. 6 *x.*—Section of same.
7.—Intermediate leaves from base of a divergent branch.
9 *x.*—Part of section of stem.
10.—Branch denuded of leaves.

principally at the outer wall of the cells, chlorophyll cells trigonous, projecting between the hyaline cells at the back. Basal intermediate leaves broadly bordered, with small rounded auricles. Retort cells of the branches straight, without any projecting neck. Catkins of male flowers subclavate, the bracts slightly secund, oblong, truncate, with several obtuse teeth, narrowly bordered, the areolation as in the branch leaves. Fruit seated in the coma, but little exserted, *peduncular bracts oblong, scarcely bordered, innermost large, convolute, emarginate,* the hyaline cells rather small, lower narrow, upper suboblong, without many threads or pores.

Hab., deep bogs, usually with *S. subsecundum*, for which it has probably been often overlooked.

Near Holyhead and at Vale Royal, Cheshire (Mr. Wilson), Loch Kandor, Braemar, 1868 (Mr. Hunt).

Closely resembling some forms of *S. subsecundum*, but distinguished by the pale bark of usually two layers of cells, and by the peduncular bracts; the fruit, however, is very rare, and the drawing is from an American specimen, collected in Cranberry Marshes, Northern Ohio, by Sullivant in 1849, for which I am indebted to my valued friend Prof. Lindberg. The male plant is from specimens communicated by the late Mr. Wilson.

II.—*Reply to "Further Remarks on Tolles' ⅛th and Powell and Lealand's Immersion $\frac{1}{16}$th."*

By Dr. J. J. Woodward, U.S. Army.

The July number of this Journal contains (page 13) an article entitled "Further Remarks on Tolles' ⅛th and Powell and Lealand's $\frac{1}{16}$th" (by Mr. E. Bicknell), which is so full of erroneous statements with regard to some of my work, that I cannot allow them to pass uncontradicted.

I do not feel called upon to enter into a discussion of Mr. Bicknell's arguments, but merely discharge the disagreeable duty of showing that his premises are not true.

He gives measurements of two of my positives on glass, each of which represents two frustules of *Amphipleura pellucida*, the one made by the first, the other by the second of the objectives above named. To these positives certain magnifying powers were attached.

He then proceeds to assume that the two positives were taken by each objective, without eye-piece, at 48 inches distance, and that the frustules represented had each 95 striæ to the $\frac{1}{1000}$th of an

inch. In fact, neither assumption is true. The positives were copied from the original negatives in an ordinary copying camera. When I sent them to Boston, I wrote to Mr. Tolles that they were, as compared with the negatives, "enlarged a little in the copying camera." I also informed him that the frustules represented, counted in the microscope, the one 92, the other 93 striæ to the $\frac{1}{1000}$th of an inch. (This letter was sent July 10th, 1871.) Nothing was said then or since of the distance at which the original negatives were taken, and the powers of these original negatives were very different from those of the positive copies.

Now, the argument of the article referred to is based on three suppositions:—

1. That the positives are of the same size as the original negatives. 2. That these were each taken by the objectives named, without eye-piece, at 48 inches distance. 3. That the frustules represented, counted each 95,000 striæ to the inch. Each supposition is untrue. Perhaps the writer of the article may be able to explain why he made these suppositions; I, for one, would be willing to judge his excuses without prejudice.

Whether now his measurements of the positives are correct, is another question, and one which does not affect his argument in the least, since the size of the two bears no uniform relation to the magnifying powers of the two objectives at equal distances. I will only say that, as the positives were copied for me by another person, it is quite possible that even those from the same negative are not of the same size, though I intended they should be.

Mr. Bicknell's measures of the two he examined do not correspond with mine, of my retained copies, but I do not emphasize this point because it does not bear on the questions involved. The positives were sent out to show the relative defining powers of the two objectives, and not to indicate their magnifying powers; everything therefore was made subordinate to the first object, and as I did not measure each copy, I may perhaps have unintentionally fallen into error as to the size of some of these.

This possibility of erroneous figures as to some of the positives, does not apply to my published measurements of the actual magnifying powers of the objective in question; and as both glasses are still in the microscopical collection of the Army Medical Museum, I should be happy to give any *respectable* microscopist an opportunity to compare the two objectives as to magnifying power and other qualities.

III.—*On the History, Histological Structure, and Affinities of Nematophycus Logani*, Carr. (*Prototaxites Logani*, Dawson), *an Alga of Devonian Age*. By Wm. Carruthers, F.R.S., &c.

Plates XXXI. and XXXII.

Sir Wm. Logan, in his 'Report on the Progress of the Canadian Geological Survey for 1844,' published an account of his discovery of fossil plants in beds of Devonian age at Gaspé, in Lower Canada. In 1856 Dr. Dawson described, in the 'Proceedings of the American Association' (p. 174), one of the most interesting of these fossils—a stem which he considered exhibited a very remarkable and previously undescribed coniferous structure. With the view of more thoroughly investigating Logan's discovery he visited Gaspé in 1859, and published in the 'Quarterly Journal of the Geological Society'* a lengthened description of the plants. Among them was the stem already referred to, for which he established the genus *Prototaxites*. The description of the fossil then given is as follows:—"Woody trunks with concentric rings of growth, and medullary rays. Cells of pleurenchyma scarcely in regular series, thick-walled and cylindrical, with a double series of spiral fibres. Disk-structure indistinct in the specimen observed." The name was given to the stem because of the "spirally-marked cells characteristic of the genera *Taxites* and *Spiropitys* of Göppert, but differing from any conifer known to me in the cylindrical form and loose aggregation of the wood cells, as seen in the cross-section." In the lengthened description Dr. Dawson says that in longitudinal section "the wood cells are seen to be much elongated, and to terminate in conical points, and their sides are covered with the remains of a double series of spiral fibres, among which are a few scattered roundish spots, which perhaps indicate a single row of disks." No *pith* is perceptible, and the medullary rays are marked by clear, structureless spaces, the cells having been entirely disorganized; but the

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EXPLANATION OF PLATES XXX. AND XXXI.

Plate XXXI.—Containing transverse sections of *Nematophycus Logani*, Carr. Fig. *a*, showing across the centre of the specimen the tract of smaller cells, which gives the exogenous aspect to the stem; and an open space with a few scattered tubes,—a medullary ray of Dr. Dawson. Fig. *b*, a part of the open space in *a*. The artist has produced the effect shown by using the binocular microscope with a deep objective. Fig. *c*, a part of *a*, showing the spaces between the tubes and the mouths of the smaller tubes.

Plate XXXII.—Containing longitudinal sections of *Nematophycus Logani*, Carr. Fig. *a*, showing the irregular direction of the tubes, and a few tubes in the clear space, or "medullary ray." Fig. *b*, a small fragment, showing a series of sub-circular markings on the inner surface of a cell—the "bordered pores" of Dr. Dawson. Fig. *c*, a part of *a*, showing the large longitudinal tubes, and the multitude of smaller tubes matting them together.

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* Vol. xv., p. 477.

space which represents them in a tangential slice shows that they must have consisted of several rows of cells. The rings of growth are rather more than one-tenth of an inch in thickness, and they indicate, in a specimen 1 foot 5 inches in its greatest diameter, and 11 inches in its least, an age of about one hundred and fifty years.

In a subsequent communication to the Geological Society,* Dr. Dawson describes some fossils collected by a member of the Canadian Survey at Gaspé, and among them appears this same fossil under the name of *Nematoxylon crassum*, gen. et spec. nov., with the following description:—"Fragments of wood with a smooth thin bark, and a tissue wholly composed of elongated cylindrical cells with irregular pores or markings. No pith, medullary rays, or rings of growth." With it are associated, in the same genus, some small stems named *N. tenue*, founded obviously upon fragments of wood, but which have nothing whatever to do with it, as Dr. Dawson's figures clearly show. As Dr. Dawson has never given characters to any of the genera which he has proposed in his numerous papers, it would not be possible to determine what consideration induced him to place two structures so different under the same genus, but in this case he tells us he was influenced by the novel consideration that "they correspond in their negative characters"! Such beautiful simplicity in the characters which unite species into genera ought greatly to simplify our systematic works; indeed, I cannot understand on what principle Dr. Dawson, after establishing *Nematoxylon*, did not include all the fossils he has described in this remarkable genus. It certainly swallowed up *Prototaxites*. It is true Dr. Dawson points out four characters in which *N. crassum* differs from *Prototaxites*. These are:—1st. The cells are one-third greater in diameter,—but in the drawings representing the two fossils, in his recently-published 'Pre-carboniferous Plants,' each being magnified to the same extent, the cells of *N. crassum* are the smaller of the two. This is obvious from an examination of the facsimile of Dr. Dawson's figure of *N. crassum* here given (Fig. 1 *a*), as compared with a small portion of his figure of *Prototaxites*, also given in facsimile (Fig. 1 *b*). 2nd. The cells are destitute of the peculiar markings of *Prototaxites*,—but the cells of *Prototaxites* are also always destitute of the markings which Dr. Dawson describes as "peculiar" to them, as will be apparent from the description which follows. 3rd. There are no medullary rays—neither are there any in *Prototaxites*. And 4th. There are no rings of growth,—and this might have been expected, as the largest specimen was only half an inch thick. There seems to be something more in the character given in the general description,

FIG. 1.

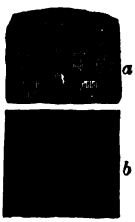


Fig. 1 *a*, copied from fig. 137 *a*, and Fig. 1 *b* from fig. 20 of Dawson's 'Pre-carboniferous Plants.'

* 1863, vol. xix., p. 446.

that the cells are "much thickened by a ligneous deposit;" but any one accustomed to the investigation of fossilized tissues will, on a hasty glance at Dr. Dawson's figures, see that he has mistaken the nearly complete filling up of the cell-cavity with mineral matter for "ligneous deposit." This condition of fossilization is very common; but the close affinity, if not identity, of *N. crassum* with *Prototaxites* is almost admitted in the 'Pre-carboniferous Plants,' where it is said:—"I place these plants here simply because of the resemblance of their tissues to those of *Prototaxites*, with which it is possible that they may have had some connection, being perhaps stems or slender roots of similar species of smaller size" (p. 20).

When Dr. Dawson visited England in 1870 I had an opportunity of examining, in his company, his specimens of *Prototaxites*. I then pointed out to him some peculiarities in its structure inconsistent with his descriptions. He presented some of his very interesting fossils to the British Museum, and among the specimens of *Prototaxites* was one fragment which he specially pointed out as suitable for microscopic preparation and examination. The drawings accompanying the present paper have been accurately made by Mr. Blair from this specimen.

In a paper "On the History and Affinities of the British *Coniferæ*," which I submitted to the Geological Section of the British Association at Liverpool (1870), I had, in dealing with the *Taxineæ*, to consider *Prototaxites*, which Dr. Dawson informed me he had found in England. A careful examination of the prepared specimens had convinced me that Dr. Dawson's plant was no conifer, and reasons which I shall presently state at length satisfied me that it was an Alga. This opinion I expressed as a reason for my eliminating *Prototaxites* from the *Taxineæ*, as is shown in the only abstract of my paper published at the time, from which I make the following extract:—"The *Taxineæ*, containing nearly 100 living species found all over the world, and represented in Britain by the yew, made their appearance in the Carboniferous rocks, as determined by a fruit described by Dr. Hooker, and shown by him to be nearly related to the living *Salisburia*. The supposed Taxineous wood from the North American Devonians, to which Principal Dawson gave the name of *Prototaxites*, was a remarkable Alga of enormous size. Several Taxineous fruits have been found in the Eocene strata at Sheppey."*

The reporter of another scientific journal, impressed apparently with the notion that this was the one point of interest in my communication, gave the following paragraph as his only account of it:—

"*Colossal Fossil Sea-weed.*—From the microscopic examination of the structure of specimens of the fossil trunks described under

* 'Nature,' Oct. 6, 1870, p. 464.

the name of *Prototaxites Logani*, and which Principal Dawson affirmed, in his Bakerian lecture before the Royal Society, to be the oldest known instance of coniferous wood, Mr. Carruthers has discovered that they are really the stems of huge Algæ, belonging to at least more than one genus. They are very gigantic when contrasted with the ordinary Algæ of our existing seas, nevertheless some approach to them in size is made in the huge and tree-like Lessonias which Dr. Hooker found in the Antarctic seas, and which have stems about 20 feet high, and with a diameter so great that they have been collected by mariners in these regions for fuel under the belief that they were drift-wood. They are as thick as a man's thigh."*

This paragraph came under the notice of Dr. Dawson in a reprint in the 'American Naturalist' for May, 1871, and he met it with the following reply:—

"*The earliest known Coniferous Tree.*—Allow me to say, with reference to a notice copied in your May number from the 'Academy,' that the opinion respecting the plant above named, attributed to Mr. Carruthers, is an entire mistake. *Prototaxites Logani* is an exogenous tree, with bark, rings of growth, medullary rays, and well-developed, though peculiar woody tissue; and if Mr. Carruthers has made such a blunder as that attributed to him, this can only be excused by defective observation or imperfect specimens.—J. W. Dawson."†

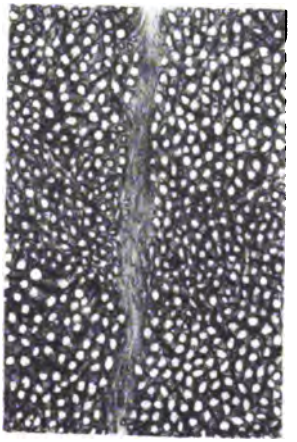
Unwilling to continue a discussion after this fashion, and aware that Dr. Dawson proposed publishing a further account of the Devonian plants of Canada, I addressed to him a letter, containing at length my interpretation of the structure and affinities of this fossil, with the view of preventing the republication of obvious error. I regret that my effort was futile. In 1871 Dr. Dawson published, as a Report to the Director of the Geological Survey of Canada, his work on 'The Fossil Plants of the Devonian and Upper Silurian Formations of Canada.'‡ All the errors of observation which I had pointed out to Dr. Dawson in his own specimens when he was in London, and shown in detail by drawings and descriptions in the letter to which I have referred, are here repeated and expounded, and the same absurd generalizations as to affinities, &c., are indulged in. My communication is made the subject of the following note on page 19, and this is the only use made of it:—"Mr. Carruthers has kindly pointed out to me some structural points in which this remarkable plant resembles Algæ of the family *Codieæ*, the long tubes traversing which he compares with the cells of *Prototaxites*. For the reasons stated in the text, however, I cannot accept this as an indication of true affinity, and must believe the plant to have been a terrestrial tree exogenous in its mode of growth. The high

* 'Academy,' Oct. 1870, p. 16. † 'American Naturalist,' vol. v., p. 245.
‡ Quoted in this paper under the title 'Pre-carboniferous Plants.'

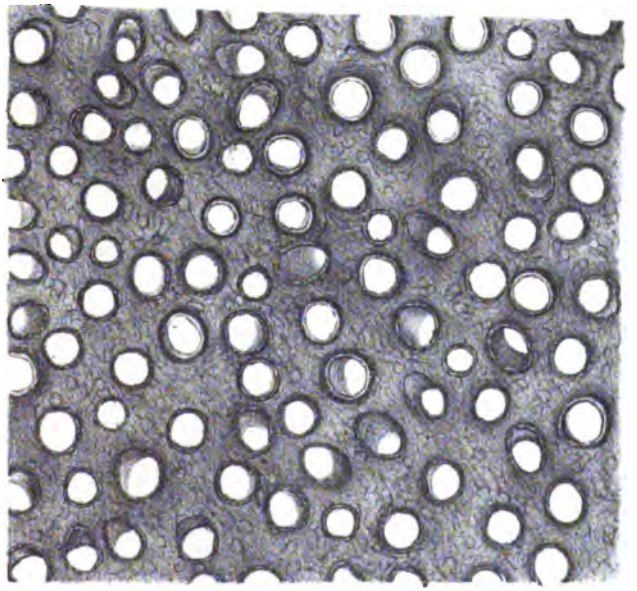
Plate XXXI. Fig. 1.

Plate XXXI. Fig. 2.

b



a



c

D. Blair, ad. nat. lith. W. West & Co. imp.

Nematophycus Logani. Carr. Prototaxites Logani, Dawson.
From the Devonian rocks of North-America.

hat the direction of all of them is not truly at right angles to the ection. Thus, they are separated from each other by spaces often as great as the width of the opening, and sometimes much greater. The merest tyro in histological botany will see at once that we have gone sufficiently far in our investigation of this stem to show us that it cannot belong to a Phænogamous plant. It wants the elements of the definite fibro-vascular bundles of the monocotyledon, and it wants also the elements of the indefinite fibro-vascular bundles of the angiospermous dicotyledon. Nor can it have anything whatever to do with the woody tissue of the gymnosperms, for the wood cells of the different bundles of these plants are universally and necessarily in close approximation, even in "the young succulent twigs of some modern conifers," in which Principal Dawson erroneously says the wood cells are loosely aggregated in relation to each other.*

The space between the large openings is not uniformly structureless, but scattered throughout it can easily be detected openings of smaller size. The walls of these small openings are very delicate, and their interior is filled with mineral substance of the same colour and condition as that which invests them. In this respect they differ from the larger openings, which are filled with a less coloured and more transparent condition of the mineral.

It is important to appreciate fully the mineral condition of this plant. It is entirely converted into amorphous silex, so that not only are the tissues replaced by this substance, but the spaces between the tissues and the cavities inside the tissues are completely filled with it. It is my purpose to deal one day with the different conditions under which fossil plants are preserved. It is too large a subject to treat of by the way. I would only say here that the condition of *Nematophycus* is not uncommon, and that most beautiful specimens of the same condition occur in the silicified plants from Antigua. In the Brownian collection in the British Museum there is a specimen of the terminal bud of a palm, in which all the tissues are replaced with silica; the interior of the cells and vessels are filled with this mineral, and the spaces between the leaves are occupied with the same substance which, from its colour and condition, was evidently supplied at the same time that the organized tissues were being fossilized.

Occasionally spaces more or less free from organized tissues are seen passing through the structure in a somewhat radial direction (Plate XXXI., Fig. *a*). These are the medullary rays of Dr. Dawson. It must, however, be noticed that the direction, though in a general way radial, is not strictly so, but that the spaces have an irregularly tortuous direction. Thus the sides of the spaces are very irregular, coming together here and there, swelling out into irregularly-shaped pouches, and turning off to the right or left. Further,

* 'Quart. Journ. Geol. Soc.,' vol. xv., p. 484; 'Pre-carb. Plants,' p. 18.

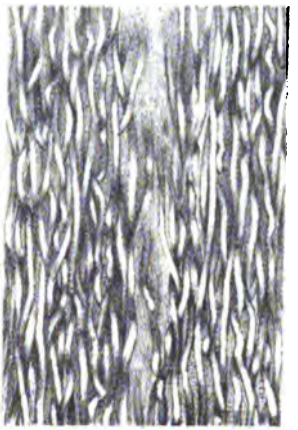
the larger tubes often pass through them from one side to the other (Plate XXXI., Fig. *b*, and Plate XXXII., Fig. *a*); and finally no medullary cellular tissue has ever been detected in them either by Dr. Dawson or myself, although the delicate-walled smaller tubes are well preserved, and may often be seen passing through them.

If we now turn to the longitudinal section, we shall find much additional light thrown on the structure of this fossil. Under a low power (Plate XXXII., Fig. *a*), we see that the large tubes are not parallel, but that while their general direction is the same, they pass in and out in an irregular manner. They are, moreover, continuous tubes without any discoverable termination. This is well shown in the singularly faithful drawings by Mr. Blair. I have most carefully again and again searched for the termination of a tube without success, except in one instance, when examining a slide with my friend Dr. Wittrock, the Algologist, I detected a horizontal septum, with the slightest indentation on the periphery of the wall, showing that it was not accidental to the fossilization, but that it had been present in the original structure. Although I marked the slide I have been unable again to detect the particular tube, else I would have had it figured. Here again I am in direct opposition to the statements and drawings of Dr. Dawson. He says, "The wood cells are seen to be much elongated, and to terminate in conical points."* And his figure† clearly shows this, as well as that (fig. 24) on plate ii. of his 'Pre-carboniferous Plants,' though the text is somewhat modified, but not really corrected. Dr. Dawson has obviously been misled by the way in which one tube is hid by the passage over it of another. I had pointed out this defective observation on his part in my letter, as is evident from the note already quoted. How far this letter led to the modification of the text I cannot tell, but it is remarkable that the text differs from the plates in so important a point.

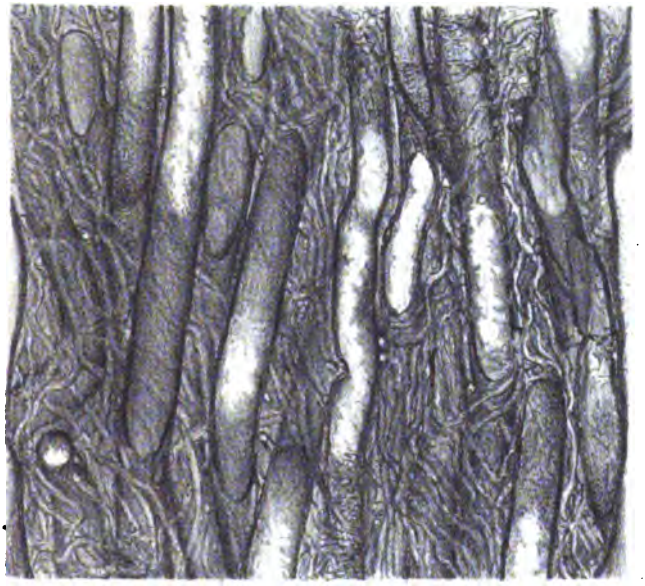
With a higher power than that employed in Fig. *a*, the true nature of the tubes becomes more apparent. The wide spaces which separate them are as obvious here as in the transverse section, and the character and direction of the smaller tubes are clearly exhibited (Plate XXXII., Fig. *c*). If one may speak of one *blunder* as more gross than another in Dr. Dawson's description of this fossil, that designation must be given to the error which he has made in connection with these small tubes. In every description published by him he says, "Cells of pleurenchyma with a double series of spiral fibres." This error in observation (persevered in after I had pointed it out to him in his own specimen) suggested the Taxineous affinity of the fossil, and influenced the drawings of the plant. I have already shown that the large tubes cannot be called pleurenchymatous cells, and now I have to show that the "double series of spiral fibres" are not inside the large tubes, but are independent

* 'Quart. Journ. Geol. Soc.,' vol. xv., p. 485. † *L. c.*, p. 484, figure 5 *h*.

b



a



c

D Blair. ad nat lith. W. West & Cº imp.

Nematophycus Logani, Carr. Prototaxites Logani, Dawson.
From the Devonian rocks of North-America.

uctures external to them! They are indeed the small circular enings seen occupying the spaces between the large tubes in the nsverse section, and which in the longitudinal section we find to slender tubes passing in and out among the large tubes in a ore or less oblique direction. An intelligent or educated observer uld at a glance see the true nature of these smaller structures, ough the great number of them is not at once apparent. This is ring to the delicate nature of their walls, as well as to the fact, nich I have already stated, that the fossilizing mineral which re-aces their tissues is of the same colour and structure as that which ls the spaces in which they occur. Continued observations with od light and a good objective (the drawings were made with a $\frac{1}{8}$th Ross) will exhibit their great number and their irregular direction, well expressed by Mr. Blair in Plate XXXII., Figs. *b* and *c*.

That the strong terms in which I am compelled to speak of rincipal Dawson's descriptions and drawings may be justified by nose who have not access to his memoir, I have copied in fac-mile a drawing from plate ii. of his 'Pre-carboniferous Plants,' in which, as in other drawings, he print is made to tell the erroneous observation vhich Dr. Dawson has given to it. This figure s thus explained:—"*Prototaxites Logani*, three two) wood cells, showing spiral fibres and obscure ores at *a a*, 300 diameters." It is not a restoration. There is a note at the foot of page 18 of Pre-carboniferous Plants' which is in strange contrast to the text. How far it owes its existence to my letter I cannot say, for in it I pressed on Dr. Dawson's notice how completely the existence of these smaller tubes destroyed his whole theory. The note is as follows:—"In some of the more perfect specimens the fibres appear as if connected with each other by fine reticulations, or by the dark bars of the thickened walls passing from one to another. This curious appearance it is difficult to explain. It may either depend on the state of preservation of the specimens, or on some peculiarity of structure at present unknown to me." If Dr. Dawson knew anything whatever about a vegetable cell, and the formation of the spiral fibre in its interior, he would not have written such nonsense as the first sentence in this footnote. A little further reading or investigation would have saved him from the equally fatal blunder of describing coniferous disks or bordered pores on the walls of tubes which are not in contact, but are widely separated from each other. Principal Dawson will find that coniferous disks could not exist in such circumstances. The markings on which the notion is based are not common; but in the slides I have examined I have

FIG. 2.

Copied from Plate II., fig.26, of Dawson's 'Pre-carboniferous Plants.'

seen several, sometimes singly or scattered irregularly on all sides of the walls of the tube, rarely in a continuous series, as in specimen figured at *b* on Plate XXXII. These markings are irregular in outline, and are generally split up into irregular polygons by cracks which pass right through them. They are without doubt placed on the inner surface of the tube. This may be determined by careful focussing, and it is distinctly shown in such a section as that shown on Plate XXXII., Fig. *a*, where a tube is seen opposite the asterisk in longitudinal section, and the markings are on the inner surface of the wall of the tube.

The walls of the large tubes are of considerable thickness. When highly magnified they exhibit a series of minute dots covering their surface. Mr. Archer, in the account of his examination of a section of the fossil, says, "the principal longitudinal tubes appeared on transverse section to have a wall concentrically stratified."* I have most carefully searched for this appearance without being able to detect it. In some cases where the section of the tube is slightly oblique, I have noticed an indication of a concentric arrangement; but this was due, I believe, to the different planes of minute dots coming into view as the object-glass was being moved towards the specimen. I do not, however, believe that so experienced and accurate an observer as Mr. Archer could have been misled in this way: the cautious way in which his opinion is expressed induces me to express the hope that he may again examine the specimen in the light of this criticism.

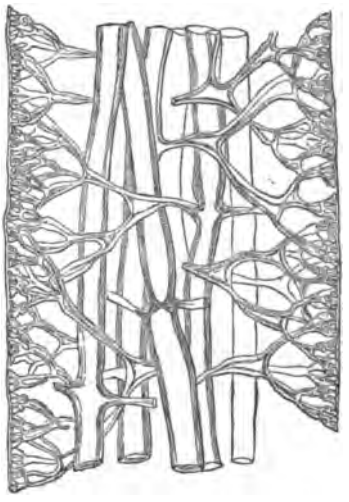
The description of this fossil which I have given, and which has been of necessity so destructive of what has been said before regarding it, establishes that its tissues are of the kind known to botanists as "tela contexta." They consist of elongated cylindrical cells of two sizes, interwoven irregularly into a kind of felted mass. No other tissue of any kind exists in the structure of the fossil. This peculiar tissue is very limited in its occurrence in the vegetable kingdom as it at present exists, being confined to the three lower groups of the cellular cryptogams. It forms the cottony medullary layer in the foliaceous Lichens, the mycelium of the Fungi, and the substance of some Algæ. No one would venture to suggest any relations with the two first Classes, and though *Nematophycus* is an anomalous Alga, it must be placed in this group of plants. We must remember that we are dealing with a portion of the vegetative structure only; the pseudo-exogenous appearance seems to indicate that that portion is an axis.

The sea-weeds in which a structure corresponding to that in *Nematophycus* occurs, are the *Siphoneæ*, *Dasycladeæ*, and *Valoniaceæ*, orders of Chlorosperms, the class which without doubt occupies the lowest place among the Algæ, because of the simple structure of the plants included in it. The three Orders which I

* 'Quart. Journ. Micr. Sc.,' vol. xi., p. 319.

have named have fronds which are composed of one or many filamentous unjointed cells. In *Siphoneæ* the whole plant is composed of one branching cell as in *Bryopsis* and *Caulerpa*, or of many filiform branching cells joined together into a spongy mass, as in *Codium* and *Halimeda*. In the *Dasycladeæ* the simple or branched unjointed stalk gives off whorls of jointed filaments, as in *Acetabularia*. And in *Valoniaceæ* the stalk is also composed of many inarticulate cells united into a firm body by the small horizontal branching tubes, whilst the head is composed of many jointed filaments, as in *Penicillus*. Excluding the jointed filamentous growths of the latter two orders, their structure agrees with that of the *Siphoneæ* in being composed of one or many ramifying simple filamentous cells. And inasmuch as the portions of *Nematophycus* with which we are acquainted are, as I have said, almost certainly portions of a stalk, we can only get a key to its affinities by an investigation of similar parts in existing plants. The articulated frond or stem of *Halimeda* is composed of a series of large tubes (Fig. 3), which give off numerous smaller and repeatedly branching tubes which pass out in a more or less horizontal direction to the exterior of the plant, where they cohere together and form a thin superficial layer of small spherical cells as seen from the outside.

FIG. 3.



Halimeda Opuntia, Lam. From Kutzing's 'Phyc. Gen.,' Pl. 43, fig. II., 1.

The stipes of *Penicillus* often attains a considerable thickness, yet it is wholly composed of a multitude of densely interwoven longitudinal unicellular filaments, which send off laterally slender branches that repeatedly branch and pass out, as in the lobes of *Halimeda*, to form the periphery of the stem.

As far, then, as the presence of large longitudinal unicellular filaments, and of smaller more or less horizontal filaments are concerned, the structure of *Nematophycus* agrees with the stems of these three orders of Chlorosperms. I have not been able to determine the connection of the smaller with the larger tubes in the fossil, as one would expect from the analogy with the recent forms. My friend Mr. James Smith, F.L.S., also made a careful examination of specimens with the view of detecting any relation between the two sets of tubes, but without success. I am further indebted to Mr. Smith for confirming my other observations

on *Nematophycus*. His great skill in microscopic manipulation and observation has been of service to me in this, as in many other investigations. It must be remembered, however, that in this fossil, we are dealing with only a fragment of what must have been a very large organism. Perhaps the size may be thought to militate against the affinity of the fossil with the recent Algæ. As I have before said, some of the specimens included under the names *Prototaxites Logani* are not even congeneric with the one which has been figured, and I believe those which I have excluded are the large stems on which Dr. Dawson's statements of the great size of *Nematophycus* are based. No doubt, however, the fragment exhibiting the structure described, belonged to a plant of considerable size. But mere bulk is of little importance among Algæ. Their increase depends simply on the repetition of similar vegetative parts, which in many recent species goes on to almost any extent.

It may be mentioned that among the living Algæ to which I have been referring, there are some remarkable phenomena in regard to vegetative bulk, if not of the organism as a whole, at any rate of the component parts. Take, for example, the extraordinary size of the single branching cell which forms the frond of the different species of *Bryopsis*, which is often several inches in length; or the saccate cells of *Valonia*, sometimes as large as a walnut, and often as large as a hazel-nut; or still more wonderful, the frond of *Caulerpa*, sometimes 2 feet in length, and yet composed of a single cell! The mere repetition of the vegetative parts, until they attain great dimensions, is not so remarkable as these. However, among existing Algæ are to be found some of the greatest masses of vegetable organisms. In the Northern Pacific a species of the genus *Nereocystis* attains a length of 300 feet, and spreads out on the surface of the water a great mass of green fronds, supported on a large floating bladder 6 or 7 feet long. Yet notwithstanding its enormous size, it appears to be only of annual growth. Much larger than this is the *Macrocystis pyrifera*, which Dr. Hooker, on reasonable grounds, has estimated to attain a length of 700 feet. And, it may be as regards external form, we have in the arborescent Lessonias a nearer approach to these Devonian Algæ. These Algæ attain a height of 25 or 30 feet, and their stems, from their exogenous growth, are sometimes a foot in diameter.

The pseudo-exogenous growth is well known in different Algæ. Reference has already been made to the giant Lessonias of Antarctic seas, but I might adduce the Laminarias of our own shores in which this exogenous growth also occurs. In mentioning these two genera, both belonging to the same group (*Laminariaceæ*) of the Melanosperms, and both characterized by the possession of pseudo-exogenous stems, it is worth adding, by way of caution in generalizing, that the method of growth is different in the two genera. In

Laminaria the exogenous increase is from below upwards, according to the growth of the roots, while in *Lessonia* the growth is from above downwards, in proportion to the increase of the leaves.*

A reference now to Dr. Dawson's specific diagnosis will show that in every particular save one it is erroneous. The fossil does not possess a *woody* trunk, and it is not penetrated by medullary rays. The cells are not pleurenchymatous, and there is no double series of spiral fibres. There are no disks or bordered pores. One character only remains, that the trunks have concentric rings of growth; but this is again and again erroneously employed to establish the exogenous, that is, the phænogamous position of the fossil.

It is difficult to conceive how such a series of errors first took possession of Dr. Dawson's mind, and it is still more difficult to realize how that, after the errors were pointed out, first in his own preparations and then in a private and friendly letter, they were persisted in by a searcher after truth. The characters, which are apparent at the first examination of a prepared section of *Nematophycus*, which are obvious at a glance in Mr. Blair's drawing, and which can be to some extent seen even in Dr. Dawson's faulty drawings, especially in the transverse sections, are sufficient to convince any one who has an intelligent acquaintance with the elements of histological botany. It is certain that from this botanical point of view I have no ground in common with Dr. Dawson. Had it been so, there never would have been occasion for this communication.

I have said that *Nematophycus* is an anomalous Alga, and, indeed, that with the materials known it is not possible to correlate it with certainty with any known group of Algæ. Since I first directed my attention to this remarkable fossil, I have lost no opportunity of submitting the preparations to botanists who were acquainted with histology, and especially those who had devoted their attention to Algæ. It is superfluous to say that I have never met with one who for a moment imagined the fossil could be coniferous. On the contrary, the universal testimony has been that it is an Alga.

Archer, of Dublin, thus describes the fossil in his Reports of the Proceedings of the Dublin Microscopical Club:—"There was no appearance of 'disks'; both longitudinal and vertical sections seemed to indicate that the mass was composed of a number of tubes running in a nearly parallel direction, rarely bifurcating, and seemingly not septate or tapering, and with an intercellular medium, apparently formed of minor tubes. . . . . The appearance of medullary rays was probably produced by accidental cracks or fissures, no structure corresponding to them being shown under the

* See Berkeley's 'Introduction to Cryptogamic Botany,' p. 57.

microscope. The 'lines of growth' would have their parallel in the pseudo-exogenous stems of the existing *Lessonia*. In connection with this Alga, it is a curious coincidence that Dr. Hooker remarks that in the Falkland Isles the ignorant observer at once takes the trunks of *Lessonia* washed up on the shore for pieces of drift-wood." *

Prof. Dickie, of Aberdeen, whose algological labours are well known, says, "The plant must have been an Alga; but unless we know the structure from centre to circumference, its exact place cannot with certainty be determined."

My friend Mr. Lindahl, of Lund, was so good as to submit a slide to Agardh, and he sends me the estimate that Agardh formed of it in the following words:—"He says that it is not like the texture of any known species of Alga, but still it is far less like anything else that he knows, and that he cannot at all believe that it is a conifer."

It is unnecessary for me to add to these testimonies; I am satisfied to leave the decision in the hands of those who know anything of histological botany. The faithful drawings that accompany this paper supply sufficient data for appreciating the structure, and for forming a proximate estimate of the position of the fossil. This is not a matter for the mere geologist.

The following short exhibition of the systematic position of the fossil, with the diagnosis of the genus, and the synonymy, will fittingly close the paper.

ALGÆ.

Class.—CHLOROSPERMEÆ.

Inter *Siphonearum*, *Dasycladearum* et *Valoniacearum* Ordines.

NEMATOPHYCUS. Gen. nov. Stem wholly composed of innumerable longitudinal unjointed cellular filaments woven together by a multitude of small sub-horizontal cellular inarticulate tubes, so as to form in the living plant a compact spongy mass.

Sp.—*N. Logani*, Carr.

Syn.—*Prototaxites Logani*, Dawson, 'Quart. Journ. Geol. Soc.,' vol. xv. (1859), p. 484, Fig. 4. 'Pre-carboniferous Plants' (1871), p. 16, Plate II.

Nematoxylon crassum, Dawson, 'Quart. Journ. Geol. Soc.,' vol. xix., p. 466, Plate XIX., Fig. 24. 'Pre-carboniferous Plants' (1871), p. 20, Plate XI., Fig. 137.

* 'Quart. Journ. Microsc. Science.' New Series. Vol. xi., pp. 319, 320.

IV.—*On the Active Part of the Nerve Fibre, and on the probable Nature of the Nerve Current.* By LIONEL S. BEALE, Fellow of the Royal Society and of the Royal College of Physicians, Physician to King's College Hospital.

MOST observers will agree in the opinion that the particular portion of the nerve fibre concerned in the transmission of nervous impressions is in all probability the axis cylinder. This view will be generally admitted, at least as regards those extended nerve cords, which in man and the higher animals intervene between the central origin and the peripheral ramifications of nerves. This intermediate portion of the nerve cord varies much in length in different cases, and it may be regarded as *non essential*, because in all animals we have parts of the nervous system which are active in which this portion of the nerve does not exist at all; while in some animals, the part of the nerve fibre under consideration is altogether absent, and in none is it to be found at a very early period of life, when nerve action is nevertheless certainly taking place. This intermediate portion of the conducting cord then may vary wonderfully in length, like an electric wire, without any great difference in the action resulting. In the smallest mammal it cannot be more than half an inch in length, while in the whale it extends to many feet. Nevertheless, impressions pass from one end to the other with great rapidity in both instances.

Now the active part of the nerve fibre distributed to the peripheral nerve organ which receives the impressions, exhibits the same general structure and anatomical arrangement in all cases. It is invariably a pale, very transparent, faintly granular, but in the natural state perfectly invisible cord, composed of still finer fibres, or consisting of a soft material which is traversed by currents which take several different lines or paths along it, as may be distinctly demonstrated. This has been regarded as an extension of the axis cylinder, and in an anatomical sense the conclusion is nearly true. The statement is not, however, perfectly accurate, because in many specimens it is evident that the so-called medullary sheath of the nerve and the axis cylinder gradually become altered until the delicate terminal ramifications of the nerve fibres are reached. The fibres of this terminal portion of the nerve network *never resemble in structure the central part of the extended cord already referred to.* Indeed, in the adult mammal these two parts of the nervous system exhibit such remarkable contrast in minute structure, and in chemical composition and physical characters, that it is difficult to believe they are of the same nature. Whatever may be the real nature of the change in the part of the cord midway between centre and periphery, the changes in the more delicate

peripheral ramifications are probably far more subtle, succeed one another with much greater rapidity, and are excited with far greater facility.

In this communication I propose to limit myself principally to the consideration of the structure and action of the intervening part of the nerve cord, which is known as the axis cylinder, and is surrounded by and imbedded in a considerable thickness of the so-called white substance of Schwann, or medullary sheath of the nerve fibre. The latter material, which is ten or more times the diameter of the axis cylinder, consists of a fatty albuminous substance known as myelin or protagon. It forms a tolerably uniform layer round the core, which it probably insulates and protects, as the gutta-percha of the electric cable insulates and protects the copper wire which transmits the electric current, and thus prevents damage and loss of conducting property by the contact of adjacent objects. Not only so, but the constitution of the white substance of Schwann or medullary sheath is such as to interfere with the free passage of fluid. It is not very permeable to aqueous or albuminous solutions, and it is probable that by this investment the access of an undue proportion of fluid is prevented, and a uniform degree of moisture in the axis cylinder is preserved. Upon this thin thread-like impression-transmitting axis cylinder all our interest centres. We desire to ascertain how it acts, and we want to know by what means it conducts or transmits impressions direct from centre to periphery, or in the opposite direction. These questions have been often asked, but never conclusively answered. Indeed, in the present state of our knowledge, a conclusive answer is probably not to be given; but it will not be an unprofitable waste of time to consider to what conclusions upon this question we are led by contemplating the facts revealed by careful microscopical investigation.

There is hardly a tissue in the body concerning the structure and action of which we do not possess more positive information than we have been able to gain as regards the exact structure and mode of action of the axis cylinder. A strange mysteriousness still hangs over the question of structure of the axis cylinder. Even the ultra-physicists and chemists, who assert that *all* the phenomena of living beings are physical and chemical, affirm little of a very positive character upon the mode of action of the axis cylinder. Confident as they are in expressing themselves upon most physiological and many medical questions, there seems to be some doubt—even a little diffidence—just the slightest hesitation—concerning the manner in which this little cord carries to us intelligence from the outside world and brings us into relation with the many objects that surround us. Its arrangement, they admit, looks very like that of an insulated wire; but then it is such a bad conductor of electricity that the

nerve force can hardly be of this nature. If it were electricity, surely the conductor *ought to be* metallic! The nerve force, therefore, they conclude, is not electricity. Having affirmed this, all further doubt and hesitation vanish, and we are confidently assured that nerve force is unquestionably some *other mode of force* not yet discovered, but undoubtedly correlated with heat, light, electricity, motion, and the other modes of energy. This, according to the teaching of the present day, is certain. Nerve force must be a mode of ordinary force, because there is nothing else that it can be. And yet one has a misgiving that the advocates of positive-fact knowledge may be exhibiting some little weakness and inconsistency in trusting to this sort of argument. For upon what science principles do they even dare to talk about *new modes* of force they know nothing about? How can they bring the observation and experiment in which only they believe, to bear upon a new mode of force which they admit exists at present only in their own imaginations? Of course we can imagine many things which we cannot investigate; but imagination is not observation and experiment, and imagination has no place in the positive science of our day. New modes of force which *may* exist are but philosophic phantoms—mere fictions of the imagination and of the inventive faculty. Is it reasonable to teach the existence of undiscovered forces correlated with ordinary force? It has been affirmed to be unreasonable, fanciful, frivolous, absurd, &c., &c., to suggest that of undiscovered forces, some there may be which are distinct altogether from ordinary forces, and not correlated with these.

Some consider that the axis cylinder is a soft pulpy material, the relative position of whose ultimate particles is much altered during its active state. Others look upon it as a peculiarly vital form of tissue, although it behaves towards reagents very much as certain forms of fibrous tissue. It certainly is not allied to any form of bioplasm or living matter. Generally the tissue of the axis cylinder has been regarded as very peculiar, and it has been inferred that its peculiar function must be in some manner determined by its peculiar structure or its peculiar chemical composition. Not that there are any new *facts* in favour of this notion; but it has been laid down most authoritatively that the properties of this particular tissue, as of everything else in nature, are due to the properties of the material of which it is composed, and of course to the properties of the elements entering into its composition.

Now when we come to examine this mysterious force-conveying axis cylinder, what do we find? A filament possessing an exceedingly simple structure, which, at least in many instances, looks very like ordinary fibrous tissue. Indeed, if we were shown only a very small piece of an axis cylinder of a frog's nerve fibre, and some pieces of fibrous tissue of the same shape and size, we should

not be able with certainty to distinguish one from the other. Or the axis cylinder be regarded as anything more than a very elongated band composed of a texture closely allied to white fibrous tissue but formed of perfectly parallel and continuous strata, not disposed as distinct fibres, but nevertheless tearing in the longitudinal direction? In many instances, however, the axis cylinder exhibits a tendency to break transversely, and in several of my specimens I have seen oblique lines, but whether these should be regarded as natural I am not sure. In one preparation which has been kept for some time the nerve (axis cylinder) near an oval nerve cell exhibits a distinctly spiral or corkscrew-like arrangement, as if it had been twisted round and round in the same direction during formation. At the same time I have never been able to demonstrate any remarkable peculiarities of structure in the axis cylinder. Even under the highest powers only a fibrous appearance can be discerned. Anatomical observation almost justifies the idea that if it were possible to replace an axis cylinder by a long filament of ordinary fibrous tissue, we should find that this would conduct the nerve current as effectually as the ordinary axis cylinder itself does. To me it appears very doubtful if the axis cylinder *is* capable of undergoing any remarkable change in ultimate arrangement during nerve action, and I think that whatever change does take place it is of such a nature that it might occur in other forms of tissue. The peculiarity of the nervous system upon which all its characteristic phenomena depend is probably not any remarkable arrangement of its ultimate molecular structure, but simply uninterrupted continuity of conducting tissue. Nor have we reason to think that the bioplasm which forms nerve tissue grows or lives very differently from other forms of bioplasm. It receives *its* nutrient material from the same blood and is derived from the same masses which give origin to other forms. Considering the characters and arrangement of the bioplasm matter and its relation to the formed material in all tissues, it is not unreasonable to conclude that currents, and perhaps of the same nature as those discharged by nerve organs, are set free, but that it is only in the case of nerve that an arrangement exists suitable for insulating the *currents* and for rendering evident, variations in their intensity, rate of transmission, &c.

The general arrangement of the fibres and cells in all central and peripheral nerve organs; the structure and arrangement with respect to one another of all nerve fibres which pass through a considerable distance before they reach their destination; the manner in which nerves act upon contractile tissues, and the circumstance that a current of electricity produces a similar action; and the fact that electricity is set free in special organs which are very *rich in* nerves, but which do not differ in any essential particulars in ulti-

nate arrangement from other nerve organs in which electricity is not set free, render it probable, as it seems to me, that the current transmitted by the axis cylinder is ordinary electricity, and that all the effects produced upon other tissues depend upon the transmission through nerve fibres of currents of electricity varying in intensity.

But what determines the particular course the electrical current shall take,—which fibres shall be traversed by a strong current and which by a weak one,—how the intensity of the current varies at different times, and is momentarily increased or reduced by the influence of the will,—are questions quite apart from the consideration of the nature of the current itself. But if we can determine the probable nature of the nerve current, or even show that there is great probability of it being determined, one very important step towards the solution of much higher physiological problems will be gained.

Of the Nerve Current.

The general opinion, however, of physiologists, as already stated, seems to be that the nerve current is some mode of force, perhaps correlated with heat, electricity, &c., but not exactly identical with any form or mode of energy known or of which we have as yet any experience. The arguments upon which this view is founded appear, however, to me very inconclusive. Much of the evidence we possess unquestionably favours the view that the nerve current *is* electricity, for many, indeed most, of the phenomena familiar to us may be explained upon this view. Lastly, some physiologists have sought to account for the wonderful phenomena of the nervous system by supposing that some force or power of a peculiar and exceptional kind is at work in nerve systems only.

I shall endeavour to show that if electricity could be made to travel in different directions, and the currents combined in various ways and made to traverse series of conducting cords very specially arranged, according to design, the phenomena of nervous action might be accounted for without resorting to the hypothesis of the existence of a peculiar power, or of some new mode or form of force not yet discovered.* It is at least not improbable that the varying effects noticed in connection with the nervous system may be determined by alterations in the intensity of the current, and in the conducting properties of the fibres, instead of being due to the

* Physicists and chemists see no difficulty whatever in assuming the existence of many modes of force of which they can form no conception, and think it very satisfactory to refer phenomena which they cannot understand to some at present undiscovered form or mode of ordinary motion; but if anyone attributes these same phenomena to the influence of some equally undiscovered form of force having no connection whatever with primary energy or motion, he is ridiculed, because, say the physicists and chemists, "there is but one force in kosmos"!

transmission of *different kinds of nerve force.* One would have thought that it would be more in accordance with the doctrines of physical science to endeavour to explain the phenomena by the action of forces we know something about, than to attribute them to the influence of other forms or modes of force which are purely fanciful and fictitious. At any rate it will be time to call in the aid of such airy nothings when all attempts to explain the facts by known forces shall have failed.

But it is interesting to notice how often minds of the most rigidly physical tendencies seize upon purely conjectural hypotheses, and use them as if they were established truths. It has been surmised that nerve action depends upon a chemical change which is supposed to take place in every part of the nerve fibre. Mr. Herbert Spencer settles the question in the most summary way by boldly asserting that the axis cylinder of a nerve consists of some colloid "matter isomerically transformed with ease." He accounts for nerve action by suggesting that the protein substance of the axis cylinder "is habitually changed from one of its isomeric states to another," while he says that the matter of the nerve cell is the seat "of destructive molecular changes and disengagement of motion."

Chemical Theory of the Nerve Current.

A chemical theory was long held concerning the nature of muscular action, but it was at last admitted, as was, indeed, apparent from the very first, that muscles would have to be destroyed and reformed at a far more rapid rate than it was on other grounds reasonable to suppose possible, if the great amount of energy manifested during their action was really due to chemical decomposition of the tissue of the muscle itself. There was, in fact, no evidence whatever, except that which was distilled from the imagination of the chemist, for the conclusion that muscular tissue did undergo rapid disintegration and reconstruction. From my own investigations of muscular fibres in various animals, I felt quite sure at the very time when these chemical doctrines were in high favour, that the conclusions were thoroughly erroneous. From the study of muscular tissue at different periods of development, and the consideration of various circumstances connected with the growth of muscle and its relation to other textures in a variety of animals, particularly in the class of insects, I was convinced that muscle was a *slowly growing* tissue, and that the work it performed was certainly not due to the chemical decomposition of its *material* particles. Nevertheless the fact of the change in the reaction of muscle from alkaline to acid is still urged in favour of the doctrine, and some have affirmed that a similar change occurs in nerve. In

spite of the statements of Liebreich, Heidenhain, and other observers to the direct contrary, the view that nerve energy is stored up in chemical compounds which undergo chemical change during nerve action is still taught. That such an idea should be stated at all betrays ignorance of the character of the axis cylinder of the nerve itself. If we examine the axis cylinder, say, of the sciatic nerve of a frog, what do we find?—a firm, tough, fibrous-like, flattened band, not easily torn, and evidently consisting of a tissue of slow growth; in fact, the very last characters we should expect to find in a tissue prone to rapid chemical change. Neither is a structure surrounded by ten times its thickness of oily matter (myelin) favourably situated for taking up new materials and quickly getting rid of products of decay. One of the least permeable substances in the body is the myelin of the nerve fibre, and yet through this must pass all the materials from the blood to renovate the disintegrated axis cylinder, if nerve action is due to chemical change in the nerve fibre itself.

The Vibratory Theory of Nerve Action.

Some think that nerve action depends upon the molecules of nerve fibres being thrown into vibration, and the axis cylinder of a nerve has been spoken of as if it had been proved to consist of a chain or chains of very minute spherical particles. But the axis cylinder is not composed of matter which would more readily propagate motor impulses than the matter of ordinary fibrous tissue. Moreover, it varies much in character and in thickness in different parts of its course. The impulses supposed would be much modified during their transmission. This theory leaves one of the most important facts connected with nerve fibres unexplained; for upon such a supposition what purpose could be served by the very thick layer of the white substance of Schwann, and in that part of the nerve only which lies between its central and peripheral distribution? Why do we find, moreover, that this investment is invariably so much thicker where a number of nerves run for a long distance parallel to one another than where they cross one another at considerable angles?

Experimental Investigation Inconclusive.

Most of those who have taken up the subject of nerve action from the experimental side, appear to have had a very imperfect acquaintance with the structure of the tissue upon which they were experimenting. The transmission of electric currents through the nerves of a recently-killed animal, is a very rough operation, and indeed very different in many particulars from the transmission of the natural currents, whatever may be their nature, along the axis

cylinder, while it remains in connection with its central cells during the life of the animal.

If we were to take a bundle of several marine cables, and smear the gutta-percha investment carelessly over the cut ends of the copper wire cores, at a distant part of the circuit, and then transmit a most powerful current through the deranged wires, we should not find the needles acting as they did when very delicate currents were made to traverse an individual wire. The differences observed might induce some to conclude that the current by which the instrument was influenced in the normal state, was totally distinct in its nature from that much more powerful current which gave rise to the much greater but irregular and unmeaning disturbances. This reasoning is applicable to the experiments in which strong electric currents have been transmitted along compound damaged nerve trunks.

Fallacy of the Argument based upon the Excitability of Nerve Fibres.

It has been said that the fact that nerve loses its excitability without losing its power of conducting electricity, is a fatal objection to the doctrine that the nerve current transmitted during life is of the nature of electricity. But many things seem to have been entirely overlooked by those who urge this argument with so much confidence. Is it not obvious that soon after death the bioplasm, which is instrumental during its life in maintaining an equable flow of nutrient fluid through the tissue adjacent to it, must cease to effect this important change? Is it not certain that in consequence the axis cylinder of a nerve fibre must be much changed, and especially at the peripheral distribution of the fibres where they are very delicate and ramify naked upon the muscular tissue? No wonder, then, that the muscles fail to respond to the stimulus as before. This fact is attributed to the nerve having lost its "excitability"; but is it not more probable that the true explanation of the fact is, that in consequence of the change in the constitution of the nerve fibre, resulting from the cessation of the currents of fluid through it consequent upon the death of the bioplasm, it fails to conduct the electrical current as it did when in a state of integrity? So far, therefore, from the above fact being an argument against the idea that nerve force is really electricity, it actually affords support to this view.

The power of causing the muscles to contract when the nerve is irritated is lost sooner if the nerve be irritated than if it be at rest. It is increased by heat and decreased by cold. When the nerve is "irritated," the operation is such as would be certain to alter any structure so delicate as the peripheral ramifications of nerves, and

impair or destroy their conducting power. Dr. Rutherford has remarked that the nerves of a weak animal conduct faster than those of a strong one. "The velocity is so great in this case that it may be scarcely measurable," a fact which probably depends upon the circumstance that the conducting tissue is more moist. In weak animals the masses of bioplasm are much more abundant than in strong ones, and the tissues contain a large quantity of fluid. The axis cylinder participates in this change, and in this way the remarkable irritability manifested may be accounted for.

Again, it has been argued that because the irritability of a nerve can be destroyed by the electrical current, nerve cannot be a mere conductor of electricity, and that nerves after death are as good *conductors* of electricity as during life, and that frozen nerves conduct electricity, though they will not transmit nervous energy. These objections are considered by many to be fatal to the idea that nerve force is after all only electricity. But a little consideration on the part of those who argue thus, would have convinced them that the facts above referred to are as easily explained upon the electrical as upon any other hypothesis of nerve action.

Little can be gained from the argument that a bit of nerve that has long been dead conducts electricity as well as a nerve just removed from a living animal; for neither are much better or worse conductors than some other tissues of the body. We must remember that in nature the thing that actually transmits the nerve current is the axis cylinder alone, but that in our experiments we send comparatively strong currents through the white substance of Schwann and axis cylinders indiscriminately. The current deranges the axis cylinders, and of course seriously damages the delicate distal ramifications of the nerve fibres.

No one who has seen and studied the ultimate ramifications of nerve fibres in tissues, will suppose for a moment that anything conclusive will be learnt concerning the action of nerves by sending powerful electrical currents through damaged nerve trunks.

Fallacy of the Argument deduced from the Rate at which the Nerve Current travels.

It has been said that the difference in the rate at which nerve energy and electricity travel, is enough to convince us that these two currents are not of the same nature. But the comparison as it has been instituted is not fair. Electricity as it travels along a *copper wire* has been contrasted with nerve energy (electricity) as it travels along a *moist fibrous cord*. No wonder that the rate at which the nerve current travels along the nerves should be very slow as contrasted with the rate at which electricity traverses a copper wire, but such a fact by no means proves that nerve energy

and electricity are very different. The nerve current, it has been proved, traverses not more than from 100 to 300 feet in a second of time, while electricity, it is known, travels at the rate of many thousands of miles in a second. The deduction is, however, defective, and in many ways. I would remark that—

1. A comparison is made between the passage of the nerve current along a moist tissue and the passage of electricity along a copper wire.

2. It must be borne in mind that it is certain that if the nerve current were electricity, it would travel very slowly along such a structure as the axis cylinder.

3. The rate at which a single axis cylinder transmits a current of electricity has not yet been ascertained, but this is exactly the information we need before we can arrive at a correct conclusion on the subject.

4. The rate at which electricity travels through such a moist tissue as the axis cylinder, has to be ascertained and compared with the rate at which nerve energy is known to travel.

I will only remark, in conclusion, that no one has yet succeeded in showing that the nerve current is *not* electricity, while a great number of well-ascertained facts, especially those known in connection with the structure and action of the electrical organs of certain of the lower animals, strongly support this inference.

V.—*On the Regeneration Hypothesis.*

By Dr. Louis Elsberg, of New York.

The more the knowledge of the facts spreads on which Lamarck's Development theory and Darwinism rest, the more the need of a logical conception as to the propagation of organisms becomes felt. Without detailing other attempts in this regard I desire to present a brief account of my hypothesis of regeneration. Its fundamental proposition is the following:—*The germ of every derivative living being contains (resp. consists of) plastidules of its whole ancestry.*

As is well known, Prof. Haeckel has given the name of Plastids* to the four elementary forms of living matter, *viz.* cells with and without investing membrane, and cytodes with and without investing membrane. Adopting the name plastid, and without entering upon the vexed questions connected with the atomic hypothesis, I use the term "plastidule" to designate the smallest conceivable actually existing—the almost infinitely subdivided—particle of the matter composing a plastid.

* As more conformable with the genius of the English language, I have omitted the final "e" of the German word *Plastide*, and of course in pronunciation place the accent on the first syllable instead of on the second.

It will readily be admitted by everybody that the impregnated ovum from which a child proceeds, consists of matter (of plastidules) derived from the bodies of its parents; in the same manner the germs from which each of these parents was developed existed in the bodies of their parents; and it requires but little reflection on the incontestably very great divisibility of matter to understand that actual material particles of the grandparents may be contained in the germs of the grandchild. If we add to our argument proofs and illustrations of the dependence of function upon matter, and point to the various laws of heredity, especially to the transmission to grandchildren of resemblances and peculiarities of grandparents, the way is clear to the exposition that the germ of a child contains actual particles of matter, *i. e.* plastidules, of all its ancestors. By thus proceeding backward in tracing the genealogy of man, I have more than once succeeded in obtaining the assent of very strong opponents of the theory of evolution to my views of the development of man down to his descent from "Adam and Eve." One advantage of this hypothesis is that its adoption does away with the relevancy of the most usual arguments against Darwinism,* and greatly facilitates the acceptance of the evolution theory by both the lay and scientific public.

I call it the hypothesis of regeneration because according to it the ancestors are to a certain extent bodily, and therefore also in every other respect, born again in their progeny.

Commencing with a parental pair, Adam and Eve, we may assume that their children came from germs which were wholly derived from their bodies; that the germs of the children of these children contained, mixed with the possibly modified plastidules of their immediate progenitors, some of the original plastidules of the first parental pair; and so on in each succeeding generation. To express this hypothesis arithmetically, I might say that in each succeeding generation the numerator remaining the same, the denominator of the fraction of each set of ancestral plastidules increases.

But we do not rest with the assumption of Adam and Eve. Let us suppose that under appropriate circumstances the combination of the requisite elements has given rise to the simplest protists, moneres, "protoplasm." (And in this connection I would in parenthesis say what everybody must admit, *viz.* that *if* all the requisite conditions are present, the conversion of inorganic elements into living matter must necessarily take place. No one who considers the subject seriously doubts that at some past time in the history of our globe such conversion has taken place, the difference among men being that some ascribe it to the exercise of direct creative agency in each individual case, while others ascribe it in the first instance to the agency of properties inherent in

* See *Thesis* 4, at end of article.

matter, and to "evolution into higher forms" subsequently; but, I repeat, the fact of such conversion must be admitted by everyone, whether he be the most ultra freethinker; whether he be the most liberal interpreter of the Bible, which says of man, "And the Lord God formed man of the dust of the ground; and breathed into his nostrils the breath of life, and man became a living soul;" or whether he be anything between these two extremes. It is not necessary for the exposition of my hypothesis to inquire what the requisite conditions of the change of not-living into living matter may be, but as to their presence I have emphasized the "*if*" to avoid the possibility of controversy even with those who believe with Prof. Huxley that they are of such physical and chemical a nature as must have existed at some remote period of the earth's history, but which can no more occur again than that an adult man can pass through all the conditions of his infancy again. For my own part, however, I confess that while I do not know exactly what these conditions are, and admit that human agency has never succeeded in bringing them demonstrably about, I can see no difficulty in believing that they may occur in the laboratory of nature at any time—at the present day as well as in the *long* past age—and that whenever they occur—at the present day as well as in the remotest age—the production of living protoplasm from inorganic elements takes place.)

A lump of protoplasm subjected to inspection, under appropriate circumstances, can be observed to become larger by taking up, *i. e.* by combination or mixture with similar plasmic matter, and finally to divide. This so-called "spontaneous division" my hypothesis regards as the consequence of the mixture of two little lumps, similar, but not entirely alike. Two specks of protoplasm, no matter how perfectly alike they may be supposed to be at their production, cannot remain identically similar, because their existence and surroundings in space, and therefore the modifying influences to which they are exposed, cannot be identically the same. If we assume that two such specks combine and are thoroughly mixed with each other, and then divide, we may draw the following two inferences:—1, the resulting specks differ from each of the original specks, inasmuch as they contain plastidules of both; and 2, the resulting specks represent accurately the constitution of the two original specks, but not as these were at the time of their production, but as they are at the time of mixture. We thus arrive at the most elementary conception of the two fundamental laws of the phenomena of all living beings, *viz.* adaptation and heredity. I use the general expression "adaptation" like Haeckel, to denote the reaction of all influences which affect the being, *i. e.* all modifications of its constitution which occur from the moment of its production to that of its death. While

adaptation involves change, heredity preserves in the progeny the constitution of the parental organisms as it is at the time of mixture. The degree or amount of actual change which has taken place in an organism after a long series of generations is a very complicated resultant, the individual factors of which, science is at present only extremely seldom able to show.

Without prolonging this article by the attempt to follow in detail any of the innumerable generations through which the protoplasm had to pass to be developed into a higher organism, I will simply state that the hypothesis of regeneration seems to me to be in accord with all the hitherto ascertained facts of development, to explain in an unconstrained and natural manner many of them, and to run counter to none.

1. The most highly developed organisms are the oldest existing, those in their constitution nearest to protoplasm the youngest; 2, the furrowing or division of a germ into innumerable parts is caused by mixture of different plastidules having occurred innumerable times; 3, the plastidules to be mixed though differing, must not differ more than to a certain, relatively very small, degree; greater dissimilarity prevents the mixture; 4, the germs of various organisms existing at the present day, though they may appear ever so closely related or alike, are, if they have passed through different stages of development, so completely dissimilar in their actual constitution or mixture of plastidules, that the one organism can never be developed from the germ of the other; 5, the derivation from lower organisms is the cause of the various stages of development through which the germ of higher organisms passes, and on this depends the fact so prominently stated and illustrated by Haeckel that "the ontogeny of every organism repeats in brief time and in great general outlines its phylogeny," *i. e.* the individual development of every organism, or the series of forms through which it passes from germ to completed form, repeats approximately the development of its race, or the series of forms through which its ancestors have passed; 6, that the various organisms now existing form a series, or so to speak a chain of gradations, *more or less* resembling the series of forms through which higher organisms have passed phylogenetically and do pass ontogenetically (embryonically), is due to the greater or less resemblance of the influences to which organisms originally derived from more or less similar protoplasmic matter have been and are subjected.

Finally, the hypothesis of regeneration does not necessarily oppose the hypothesis which Darwin names pangenesis, and advocates so learnedly, in the essential features of which Darwin has been anticipated already by (if my memory serves me correctly) Dr. Benedict Hösch in the beginning of this century.

VI.—*On the Use of Monochromatic Sunlight, as an Aid to High-power Definition.*

By Dr. J. J. Woodward, U. S. Army.

A few years ago I published some brief remarks "On Monochromatic Illumination."* These remarks were suggested by the perusal of a letter from Count Francisco Castracane published in the same Journal some time before.†

Count Castracane's method consisted essentially in the use of a prism by which the sunlight was decomposed, and any selected colour could be employed, blue or green seeming to him most advantageous. Mine consisted in passing the sunlight through a cell containing a saturated solution of the sulphate of copper in ammonia, which transmits a bluish violet light, admirably suited to high-power definition, and less fatiguing to the eye than any other colour.

At the time I supposed Count Castracane's method to be new; the one I employed I ascribed to Von Baer.‡ I have since learned that I was in error in both particulars. The proposition to escape chromatic aberration by employing monochromatic illumination goes back in fact to a very remote period in the history of achromatic microscopes; and monochromatic lamps, as well as the use of the prism and of glasses and coloured fluids as absorptive media, were early suggested. It would carry me away from my present purpose to go into a detailed history of the various attempts made from time to time in these directions. As the construction of achromatic objectives continued to improve, these devices fell into obscurity, and it is only of late that attention has been directed to them anew. As for Count Castracane's method, without going farther back, a full account of all the principles involved in the use of the prism for attaining monochromatic light to illuminate the microscope will be found in chapter vii. of the article on the microscope in the eighth edition of the 'Encyclopædia Britannica.'§

The use of the solution of the ammonio-sulphate of copper to exclude certain portions of the solar rays, especially for photographic purposes, would appear to have been first suggested by one of our own countrymen more than thirty years ago.

Professor W. J. Draper published in the 'Journal of the Franklin Institute' of Philadelphia, during the year 1837, a series of "Experiments on Solar Light," in the course of which several observations on the properties of the ammonio-sulphate of

* The then Journal of the Royal Microscopical Society, vol. vii., 1867, p. 253.
† Ibid., vol. v., 1865, p. 249. ‡ 'Einleitung in die Höhere Optik,' p. 48.
§ American edition, 1857, Boston, vol. xiv., p. 798.

copper are recorded. In one of these papers* he states that the ammonio-sulphate solution absorbs the red and yellow rays of the spectrum, and with them so much of the heat, that but "twenty rays for every hundred that fell upon it" were transmitted.

In the 'London, Edinburgh, and Dublin Philosophical Magazine' for September, 1840,† the same gentleman published a paper "On the Process of Daguerreotype and its Application to taking Portraits from the Life," in which he describes his attempts to reconcile the chemical and visual foci of portrait objectives, to escape "the effulgence" of the solar rays thrown directly on the sitter, as practised at that time, "abstract from them their heat and take away from them their offensive brilliancy." These are almost the very objects for which microscopists to-day resort to the copper solution. Professor Draper employed in his experiments "a large trough of plate glass, the interstice being an inch thick," filled with a dilute solution of the ammonio-sulphate. Its size was about three feet square. This was so fixed in the course of the sun's rays, reflected from a mirror upon the sitter, that his head and the adjacent parts were illuminated only by the light which had passed through the copper solution. By this device he reports he obtained excellent results.

In the spring of 1869 I received a letter from one of the sons of Professor Draper (dated April 19th), calling my attention to the above facts, and transmitting several daguerreotypes of microscopic objects, all bearing the marks of considerable age. These the writer (Prof. Henry Draper) states were made at various dates from 1851 to 1856. A Nachet microscope was used, and in every case the ammonio-sulphate of copper is said to have been employed.

The results are not particularly good as compared with modern photo-micrographs, but appear to me not much inferior to the best that could have been done by the daguerreotype method with the microscope used. The time was not yet ripe, and both microscopic objectives and photographic methods have greatly improved since those days.

My present purpose does not permit me to give greater space to these reminiscences, the real object of this paper being to indicate the best practical method to be pursued in obtaining economically the advantages of monochromatic sunlight for high-power definition.

This object excludes a further consideration of the use of the prism. It does its work admirably, as I know by repeated trial, but the results are practically no better, even for photography, than those obtained by the use of the ammonio-sulphate cell; it requires greater skill to use, and the necessary apparatus is more expensive. For the same reason I shall say nothing in this article

* *Loc. cit.*, vol. xix., 1837, p. 473. † Vol. xvii., p. 217.

on the use of artificial lights, further than that both the prism and the ammonio-sulphate cell may be satisfactorily used with either the calcium, the magnesium, or the electric lights by those who are unable conveniently to secure the advantages of sunlight. The light of ordinary coal oil or gas lamps, however, is not suitable for the purpose.

Two very simple methods of securing the advantages of the ammonio-sulphate solution will now be briefly described.

(*a.*) I suppose the observer to be possessed of a good microscope stand, with achromatic condenser and suitable objectives. Then it is only necessary to prepare a proper ammonio-sulphate cell and fix it between the plane mirror of the instrument and the achromatic condenser. The microscope should be set near a window so that the direct rays of the sun fall on the plane mirror, while the head of the observer is protected by a convenient screen, and all becomes easy.

(*b.*) A still better method for the resolution of lined test-objects with the highest powers, and one which is almost as simple as the foregoing, is that described in my paper "On the Use of *Amphipleura pellucida* as a Test-object for High Powers."*

"Erect a perpendicular wooden screen about two feet square on one edge of a small table. Cut in this a circular hole an inch and a half in diameter at about the height of the under surface of the stage of the microscope. On the outside of this hole mount a small plane mirror which can be adjusted by passing the hand to the outside of the screen. On the inside cover the hole with the ammonio-sulphate cell." Now move the table to a window through which the direct rays of the sun can fall upon the mirror, and adjust this so as to throw the solar pencil nearly horizontally through the ammonio-sulphate cell. The mirror and achromatic condenser, if the microscope has one, are removed and the microscope turned so that the solar pencil shall fall with the desired degree of obliquity on the under surface of the object. It will generally be best to condense the light upon the object by a small ordinary lens, or still better by a low-power objective mounted like a bull's-eye lens on a separate stand.

For prolonged observation, however, the motion of the sun will render it necessary to readjust the mirror from time to time, and the use of a heliostat becomes desirable. This gives the most satisfactory results no doubt, but the cost of the heliostat will, of course, prevent it from coming into general use, except among those who desire to photograph what they observe, and the simpler methods above detailed will answer very satisfactorily for every other purpose.

The ammonio-sulphate cell used in either method should be

* 'American Naturalist,' April, 1872, p. 193.

made of two pieces of thin plate glass about 2½ inches square, held apart by thin strips of plate glass, or by a square of plate glass suitably drilled. The point is to obtain a layer of the blue solution about ⅛ of an inch thick between two parallel planes of plate glass. The best cement for the purpose according to my experience is old Canada balsam applied hot; but many other devices may be employed. The solution is made by saturating strong *aqua ammonia* with sulphate of copper, and should be strained or filtered so as to be free from all solid particles. A sheet of fine blue glass may be substituted for the ammonio-sulphate cell, but only with tolerable results; at least, I have never had a sample of blue glass which was of just the right colour.

The selection of the best condenser for high-power definition is a matter which has recently elicited much discussion. In a general way I may say that any condenser will do its best work under the conditions above indicated if skilfully used. For the benefit of those who possess first-class stands but have never purchased an achromatic condenser, it may be stated that almost any objective suitably mounted on the secondary stage can be made to answer instead, the best results being attained when the angle of aperture of the objective thus used is rather less than that of the one employed to magnify the preparation, and when the secondary stage is capable of being centred or decentred at pleasure by screws working at right angles to each other. An ordinary low-power objective (of 1 to 3 inches focal length), mounted on a separate stand and used to throw the light obliquely as already described, is, however, perhaps the most convenient and efficient mode of illuminating line test-objects with high powers.

As to the objectives suitable for monochromatic illumination, the best compound objectives of some first-class maker should be selected. It is a mistake to suppose, as some have done, that a single lens can be substituted for the modern carefully-corrected compound objective, even if the pure monochromatic light of a narrow portion of the solar spectrum as obtained by a prism were employed. For the objective always requires to be corrected for spherical aberration, and in the case of high powers must be provided with a screw-collar to modify the distance between the posterior combination and the front one in accordance with the different thickness of the covering glass of the preparation. Now practically the spherical aberration is best corrected by the just combination of crown and flint glass, and combinations very nearly the same as those employed for white light would still be necessary if the objective were made for exclusive use with monochromatic illumination.

Under these circumstances I do not recommend the use of monochromatic illumination for low or medium powers except when photographs are to be made. It is only as an aid to high-

power definition that I here commend it. With its aid, objectives incapable of resolving certain difficult tests (such as *Amphipleura pellucida*, *Grammatophora subtilissima*, &c.) with white light, show them in a satisfactory manner, and those which even with white light are capable of displaying the most difficult tests, exhibit them with greater clearness and distinctness. I attribute this result chiefly to the well-known fact that the chromatic correction of our very best modern objectives is far from perfect, more or less of a secondary spectrum being always visible, and interfering with distinct vision. Moreover, many of the objects we desire to examine are themselves capable of producing enough chromatic dispersion to interfere with our perception of their true form. Both these evils are escaped by the method here described. I do not advise it as a substitute for other modes of using the microscope, but as a special means of research to be reserved for occasional use in connection with the higher powers of the instrument.

I have frequently been asked to express an opinion as to whether the use of monochromatic sunlight is likely to prove injurious to the eye of the observer. On this subject I can speak from an extensive personal experience in connection with photo-micrography. The only injury to my own eyesight of which I have ever been conscious was produced by an injudicious exposure to the electric lamp. If the microscopist so manages his illuminating apparatus that the field of the microscope resembles in colour and intensity the azure blue of the sky on a clear day (and this is the condition which should always be aimed at), I do not believe the use of the method for any reasonable time will be found injurious. I have recently found, when a sheet of plate glass backed with black velvet is substituted for the ordinary plane mirror in any of the above arrangements, that while the brilliancy of the light is much moderated, its desirable qualities are unchanged, and it is still intense enough for the adequate illuminating of the highest powers. Those who find the light obtained from the ordinary mirror too brilliant may resort to this contrivance with advantage.—*Paper read before the Philosophical Society of Washington, U.S.A.*

VII.—*One of our Common Monads.*

By Professor Albert H. Tuttle.

Since the investigations of Clark, Carter, and others upon the sponges and their allies, anything which adds to our knowledge of the Flagellate Infusoria is of especial interest. I have fortunately had an opportunity of making a careful study of a genus *Urella* of

Ehrenberg, about which thus far but little that is satisfactory has been known.

Until recently but little has been known of the real nature of the Monadina. As a natural consequence, organisms have been referred to the different genera of this family upon very slight study and superficial resemblances. Urella, which Ehrenberg describes thus, "aggregate monads, free-swimming, tailless, devoid of an eyespeck, and having one or (doubtfully) two flagelliform filaments," has of course received its share of attention, until one might think from the "species" and the figures given that the diagnosis of the genus had been "anything very minute, aggregated, free-swimming,"—whether Infusorian or Alga making no difference.

When, therefore, I found in a collection made at Spy Pond, near Cambridge (U.S.A.), on the 25th of November last, a large number of individuals of this genus (probably the species *glauconia* Ehr.), I made use of the opportunity for a careful study of it, devoting my spare time to it daily as long as I continued to find it in the water; what follows is therefore the result of a number of observations, at which each point has been examined and verified.

Urella probably finds its nearest ally in Anthophysa, differing from that genus principally in being free-swimming instead of fixed upon a stalk. The number of monads in a colony is quite variable, almost every number having been seen, up to forty or fifty; in this, however, as in many other respects, the constant activity of the colonies renders it impossible to speak with absolute certainty; even when cornered, so that they could no longer progress in the direction in which they had been moving, they continued to revolve upon their axes with considerable rapidity, making it impossible to count them with accuracy. Occasionally a group of five or six, or even two or three, and not unfrequently a single monad, would be seen, and these were more available for purposes of study, though the larger groups were more frequent.

From such measurements as I was able to make while they were in motion, I should say that the average length of each monad was about $\frac{1}{1000}$th and the breadth $\frac{1}{2000}$th of an inch; but these dimensions varied a good deal with the size of the colonies, the individuals in the larger groups being more elongated and narrower than those in the smaller ones. The form may be described as conical, with a rounded base not at right angles with the axis of the cone, the part at the greatest distance from the apex being the one nearest the apex of the colony; in colonies of over ten or twelve the axis of the cone being also bent towards the apex of the group, especially in those Monads near the base. The form of the larger colonies varied from hemispherical to raspberry or even mulberry shape.

I was unable to find any trace of a common investing membrane

in either the larger or the smaller groups, nor do I believe that any such membrane exists in any true species of Urella.

I was able to distinguish clearly *two* flagella, both arising from a point near the most elevated side of the base of the cone. The larger one was stiff, arcuate with the concave side toward the base before mentioned, somewhat longer than the body of the Monad, and very plainly seen with any objective above a $\frac{1}{4}$-inch; the smaller is very delicate, short, and incessantly active. I first saw it with a Wales' $\frac{1}{15}$th; but after becoming familiar with its appearance, I was always able to detect it with a Tolles' $\frac{1}{8}$th or a Hartnack No. 9.

I am not certain as to the number of contractile vesicles, but it is my opinion that there is but one: sometimes a larger number of clear spaces would be seen in the body, but they were not observed to contract. I do not wish, however, to speak with any degree of positiveness upon this point, as I was not able to keep a single Monad in view long enough to satisfy myself, on account of the constant revolution of the colonies. As regards the ingestion of food, I have seen something, though not as much as I could wish, as this is still a disputed question. I fed them with indigo, which they ate readily, and I frequently watched its ingestion. On account of their incessant motion, I was not able to satisfy myself of the existence of a definite mouth, but I did not see a single instance of the indigo being received at any point except very near the common base of the flagellum, and in every instance observed the act of ingestion was preceded by a quick bending of the larger flagellum, by which the particle of indigo was thrown against the surface of the body in a manner similar to that described by Professor Clark in his observations upon Monas. So far as I was able to follow the process, whenever a colony reduced its rate of motion sufficiently to permit of careful observation upon this point, it was so much like the process described in Monas as to leave little doubt in my mind that it was substantially the same, although, as I have said, I did not *see* a distinct mouth.

It appears probable, now that the Monadina are better understood, that we shall soon be able to recognize in them a well-defined family of the Flagellate Infusoria, although, doubtless, many forms that have been assigned to that group are vegetable in their nature. These will be gradually removed, and those forms which are unquestionably animal will be distinguished. Among these it seems to me the genus Urella, as described by Ehrenberg, will undoubtedly take its place.—*A paper communicated to the Section Microscopy of the Boston Society of Natural History.*

NEW BOOKS, WITH SHORT NOTICES.

The Micrographic Dictionary: a Guide to the Examination and Investigation of the Structure and Nature of Microscopic Objects. Third edition. By J. W. Griffiths, M.D., assisted by the Rev. M. J. Berkeley, M.A., F.L.S., and Rupert Jones, F.G.S. London: Van Voorst. 1872. Parts III., IV., V., VI., VII., and VIII.—This excellent work continues, though slowly, to make its appearance, and we must say that though in some few instances the text is not sufficiently altered or added to, in most cases the additions made in point of reference sufficiently counterbalance this otherwise defect. This is shown more particularly under the head of Angular Aperture, in which we find an omission of most of the facts relating to the aperture highest and lowest of immersion glasses. This, however, will be forgiven when the reader remembers that for a great many microscopists it is still an undecided point, and also from the fact that all the references to the several papers which have appeared in this journal are to be found given. Notwithstanding this want of reference to immersion lenses, the article on Angular Aperture is fully given and the explanations are very plainly stated. The classification of the animal kingdom, however, requires revision; the term Radiata should not have been admitted in so late a publication, and the Cœlenterata should not have been given such an inferior position; Annulosa too should have taken the position of Articulata, and the Bibliography might have been more fully given. The section of Aphides seems to have been carefully done, and so also are Arachnida, Arteries, Ascomycetes, Batrachospermum; Blood corpuscles, as to their measurement and constitution; Bone, capitally illustrated; Bursaria; Cambium; Cell, very good, if not best in the volume, but that reference to Huxley's able paper in the 'Med. Chir. Rev.' is omitted; Chalk, excellent; Characeæ, Chlorophyll, Ciliæ, Closterium, Coccoliths, Coal, and lastly Confervoideæ, which completes the eighth number, but is not yet finished. These articles are all admirable, and where information is not distinctly given, the references to it are abundantly supplied, so that the reader may inform himself to the utmost on each subject which he takes up. Of course, there are deficiencies in some of the papers, but then we cannot consider these till the volume is completed, as there are many occasions of giving information which might or might not be placed under a single heading. All we can say is that the work so far meets our desires, and that we doubt not, it will be even better in the future numbers.

PROGRESS OF MICROSCOPICAL SCIENCE.

The Mode of Existence of the Dendroid Graptolites.—In a paper on the Graptolites of the Arenig Rocks of St. David's, read before the Geological Section of the British Association at Brighton, Mr. John Hopkinson mentioned the occurrence of a new species of *Callograptus*, with an entire "hydrocaulus," or main stem, and a "hydrorhiza"—the organ of attachment. This appears as a series of interlacing fibres at the proximal end of the main stem, which suddenly widens out and becomes more loose or fibrous in structure as it merges into the adhesive fibres. The genus *Callograptus* is thus proved to have been capable of attachment to foreign bodies, and it may therefore be inferred that its near allies, *Ptilograptus*, *Dendrograptus*, and *Dictyonema*, were also provided with hydrorhizæ. *Ptilograptus* and *Dendrograptus* being thus, in general form, in mode of growth, and most probably also in structure, essentially similar to our recent Hydroida; *Callograptus* and *Dictyonema* differing only in their branches, being more or less regularly connected together by transverse processes. For this species the name *C. radiculatus* is proposed, to distinguish it as the first in which these radicular fibres have been seen.

The Nature of Diatom Structure.—Some researches on this point have been lately carried on by Professor Weiss, of Lemberg, who regards the silicious envelope as capable of polarizing light, and as consisting of a cellulose coat more or less infiltrated with silex. He does not consider the individuals one-celled, but finds the valves composed of cells from ·008 to ·00025 mm. in diameter. These cells are furnished at their centres with papillæ, which appear as striæ under low powers, and as moniliform markings under high powers. The large cavity between the frustules is regarded as equivalent to the embryo-sac of higher plants, and the formation of new individuals has been observed within it. An alternation of generation is indicated by the observations made.

The True Markings of the Test Podura Scale.—An interesting paper on this subject has appeared in the July number of the American 'Lens.' From the latter journal we take the following account, which, though somewhat long, is incapable of being abstracted. In the first place, says the author, Dr. Arnold, of New York, I will briefly state my views, as expressed in June, 1871. There are, I believe, some species of Podura that are scaleless, and are clothed with hairs—compound hairs; that is, the hairs are covered by projections, usually in considerable number at *one* end, where they form a brush-like arrangement. In fact, all the species of Podura that I have met with possess analogous hairs to a greater or less degree. It is, then, my purpose to try and establish some points of similarity in structure between the hairs and scales themselves. If we agree that these compound hairs are covered by epithelial scales, and are hairs of other

animals, why should not the "*scales*" also be covered with like structures? As it is my belief at the present time that in the Test Podura scale, as well as that of the Degeeria, the structure of hair and scale is alike, I will give the points which have caused this opinion. There is no doubt that the epithelial scales on the hairs of *L. curvicollis* and *Degeeria domestica* bear a great similarity to the markings on the scales themselves. They are of the same shape, and I have in several instances made measurements which showed the coincidence in size between the "spines" on the hairs and those of the scales. It is comparatively easy to obtain isolated spines from the hairs, and these, when viewed by central illumination, present, as I have stated, an appearance identical with the so-called spines of the scales. If unilateral illumination be employed, a beading is also seen on the spines separated from the hairs, although I have never been able to produce the "varicosities" as well as when they were attached to and lying upon the hair itself. Having satisfied myself as to this fact, the next thing to do was to separate the spines from the scales. I accidentally came into possession of a slide of *Degeeria domestica* scales, mounted by J. Beck, of London, I believe. Here, on this slide, I perceived to my great satisfaction a fractured scale, which showed the spines projecting beyond the broken edge, some of them bent and distorted, and what were evidently places from which others were torn. At a short distance from the broken scale I beheld *isolated spines* just like those on the scale, the measurements of which corresponded to the size of those attached to the scale. These separate spines I have repeatedly seen as rows of beads when unilateral illumination was resorted to, and the projecting spines from the broken margin of the scale exhibited the beading under similar conditions. Dr. J. J. Woodward, of the Army Medical Museum, Washington, on receiving from me some photo-micrographs that I made of the appearances with central light, wrote me that he was of opinion that the free spines came from some of the compound hairs that might be upon the slide. Now, even granting this to be so, the *projecting spines*, their being beaded under oblique light, the beading of the free spines, seem to point to the fact that there is something yet to be accounted for that the theory of beads leaves untouched. This, then, represents the state of the case and conclusions which I incorporated in my paper nearly a year ago. Since then I have secured some specimens of Degeeria, and continued my experiments,—imperfectly, it is true, as time could not be spared for exhaustive research in this direction. I selected specimens of scales that were not mixed with hairs, and that showed no free spines, and, placing these in such a manner that the discharge from a Leyden jar could be brought to bear upon them, not only tore to pieces the scales, but scattered the spines far and wide. Again, by means of crushing in a small agate mortar, and even on the slide itself, by pushing about the covering glass with the handle of a dissecting needle, I have fractured a scale in such a manner that the spines lay free and side by side with the broken scale. What more is wanting to show that hairs and scales are of a similar structure?—the hair consisting of a shaft, upon the surface or exterior of which

are attached spine-like epithelial cells; the scale of a flattened shaft, so to speak, covered by analogous structures. I may state that the scales or even hairs from the live insect are not in a fit condition to break; they are too pliable and tough, so that I have not succeeded in doing anything in the way of breaking when the scales were in this state. By carefully drying in an ordinary drying oven, such as is used in chemical laboratories for drying precipitates, &c., the scales become much more brittle, and the greasy substance that can be pressed from them while fresh entirely disappears through the agency of a gentle and continued heating. It will require a discharge of some force to break off the spines, and several discharges in rapid succession will give best results. As it is desirable to examine the scales immediately after the action of the spark, some arrangement similar to that used in Germany for acting upon tissues with the induced current is most convenient. A rather thick glass slide, with two pieces of tin-foil pasted upon it at a distance apart just sufficient to allow of the passage of the spark, with the scales in the pathway of the discharge covered by another slide or strong cover glass, will make a simple and effective instrument. It must not be supposed that the blowing off of the spines is to be accomplished at every attempt. I find that when the spark passes across the scale from side to side, so as to strike the spines on their lateral surfaces, the chances are best of rending off the spines. Many attempts must fail, and many a fine "Test Podura" will have to be sacrificed, as the scales, either untouched or in fragments, will be oftentimes scattered to such an extent that only here and there on the slide can one be found; and then to mount the individual on a clean slide with thin cover, in order to examine with higher powers, is a very difficult matter.

The Anatomy of Balantidium coli.—The 'Northern Archives of Medicine' contains a paper by *M.* P. J. Wising, of Stockholm, on the anatomy and physiology of *Balantidium coli*, a parasite first described by Professor Malmsten in 1856. In 1869 a labourer, Lars Nilsson, sixty-two years of age, noticed for the first time the presence of small white worms in the motions from his bowels. About Christmas of that year he suddenly became affected with severe colicky pains, followed by considerable diarrhœa. This at last became so bad that there were as many as twelve stools a day; they were usually of the consistence of thin pea-soup, sometimes with firmer lumps intermixed, sometimes quite watery, and most frequently tolerably small in quantity. There was but slight tenesmus, while after some time the stools were streaked with blood. The microscope showed, in addition to the remains of food, a good many lymphoid cells, a few red blood-corpuscles, vibriones, and *Balantidium coli* in very large numbers. The more original researches of the author are directed to the question of the reproduction and propagation of the parasite. In a series of beautifully-executed lithographs the anatomical structure of the animal, including the peristome, with its adoral ciliæ, nucleus, nucleolus, and contractile vacuoles, the phenomena of coition and the process of reproduction, are represented. The last-named is carried on in this way. The body of the parasite elongates so that its length

becomes more than double its breadth, and the nucleus itself, also somewhat elongated, assumes a central position, with its long axis in the direction of that of the body. A tendency to constriction across the middle of both body and nucleus is the next change observed. This increases until the two halves are at last connected by a tiny filament only. The anterior half contains the old peristome quite unaltered, while in the posterior half new adoral ciliæ begin to develop from the ordinary ciliary layer. At this stage the nucleus parts in two, so as to form a separate nucleus for each half of the body. Usually only one contractile vacuole is now found lying near the back portion of each half, for the anterior vacuole does not generally appear until the separation has been quite completed; both halves of the animal gradually take on a more independent life, and, finally, the connecting filament gives way, and the two halves fall apart as distinct living beings. The anterior half, which contains the original peristome, is a fully-developed animal from the time of the separation; but the posterior half is as yet very unlike the full-grown parasite, into which it in time develops.

Bacteria in the Uterus.—Professor Waldeyer, of Breslau, records that he has found these in great numbers in the diphtheritic deposit on the inner surface of the uterus, the puriform mass from the lymph-vessels of the uterus and broad ligaments, the peritoneal exudation, both in the fluid and also in the whitish-yellow purulent fibrinous flakes. Haussmann formerly only found the Bacteriæ and vibriones in the lochial secretion. In the diphtheritic exudation these Bacteriæ lie between the pus-cells and those of the partly decomposed tissue elements. The puriform contents of the lymph-vessels consist of, besides the pus-cells, for the most part of Bacteriæ, and they are also found in the peritoneal exudations and flakes, mixed with pus-cells, young cells, and masses of fibrine, and even in the interior of the pus-cells.

Opaque Illumination under High Objectives.—Dr. W. A. Johnson has communicated to the Microscopical Society of Illinois another mode of managing the above. He employs Professor H. L. Smith's mode of making the objective its own illuminator; but instead of the silvered reflector, thin glass disk, or reflecting prism, used by Professor Smith and subsequent experimenters, he employs the binocular prism of Wenham's binocular. A beam of light reflected down the oblique body of the Wenham binocular, the prism being in position as for ordinary use, is of course condensed by half the objective upon the object in its focus; while the other half of the objective and direct body of the instrument is used at the same time as a monocular for viewing the object. A portion of the field is intensely illuminated, and definition is said to be better than with other arrangements.—*American Naturalist.*

The Internal Limiting Membrane of the Retina.—A paper on this subject appears in the 'Northern Archives of Medicine.' The author, *M.* G. Retzius, describes peculiar supporting filaments with trumpet-shaped terminal disks. He has in the same journal a second paper,

"On the Inner Layer of the Connective Tissue of the Eye." Both papers are of interest.

The Microscopic Anatomy of Skin Grafting.—In Part iv. of the 'Proceedings of the Swedish Society of Physics,' M. Misterton gives the results of six cases of skin grafting. He says that in all these, even where the transplanted piece of skin seems to disappear completely, the operation succeeds if only a few cells of the Malpighian layer of the cuticle remain possessed of life, so as to form a nucleus for a fresh epidermis formation. The operation of skin grafting is graphically and physiologically described in the paper.

The Embryology of Fossil Cephalopoda.—In vol. ii., No. 5, of the 'Bulletin of the Museum of Comparative Zoology' (U.S.A.), Mr. Alpheus Hyatt describes the results of a careful investigation with respect to the embryology and structure of ammonites and related cephalopods, by a study of the shell in its different stages of development, and also by a comparison of its characters with those of the living nautilus.

The Eozoon: Dr. Dawson and Professors King and Rowney.—Our readers are perhaps aware that Messrs. King and Rowney have attempted to prove that this unquestionably animal form was merely a peculiar mineral. They wrote a long paper setting forth their views. This has now been replied to by Dr. Dawson, and his answer is to be found in substance in 'Silliman's American Journal' (July, 1872). He (Dr. Dawson) says, that in opposition to the facts stated by him, the authors of the paper under consideration maintain that the structures are mineral and crystalline. "I believe that in the present state of science such an attempt to return to the doctrine of 'plastic force' as a mode of accounting for fossils would not be tolerated for a moment, were it not for the great antiquity and highly crystalline condition of the rocks in which the structures are found, which naturally create a prejudice against the idea of their being fossiliferous. That the authors themselves feel this is apparent from the slight manner in which they state the leading facts above given, and from their evident anxiety to restrict the question to the mode of occurrence of serpentine in limestone, and to ignore the specimens of Eozoon preserved under different mineral conditions. With reference to the general form of Eozoon and its structure on the large scale, I would call attention to two admissions of the authors of the paper, which appear to me to be fatal to their case. First, they admit, at page 533,* their 'inability to explain satisfactorily' the alternating layers of carbonate of lime and other minerals in the typical specimens of Canadian Eozoon. They make a feeble attempt to establish an analogy between this and certain concentric concretionary layers; but the cases are clearly not parallel, and the laminæ of the Canadian Eozoon present connecting plates and columns not explicable on any concretionary hypothesis. If, however, they are unable to explain the lamellar structure alone, as it appeared to Logan in 1859, is it not rash to attempt to explain it away now, when certain minute

* 'Proceedings R. I. A.,' vol. x.]

internal structures, corresponding to what might have been expected on the hypothesis of its organic origin, are added to it? If I affirm that a certain mass is the trunk of a fossil tree, and another asserts that it is a concretion, but professes to be unable to account for its form and its rings of growth, surely his case becomes very weak after I have made a slice of it, and have shown that it retains the structure of wood. Next, they appear to admit that if specimens occur wholly composed of carbonate of lime their theory will fall to the ground. Now such specimens do exist. They treat the Tudor specimen with scepticism as probably 'strings of segregated calcite.' Since the account of that specimen was published, additional fragments have been collected, so that new slices have been prepared. I have examined these with care, and am prepared to affirm that the chambers in these specimens are filled with a dark-coloured limestone not more crystalline than is usual in the Silurian rocks, and that the chamber walls are composed of carbonate of lime, with the canals filled with same material, except where the limestone filling the chambers has penetrated into parts of the larger ones. I should add that the stratigraphical researches of Mr. Vennor, of the Canadian Survey, have rendered it probable that the beds containing these fossils, though unconformably underlying the Lower Silurian, overlie the Lower Laurentian of the locality, and are, therefore, probably Upper Laurentian, or perhaps Huronian, so that the Tudor specimens may approach in age to Gümbel's *Eozoon Bavaicum*. Further, the authors of the paper have no right to object to our regarding the laminated specimens as 'typical' Eozoon. If the question were as to *typical ophite*, the case would be different; but the question actually is as to certain well-defined forms which we regard as fossils, and allege to have organic structure on the small scale, as well as lamination on the large scale. We profess to account for the acervuline forms by the irregular growth at the surface of the organisms, and by the breaking of them into fragments confusedly intermingled in great thickness of limestone, just as fragments of corals occur in Palæozoic limestone; but we are under no obligation to accept irregular or disintegrated specimens as typical, and when objectors reason from these fragments we have a right to point to the more perfect examples. It would be easy to explain the loose cells of *Tetradium*, which characterize the Bird's-eye limestone of the Lower Silurian of America, as crystalline structures; but a comparison with the unbroken masses of the same coral shows their true nature. I have for some time made the minute structure of Palæozoic limestones a special study, and have described some of them in the Trenton formation of Canada. I propose shortly to publish additional examples showing fragments of various kinds of fossils preserved in these limestones, and recognizable only by the infiltration of their pores and other minute structures. I shall also be able to show that in many cases the crystallization of the carbonate of lime and the infiltration of other substances have not interfered with the perfection of the most minute of these structures. The fact that the chambers are usually filled with silicates is strangely regarded by the authors as an argument

against the organic nature of Eozoon. One would think that the extreme frequency of silicious fillings of the cavities of fossils, and even of silicious replacement of their tissues, should have prevented the use of such an argument, without taking into account the opposite conclusions to be drawn from the various kinds of silicates found in the specimens, and from the modern filling of Foraminifera by hydrous silicates, as shown by Ehrenberg, Mantell, Carpenter, Bailey, and Pourtales.* Further, I have elsewhere shown that the loganite is proved by its texture to have been a fragmental substance, or at least filled with loose *débris;* that the Tudor specimens have the cavities filled with sedimentary limestone, and that several fragmental specimens from Madoc are actually wholly calcareous. It is to be observed, however, that the wholly calcareous specimens present great difficulties to an observer; and I have no doubt that they are usually overlooked by collectors in consequence of their not being developed by weathering, or showing any obvious structure in fresh fractures. With regard to the canal system, the authors persist in confusing the casts of it which occur in serpentine with 'metaxite' concretions, and in likening them to dendritic crystallizations of silver, &c., and coralloidal forms of carbonate of lime. In answer to this, I think it quite sufficient to say that I fail to perceive the resemblance as other than very imperfect, imitative. I may add that the case is one of the occurrence of a canal structure in forms which on other grounds appear to be organic, while the concretionary forms referred to are produced under diverse conditions, none of them similar to those of which evidence appears in the specimens of Eozoon. With the singular theory of pseudomorphism, by means of which the authors now supplement their previous objections, I leave Dr. Hunt to deal. With regard to the proper wall and its minute tubulation, the essential error of the authors consists in confounding it with fibrous and acicular crystals, and in maintaining that because the tubuli are sometimes apparently confused and confluent they must be inorganic. With regard to the first of these positions, I may repeat what I have stated in former papers—that the true cell-wall presents minute cylindrical processes traversing carbonate of lime, and usually nearly parallel to each other, and often slightly bulbose at the extremity. Fibrous serpentine, on the other hand, appears as angular crystals, closely packed together, while the numerous spicular crystals of silicious minerals which often appear in metamorphic limestones, and may be developed by decalcification, appear as sharp angular needles usually radiating from centres or irregularly disposed. Plate 44, Fig. 10,† is an eminent example of this; and whatever the nature of the crystals may be, they have no appearance in the plate of being tubuli of Eozoon. I have very often shown microscopists and geologists the cell-wall along with veins of chrysotile and coatings of acicular crystals occurring in the same or similar limestones, and they have never failed at once to recognize the difference, especially under high powers. I do not deny that the tubulation is often

* 'Quart. Journ. Geol. Soc.,' 1864.

† Ophite from Skye, King and Rowney's paper, 'Proc. R. I. A.,' vol. x.

imperfectly preserved, and that in such cases the casts of the tubuli may appear to be glued together by concretions of mineral matter, or to be broken or imperfect. But this occurs in all fossils, and is familiar to any microscopist examining them. How difficult is it in many cases to detect the minute structure of Nummulites and other fossil Foraminifera. How often does a specimen of fossil wood present in one part distorted and confused fibres or mere crystals, with the remains of the wood forming phragmata between them, when in other parts it may show the most minute structures in perfect preservation. But who would use the disintegrated portions to invalidate the evidence of the parts better preserved? Yet this is precisely the argument of Professors King and Rowney, and which they have not hesitated in using in the case of a fossil so old as Eozoon, and so often compressed, crushed, and partly destroyed by mineralization. I have in the above remarks confined myself to what I regard as absolutely essential by way of explanation and defence of the organic nature of Eozoon. It would be unprofitable to enter into the multitude of subordinate points raised by the authors, and their theory of mineral pseudomorphism as discussed by my friend Dr. Hunt; but I must say here that this theory ought, in my opinion, to afford to any chemist a strong presumption against the validity of their objections, especially since it confessedly does not account for all the facts, while requiring a most complicated series of unproved and improbable suppositions. The last point which I shall mention is the taunt that so little further progress has been made in the investigation of Eozoon. With reference to this, I beg leave to doubt whether a process of confounding the actual structure of Eozoon with all manner of dendritic and crystalline forms, in the way followed by the authors, would constitute progress. But in so far as careful comparison with all specimens which have been recently found is concerned, some progress has been made, and I trust that it will soon be possible to bring forward not merely additional specimens illustrative of the structure of Eozoon, but fresh evidence of its wide geographical range, and also links of connection with fossils of the Palæozoic rocks. The discovery recently made in Massachusetts, and alluded to by Messrs. Rowney and King, is itself not without importance. In the meantime I am content to accept the investigations of Messrs. King and Rowney as nearly exhaustive of the natural history of those imitative forms which may be confounded with Eozoon, and therefore as in a certain way useful in the further prosecution of the subject. As already stated, I am at this moment engaged in following out, as opportunity offers, two lines of investigation bearing on the following points:—(1) The study of the Lower Silurian and Primordial successors of Eozoon; and (2) that of the tubulation and other structures similar to those of Eozoon preserved in the Palæozoic rocks."

The Histology of the Penis.—One of the best papers that we have for some time seen is that on the above subject, in a recent number of the 'New York Medical Journal.' It deals very fully with the whole subject, and is illustrated by a number of cuts demonstrating the more important points in the histology of this organ. Besides the author's

own opinions on the subject, he has given those of the most recent foreign authorities, and thus has given the reader a useful summary of the work done on this subject. We hope, when the blocks have been received, to lay the paper fully before our readers.

The precise period at which the Laminar Membrane appears.—Dr. Joulin, who has been known for his researches on the placenta some years since, presented to the Academy of Medicine a memoir in which he endeavoured to show that the chorion at term did not, as is generally described, cover the fœtal surface of the placenta, but that it was replaced by a membrane of new formation called the "laminar membrane." The object of his present communication is to show the precise period at which the chorion disappears, when new membrane takes its place. The conclusion he arrives at is that the chorion begins to disappear from the fœtal surface of the placenta about the tenth or eleventh week of utero-gestation, the new membrane being formed at first in connection with the great vessels, and gradually extending over the entire placenta. Microscopically it consists of fibres in distinct layers, and can be readily distinguished from the other membranes surrounding the fœtus.

The Structure of the Uterus has been very fairly investigated by Dr. Snow Beck, and his researches will be found more fully stated in the 'Obstetrical Transactions' (vol. xiii.). In his paper he advances views somewhat different from those generally taught. He does not admit the existence of a separate mucous membrane lining the uterus. He describes the walls of the uterus as consisting of two parts:—(*a*) The contractile tissue destined to expel anything contained in the uterine cavities, and which exists in greatest amount at the outer and middle portions, whilst towards the inner surface it is divided into smaller striæ with considerable spaces between them. (*b*) The soft tissue which lines the internal surface, where it exists alone to some thickness, and afterwards penetrates a considerable distance into the substance, lying between the striæ and bands of the contractile tissue. It is destined to form the placenta which is thrown off at the completion of parturition, together with the inner layer from the whole of the inner surface of the uterus, as decidua. But it is only the inner layer of the soft tissue which comes away, the principal portion yet remaining in the inner portion of the walls. The decidua and the formation of the placenta appear to be the natural development of the soft tissue which exists at the inner surface of the unimpregnated uterus, and at the completion of parturition it is thrown off from the surface as now useless products, and there does not appear any sufficient reason to apply special designations to particular portions of it.

NOTES AND MEMORANDA.

Angular Aperture of Immersed Object-glasses.—Mr. Wenham having expressed his willingness to assist in testing the aperture of Mr. Tolles' object-glass, which he announced in his letter to the Editor, dated 15th July,* would be sent, in order to have the result verified or not, we have to announce that the glass has not arrived. Though Mr. Wenham has declined further discussion directly with Mr. Tolles, we think that the subject has not been quite satisfactorily determined on the question of the discrepancies, or perhaps fallacies of measurement of extreme angular apertures. If Mr. Wenham holds this opinion, we hope that he will give us the result of his experience, irrespective of any individual question of controversy.

The British Association.—At the late meeting the amount of microscopical work brought before the Association was neither large nor important. And this we should not have been led to expect from the fact that the President for the year was a man who especially stands high among English, and we might add Continental naturalists, as a microscopist and a master of histology. Indeed, with the exception of the President's Address, and a few of the special addresses, there was not much to be noted. Dr. Carpenter's opening remarks were, as might have been anticipated, a masterly summary of the present condition of science. And while he left nothing untouched, he did not fail to comment on the readiness with which men in special departments were ready, not only to believe in particular conclusions drawn from very recent experiments, but were ever disposed to go beyond them, and to give absolute credence to a multitude of other mere speculations which took their rise in them. We think the President touched an important subject in these remarks, and we doubt not that the tendency will be much lessened for the future. We believe the Address may still be had by communication with the Secretaries.

A Prize for Microscopic Work is offered by the Royal Danish Society, and we trust some of our Fellows will attempt competition for it. It is now a hundred years since the celebrated observations of Müller on agamous reproduction (gemmiparity) of the Naiades was published, and although there is no reason to question their perfect accuracy on all essential points, it is very desirable to have them resumed in the actual light of science and with the means which it possesses to-day. Schultze, Leuckart, and Minor have furnished history with valuable contributions of the manner of reproduction of the Naiades properly called, as Claus and Lankester have of Chætogaster; nevertheless, science ought to be in possession of materials sufficient for intelligence on all points of which it is necessary to keep account. It is not known definitely what is the first origin of buds or first individuals, and the relations between the modes of gemmiparous and scissiparous reproduction consequently need to be better explained; complete evolution, from the moment when one Naiad leaves the egg

* 'M. M. J.,' page 147, July, 1872.

to that in which, among the generations sprung from that Naiad, there are found again sexual distinctions, has not been studied in all its phases, and it is still a question whether the same individuals (zoöides) are gemmiparous and sexual, or if the sexual and agamous reproductions are strictly distributed over different individuals or generations. As for the other two groups of Anellides in which agamous reproduction has been observed hitherto, namely, the Syllides and the Sepulides, the question is almost at the same point. For these reasons, the Society desires to urge a thorough research, and one answering the actual demands of science, of agamous reproduction and all the points pertaining to it, of one of the groups of these setiferous Anellides. It therefore offers its gold medal as a prize to the one who shall solve this question in a satisfactory manner, either for one or several species of the group of Naiades (comprising Chætogaster) or for one or several species of Syllides or Tubicolides. The papers should be accompanied by the necessary drawings explaining the points on which the researches have especially borne. The answers to these questions may be written in Latin, French, English, German, Swedish, or Danish. The papers must not bear the name of the author but a motto, and must be accompanied by a sealed note, furnished with the same motto, enclosing the name, profession, and address of the author. The members of the Society who live in Denmark do not take part in the competition. The prize awarded for a satisfactory answer to the question proposed is the gold medal of the Society (value, about 20*l*.). Papers must be addressed before the end of October, 1873, to the Secretary of the Society, Counsellor J. Japetus Sm. Steenstrup, Copenhagen.

Photographing Amphipleura pellucida.—Dr. J. J. Woodward gives, in a late number of the 'American Naturalist,' an able paper, accompanied by a photographic plate of great excellence, on the subject of photographing *A. pellucida*. After describing other means of conducting the operation, he states that that by sunlight involves least trouble and expense, and may be best managed as follows:—Erect a perpendicular wooden screen about two feet square on one edge of a small table. Cut in this a circular hole an inch and a half in diameter at about the height of the under surface of the stage of the microscope. On the outside of this hole mount a small plane mirror which can be adjusted by passing the hand to the outside of the screen. On the inside, cover the hole with the ammonio-sulphate cell. (A piece of dark blue glass will answer the purpose, though not so well.) Now move the table to a window through which the direct rays of the sun can fall upon the mirror; adjust this so as to throw a nearly horizontal pencil of parallel rays through the hole, and place the microscope in the shade of the screen in such a position that the parallel blue rays will fall on the under surface of the Amphipleura slide at an angle of from 50° to 75° with the plane of the slide (I suppose the frustule to be examined has first been found by ordinary daylight or lamplight). Next place a small bull's-eye or any other condenser of from one to three inches focal length (mounted on a separate stand or on a radial arm) in the parallel pencil in such a

position as to concentrate the light, at the angle above indicated, upon the frustule under examination. After this nothing remains but to regulate the cover correction and the fine adjustment. The precise angle which should be given to the illuminating pencil will vary with the angle of aperture of the objective used. As a rule it should be less than half the angle of aperture of the objective, and 70° to 75° is the maximum angle which should be given even for objectives of 170° angle, a greater angle distorting the image without improving the definition. The same results can be obtained by using a heliostat to fix the direction of the solar rays, and obtaining obliquity by an achromatic condenser of from 130° to 150° suitably decentred. On account of the stability of the illumination this method is especially suitable for photographing the Amphipleura, but the simpler method above described answers every purpose if the object is to compare objectives.

An Optical Instrument of Mr. Tolles's Device.—The 'Lens' states that Mr. Tolles has just made to order a novel instrument. It consists of a vertical tube, seven-tenths of an inch in diameter inside, twelve inches high, supported on a base about two inches square. On one of the vertical faces of the base is a compound achromatic telescope lens three-quarters of an inch in diameter. Between the lenses is a total reflecting prism; at the upper end of the tube a solid ocular. Looking down the tube, the observer can obtain a well-defined image, magnified thirteen diameters, of an object distant three feet horizontally from the lens. "As the term micro-telescope has been applied to Tolles's instruments where one tube and ocular serves for both microscope and telescope, this instrument may be called a telemicroscope."

The Paraboloid an Immersion Instrument.—Notwithstanding the introduction into use of special contrivances as immersion paraboloids, it may not have occurred to all who use the microscope that the ordinary form of parabolic illuminator is capable of being used wet with excellent results. Placing the microscope in a vertical position, says T. D. B. in the 'American Naturalist' for August, and greasing the rod in the centre of the paraboloid to keep the water from running out by the side of it, the cup of the paraboloid is filled with water heaped up as far as can be without running over, and then brought up until the water comes in contact with the under surface of the slide. The direction of the rays leaving the paraboloid is not altered by this arrangement, but dispersion at two surfaces is avoided and the rays enter the object-slide without the usual refraction, and at such an angle as to suffer total internal reflexion before reaching the objective. With the highest objectives generally used with black-ground illumination, as a $\frac{1}{4}$th of 75° to 110°, the object seems no brighter than usual, but the field is free from the foggy diffuse light, otherwise present, and the object appears, beautifully distinct, upon a jet-black ground. Even a $\frac{1}{8}$th or $\frac{1}{6}$th of 130° gives the same effect of a deep black background, and shows the object with good stereoscopic effect in Wenham's binocular. With objectives of 170°, the main effect is that of a dark background, though not so perfect as with the lower angles.

CORRESPONDENCE.

Correction of an Error.

To the Editor of the 'Monthly Microscopical Journal.'

Troy, New York, *July* 30, 1872.

Dear Sir,—Please make the following correction to page 17 of your July number. In fifth line of note, for "applications" *read* appliances.

Yours truly,
R. H. Ward.

PROCEEDINGS OF SOCIETIES.

Brighton and Sussex Natural History Society.

The Microscopical and Botanical Displays at the Soirées of the British Association Meeting at Brighton.

The executive committee of the British Association having asked the Society to organize the microscopic displays at the soirées of Thursday, August 15th, and Tuesday, August 20th, and to arrange, as far as practicable, a living flora of the county for the soirées and temporary museum, the following gentlemen were appointed a committee to carry out the first, *viz.* Dr. Hallifax; Messrs. Hennah, Sewell, C. P. Smith, Hazelwood, W. H. Smith, F. E. Sawyer, G. Scott; the President, Mr. Hollis; and the Hon. Secretaries, Messrs. Wonfor and Onions; and to arrange the flora, Messrs. W. Mitten, Nourse, Simonds, C. P. Smith, W. H. Smith, Benjamin Lomax, D'Alquen, G. Davies, W. H. Hallett (Local Treasurer of the British Association), H. J. H. Nicholls, and the officers of the Society were elected a committee.

The microscopical committee feeling that the leading feature of one of the soirées ought to be marine, drew up the following suggestive programme:—

British Association for the Advancement of Science.

Brighton Meeting.

Particulars of the intended Microscopical Displays for the Soirées arranged by the Brighton and Sussex Natural History Society.

Thursday, August 15.—*Objects Illustrative of Marine Life.*

Algæ—Sea-weeds, &c., fructification.
Diatomaceæ—Living and prepared valves.
Rhizopoda—Foraminifera—porcellanous, arenaceous, &c.
Polycystina—recent and fossil.
Spongiadæ—Calcareous, silicious, &c.
Spicules, gemmules, skeleton, &c.

HYDROZOA—Corynne, sertularia, acalepha, &c.
ACTINOZOA—Alcyonium, gorgonia, helianthoida, &c.
ECHINODERMATA—Star fishes, ophiocoma, spatangus, comatula, cidaris.
Echinus (shell, spines, ambulacra), &c.
Holothuria, synapta, &c.
POLYZOA—Lepralia, flustra, halodactylus, bugula, avicularia, &c.
Ascidia, salpidæ, &c.
MOLLUSCA—Shells, palates, cilia, eyes (cuttle), &c.
ANNELIDA—Serpula, aphrodita, &c.
CRUSTACEA—Zoea of crab; eye, shell, &c., of shrimp, prawn, lobster, &c.
Pycnogonidæ, caprella, balanus, &c.
PISCES—Teeth, scales, dermal skeleton, &c.
REPTILIA—Turtle, &c.
MAMMALS—Teeth, bone, hair, whalebone, &c.

Tuesday, August 20.—*Objects of General Interest, either from their Novelty or Scientific Value.*

At both soirées full opportunity will be given for the display and inspection of microscopes and other optical instruments, with their latest improvements and additions.

BRIGHTON, *July*, 1872.

And asked the co-operation of the different country and metropolitan societies, as well as microscopists throughout the county, and the principal London makers. Promises of help came in very freely; but although the rooms in which the soirées were held included the Dome, the Royal Exchange, and the New Library and Picture Gallery, the space at their disposal did not admit of more than about eighty microscopes, allowing sufficient room to each to prevent crowding.

The arrangements made by the committee admitted the placing the members of societies at one long table, capable of accommodating fifty microscopes, and giving a similar table at another part of the Corn Exchange to the opticians; this enabled the Society to carry out the programme as arranged by the committee.

A number of living objects had been collected and dredged by Dr. Badcock, and other gentlemen; but to guard against accidents some 300 objects, illustrative of marine botany and zoology, were provided by Drs. Ormerod, F.R.S., and Hallifax; Messrs. T. Curties, Hennah, and Wonfor; classified in accordance with the memorandum issued by the Society.

The different societies which co-operated with the Society, and were represented by deputations, were the Royal Microscopical Society, the Quekett Club, the Lewes Natural History Society, the Croydon Club, the Eastbourne Natural History Society, the West Brighton Club, and the Brighton Medico-Chirurgical Society; while the gentlemen who exhibited were, Drs. Badcock, Carpenter, F.R.S. (the President of the Association), Hall, Hallifax, Kebbell, Ormerod, F.R.S., and Sim; General Worster; Captains Marshall Hall, and Noble; Revs. J. H. Cross and W. Payne; Messrs. Ardley, Aylen, S. Burrows, T. Cooper, T. Curties, J. Dennant, T. Glaisyer, R.

Glaisyer, Gwatkin, Gwatkin, jun., Hillman, Histed, T. H. Hennah, McIntire, Mitchell, Nash, W. D. Savage, F. E. Sawyer, J. Sewell, Shaft, C. P. Smith, W. H. Smith, Tatham, Turner, Unwin, Upton, C. A. Wells, Williams, T. W. Wonfor, and W. Wonfor, jun. About a dozen microscopes, belonging to different members of the Society, were placed at the disposal of Dr. Carpenter, for the purpose of exhibiting his deep-sea dredgings.

At the second soirée many very choice and interesting objects, including several living objects, were exhibited by the same gentlemen.

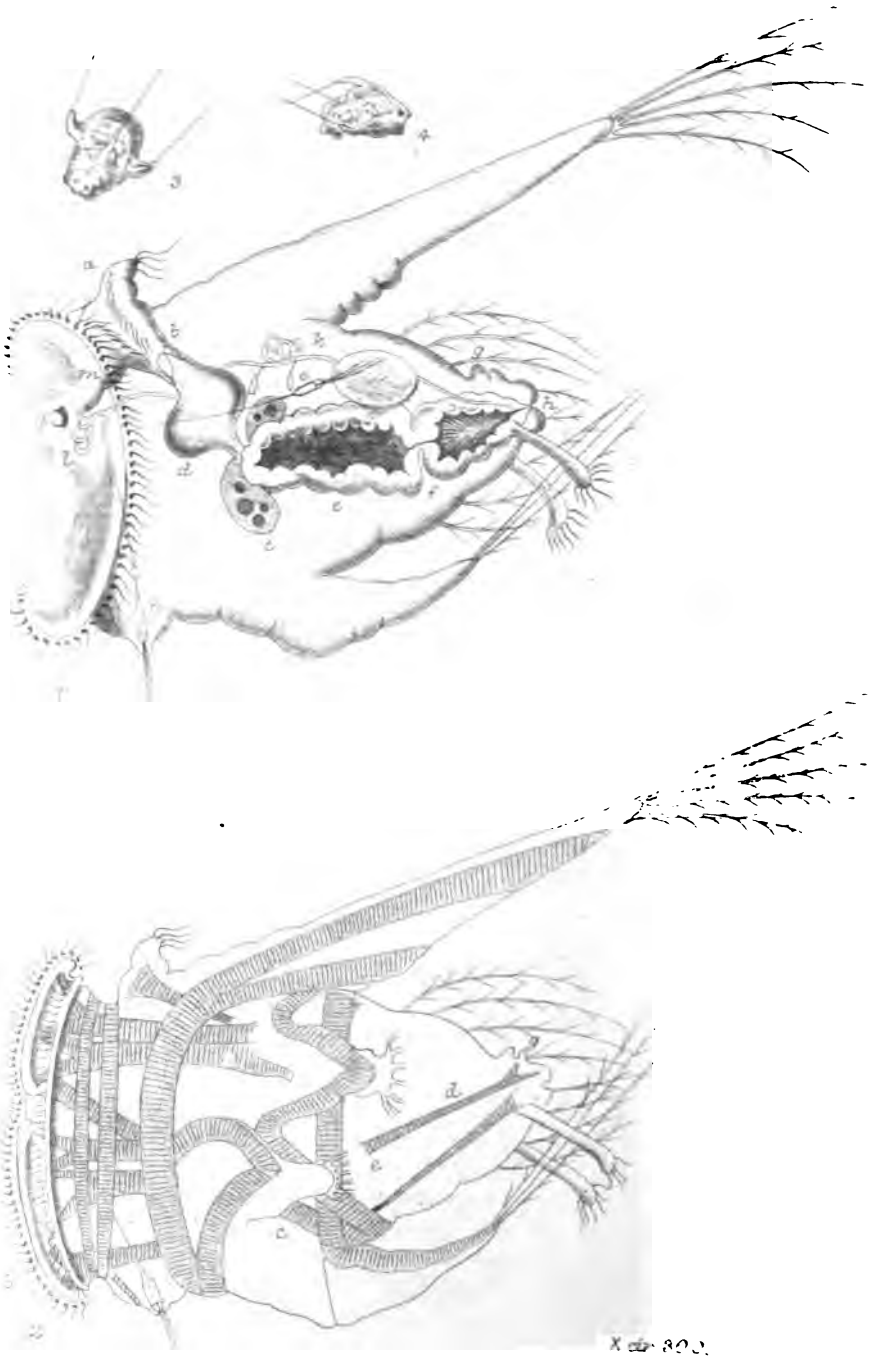
The opticians who responded to the Society's invitation and exhibited microscopes, were Messrs. Ross, who exhibited their new patent $\frac{1}{15}$th and $\frac{1}{10}$th objectives, and their tank microscope; Murray and Heath, who exhibited Dr. Gladstone's crystals; Ladd, who exhibited spectroscope, &c.; Horne and Thornthwaite; How, who exhibited hand and class microscopes; Swift; Moginie, who also exhibited revolving stereoscope with very beautiful slides; C. Baker, who placed several microscopes and lamps, and a collection of objects at the Society's disposal; Norman, who exhibited three large frames of very choice objects; and Wheeler, who exhibited book-folding cases containing 1000 objects.

The botanical display was arranged in three upstairs rooms of the new museum and library: two rooms were filled with living plants obtainable in the county during the month of August, and to the number of nearly 500 species were arranged in two tiers in long, narrow boxes filled with damp sand; and as this part was to be kept open not only during the Association's visit, but for ten days afterwards, the plants which withered were replaced from day to day by fresh ones.

In the third room dried plants were exhibited in three rows on the walls; while mosses, lichens, &c., Sowerby's 'English Botany,' 'Nature Printed Sea-weeds,' and other botanical works from the Society's library lay on tables. The management of the living flora was undertaken by Mr. Benjamin Lomax, and of the dried plants by Mr. C. P. Smith, who admirably carried out the views of the committee.

The following ladies and gentlemen contributed to the living flora:—Miss A. Woodhouse (Eastbourne) and Robinson (Saddlescombe), Mrs. Hodgson, the Rev. — Brewer (Worthing), Messrs. Griffith, Boys, Hadlow, T. H. Hennah, A. Bigge, H. J. H. Nicholls, F. Merrifield, W. Mitten (Hurst), H. Willett, G. D. Roper (Eastbourne), Nourse, Hall, G. D. Sawyer, W. H. Smith, C. P. Smith, G. Davies, B. Lomax, Helmsley (Hassock's Gate), and C. Hill (Rockhurst).

Among the exhibitors of dried plants were Miss Hall (Eastbourne), who lent a fine collection of flowering plants; Mr. W. Mitten, mosses and hepaticæ, all Sussex, including several unique species; Mr. C. P. Smith, Sussex grasses, and marine and fresh-water algæ; Mr. G. Davies, Sussex lichens, including the rare *V. Garovaglii* and *V. microcarpa*; Mrs. Hodgson, Brighton marine algæ; Mr. R. C. Roper, leaf fungi; Mr. Clarke, Mayor of Saffron Walden, new British plants; and Miss Turner, New Zealand plants.



Pedalion mira.

THE

MONTHLY MICROSCOPICAL JOURNAL.

NOVEMBER 1, 1872.

I.—*Is Pedalion a Rotifer?* By C. T. Hudson, LL.D.

PLATE XXXIII.

Is Pedalion a Rotifer? To this I may reply, "What is a Rotifer?" a question as difficult to answer as Sir Robert Peel's famous query, "What is a pound?" Few things are so difficult to construct as a good definition, and there are few better ways of tripping up a troublesome antagonist than that of suddenly calling upon him to define some term which he is using. He must be a very capable man who can at a moment's notice so reply, that his definition may not be torn to bits almost before it has left his lips.

It is easy enough to define the typical form of a natural group of animals, or even to include in the definition forms that must be placed pretty near the central one; but, in the ambitious search for a definition which shall include many families, as we get further away from the typical form, we find that one by one all the positive statements are disappearing from our definition, till at last we have nothing left but the mere shuck of a proposition, shelled of everything worth the stating. Take the definition of one of the zoological manuals:—"Rotifers are microscopic animals, contractile, with vibratile cilia at the anterior part of the body, which by their motion often resemble a wheel moving rapidly. Intestine distinct; terminated at one extremity by a mouth, at another by an anus; generation oviparous, sometimes viviparous."

Now there are rotifers that have no "vibratile cilia at the anterior part of their bodies," others that have no intestine or anus, so that including these the above definition must shrink down into "Rotifers are very small animals that lay eggs."

That it is not an easy matter to answer off-hand the question "What is a rotifer?" may be readily seen by turning to one of our latest text-books, where no less than two handsome octavo pages are taken up in describing the characteristics of the class. I take for granted that the author considers the whole of this necessary in replying to the question, "What is a rotifer?" for if the first

two sentences are to be considered as constituting the definition of the term "Rotifera," then the definition will collapse as completely as the former one under the weight of various aberrant (but clearly rotiferous) forms. Let us see: rotifers are "Vermes with a retractile ciliated disk at the anterior extremity of their bodies which are ordinarily microscopic in size, though they may attain as large a size as $\frac{1}{36}$th of an inch in length. They are usually more or less plainly annulated externally, but they never are divided internally into compartments by any transverse septa."

Now it is not long ago that I read an account of a discovery in Germany of a rotifer without any ciliated disk at all; this, if true, disposes of nearly the whole of the first sentence. As to the second sentence, the first part excludes the whole of the Brachionæa; and the second excludes the Floscules. Here again, then, the definition would shrink down into "Rotifers are small worms"; a statement which I trust that *Pedalion* will effectually dispose of.

Happily *Pedalion*, in all but its six limbs, too closely resembles such forms as *Brachionus pala* and *Triarthra longiseta*, to compel us to discuss such a knotty question as that of how much of a rotifer's form and structure an animal may lose and yet remain a rotifer. In Plate XXXIII., Fig. 1, I have taken the liberty of removing two of *Pedalion's* limbs as well as all its muscles, so as to show its internal structure. Its large trochal disk, though of a pattern peculiar to itself, is still constructed on the same general plan, and with precisely the same object as those of *Rotifer vulgaris* and *Melicerta*. An imperforate oval disk bears on its edge large cilia, which by merely moving up and down, draw the water into currents setting right against the face of the disk. These currents on striking the disk are forced along the face of it outwards in all directions between the large cilia. Below the edge of the trochal disk lies a circular groove which surrounds the whole animal at its neck, and commences and terminates at its mouth. The outer edge of the groove is densely ciliated, and the cilia are very fine and set very closely together. When, then, the currents due to the larger cilia drive their floating atoms over the edge of the trochal disk, the smaller cilia lash the atoms back into the groove and hurry them along on both sides of the animal towards the mouth. This process can be readily watched in *Melicerta* or *Limnias*, and not quite so easily in *Œcistes*, and has been often described: in *Pedalion*, too, it can be distinctly seen by those who have either the patience to watch for a favourable opportunity, or the good luck to catch the animal resting at the proper moment.

Pedalion's trochal disk has a curious thin portion at each side (Fig. 2, *a*), bounded by branches of two longitudinal muscles (Fig. 2, *b*), and for the purpose I believe of permitting the disk to

be neatly folded when acted upon by the retractor muscles. Of these *Pedalion* possesses no less than six on each side (Fig. 2, *b*), as well as one long one (Fig. 2, *c*), which is attached to a point in the mid-dorsal line just under the dorsal limb, and stretches obliquely thence to the trochal disk, where it is fastened symmetrically by two branches.

To return to the atoms which have been driven along the ciliated groove till they reach the buccal funnel (Fig. 1, *a*). This latter resembles that of *Triarthra;* it is densely ciliated, and leads directly to the mastax. The mastax is like that of *Limnias ceratophylli;* and the entrance to it from the buccal funnel is guarded as in *Brachionus Mülleri* (and other species) and in *Floscularia campanulata*, by two chitinous lips (Fig. 1, *b*), if I may so term them, which are often raised to secure some desirable morsel. The food after being comminuted in the mastax enters a short œsophagus, and passes thence into a nearly cylindrical stomach. The walls of the stomach are very thick and elastic. In a dying specimen I have seen the food expelled and the walls close right in upon themselves. In general the stomach is greatly distended with an orange-coloured food, and gets sadly in the way of anyone who wishes to make a paper for the Microscopical Journal. At the anterior end of the stomach are the usual pair of gastric glands (*c*); less spherical in form, it is true, than they are in many rotifers, but less aberrant than those of *Pterodina patina* or of *Brachionus Mülleri*. I am pretty sure, too, that I saw now and then two other small glandular-looking bodies on the œsophagus, similar to those of *Euchlanis deflexa*, figured by Gosse in his paper in the 'Philosophical Transactions' on the manducatory organs of the rotifers; plate xvi., fig. 12, *n*.

From the stomach the food passes into a ciliated intestine (*f*), a broad short chamber with even thicker walls than those of the stomach, and with much coarser cilia. Although the division between the stomach and intestine is perhaps more marked in *Pedalion* than in the majority of rotifers, still it exists to some degree in all that I have seen except in *Asplanchna priodonta;* though in some it is little more than a temporary folding-in of the lower third of the alimentary canal.

The fæcal matter is expelled at the anus (*h*). For some time I imagined that the anus lay at the point (*g*) in the ventral surface, where is seen a curious projection. It is true that this would be a unique position—but then a rotifer with six legs may be considered as fairly entitled to have it in a unique position. Moreover, both the ciliated intestine and the passage of the anus are often so completely closed that no one would suspect there was an opening. Add to this that the viscera do not always lie in the handy position

in which I have drawn them; but so fill the posterior extremity that there would be no apparent difficulty in the aperture's being at (*g*). After I had seen the fæces expelled and had ascertained the true position of the anus, I was still puzzled with the spot (*g*). There was an appearance of corrugated skin overlying the ventral surface round about it, which made me fancy at first that the particular animal I was looking at was unhealthy, or had suffered some accident; and that the projection at (*g*) was not normal. However, this explanation would not do, for almost every specimen had the same appearance. It is only quite lately that I arrived (as I believe) at the right conclusion. I had the good fortune to find a *Pedalion* with an almost empty stomach and intestine, with a skin brilliantly transparent and quite free from the various parasites that often mar its beauty. The first glance at this gem showed me a muscle which had before escaped me, and which I have described above, and drawn in Fig. 2, *c*. The second showed me another new pair of muscles (Fig. 2, *d*), each with one branch attached to the posterior extremity and the other at (*g*), the broad attachment being at (*e*), and the effect of the whole arrangement to give a sideways pull at (*g*). It occurred to me at once that this must be the aperture of the oviduct, and that the corrugated skin surrounding it is due to the distension caused by the passage of the large eggs. It is true that such an aperture is almost an unheard-of luxury in a rotifer, and that *Asplanchna*, I think, alone possesses one; and in its case the aperture is a necessity, not a luxury; for without it this unhappy rotifer would have to expel its young as well as its fæces from its mouth. But the ovarian apparatus in *Asplanchna* is precisely similar, and I have seen it used; I have seen the aperture opened by its proper muscle, and the young *Asplanchna* expelled. The obvious way to determine the point would be to watch *Pedalion* laying an egg; but although I have had scores of specimens under observation I have never seen an egg laid. I watched in vain one specimen, in which there was an egg apparently nearly mature, for upwards of two hours; and then I desisted, for eyesight is more precious than rotifers. It is, however, of little consequence, for rotifers are so constantly laying eggs that some observer is sure to see the process next year.

Pedalion lays both female and male eggs, and, with respect to them, resembles the majority of the rotifers in the following points; *viz.* eggs of different sexes are not found on the same individual; virgin females lay female eggs during the greater part of the year; male eggs are half the size of female ones, and are carried in clusters of often a score at a time; and, finally, the males are small abortions without any organs for procuring or digesting food, appear at only one period of the year, and live a very short time—

sometimes not an hour. They are shown in Figs. 3 and 4, of their proper relative size, swimming round their mother.

Starting from somewhere on or under the intestine, and at its posterior extremity, are the two usual convoluted tubes (Fig. 1, *k*). Each of these has one ordinary bunch of loops at (*k*), with two vibratile tags just above it, and another bunch at (*l*), close under the eye, with a third vibratile tag close adjoining.

This system of tubes and tags is most difficult to see in *Pedalion;* not only are the lower portions of the tubes unusually transparent and unusually free from loops and tags, but the upper portions, where alone are the loops and tags, are hidden generally behind a stout striated muscle running from near the eye to the outer limb, and the muscle itself has frequently in front of it some portion of the curved and folded surfaces of the same limb.

Under these circumstances, it is needless to say that the tags and loops are not to be seen; and these unfavourable conditions are often aggravated by the stomach's being so distended as to form a coarse shifting yellow background to every portion of the field of view. It was not till I had nearly exhausted my stock of rotifers that I caught sight of the vibratile tags again this year; but since then I have repeatedly traced the tubes from end to end on several specimens.

Pedalion has a nerve mass (Fig. 1, *m*) closely applied to the buccal funnel, with two eyes (clear spheres on plates of red pigment) seated on it, and with nerve threads connecting it to the ganglionic intumescences of the dorsal and shoulder antennæ (*n*, *o*). In all these respects it closely resembles *Triarthra;* and a somewhat similar nervous system is visible in most of the free-swimming rotifers. There are also two delicate threads, commencing at the aperture of the oviduct, but whose other ends I have been unable to find.

I may here point out a curious and persistent difference between the free-swimming rotifers and the tube-forming ones. Most of these latter—*Melicerta*, *Limnias*, and *Œcistes*, for example—have their antennæ on the ventral surface, just below the ciliated chin, which underlies the mouth: but the free-swimmers have their antennæ on the dorsal surface, the normal number appearing to be three; one on the mid-dorsal line at the neck, and either two at the sides, as in *Triarthra*, *Hydatina*, and *Asplanchna*, or two at the posterior extremity, as in *Synchæta*.

The ciliated projections at the posterior extremity of *Pedalion* seem at first sight as abnormal as its six limbs: but as they exude a viscid secretion I have little doubt that they represent the "pincers" of *Brachionus*, *Synchæta*, and others, none of which "pinch," but all of which secrete a web by which the animal moves itself at will. In *Brachionus* the "pincers" are at the end of the

tail of the pseudopodium; in *Pterodina* the pincers disappear and the tail ends in a ciliated cup; while in *Pedalion* the tail disappears and the pincers are ciliated and set widely apart.

These projections are of various shapes and most varied length, and Mr. Slack called my attention to the fact that in some specimens the cilia were invisible. I did not find this to be often the case in my specimens; but I did meet with one or two in which I could not see the cilia. Possibly—as suggested by Mr. Slack—the cilia may at times cease to vibrate, and then lie close to the organ which carries them; and possibly they may be shed by an animal which is not quite healthy. Indeed, I found one pair, on one of which there was only one cilium feebly lashing the water, and on the other but two stumps: there would be nothing unusual in this, as I have frequently seen *Floscularia campanulata* shake off the long setæ which fringe its disk, and have seen the pieces float away. The cilia, too, on the trochal disk of dying rotifers come off on the slightest disturbance; and *Stephanoceros*, when unhealthy, may be seen to throw off considerable portions of its fringed arms.

Pedalion's internal structure, as detailed above, closely resembles that of any one of the typical rotifers. If this be admitted, it must, I think, also be conceded that through some of its forms the class Rotifera is linked to the Crustacea rather than to the Annulata.

Who, looking at *Pedalion's* pairs of jointed limbs worked by appropriate muscles, would think of placing it among the worms?

Gosse long ago insisted that the Rotifers must be ranked among the Arthropoda, adducing in evidence of this the approach of the manducatory apparatus to that of an insect; the curious condition of the males, resembling that of the males of the Cirripedia; and the presence of true condyles in the joints of such rotifers as *Dinocharis* and *Polyarthra*. To these three weighty reasons may now be added *Pedalion's* three pairs of legs; and the effect of the whole is, I think, irresistible.

In Dr. Rolleston's 'Forms of Animal Life' it is stated that the sub-kingdom Vermes consists of animals "bilaterally symmetrical, very various in shape and other external characters, but agreeing in the absence of heteronomy from their post-cephalic regions, in not possessing hollow segmented limbs, and in having their locomotor muscles closely connected with their integumentary system, not only on the ventral, but also on the dorsal and lateral aspect of their body walls."

Now, if the above is to be accepted as the definition of the sub-kingdom Vermes, then surely it excludes *Pedalion*: for *Pedalion* is strikingly heteronomous both as to its external shape and internal organs, possesses six hollow limbs, and though its muscles are necessarily attached to the integument, yet they cannot be said to

be closely connected with the integumentary system; the majority of them touch the integument at their extremities only, and moreover lie often well away from it, and may be seen now and then to hang quite slack, or even to form wide loops; and *one*, *viz.* that figured at Fig. 2, *c*, actually traverses two-thirds of the innermost portions of the animal's body, passing from the mid-dorsal line to the face of the trochal disk, to which it is attached by a double head.

Nor is *Pedalion* the solitary rotifer which refuses to submit to this definition. The great majority of rotifers are fairly heteronomous, even as to external shape, and undoubtedly so when both external shape and internal organs are taken together. No transverse section, however thin, of any rotifer that I am acquainted with, repeats itself in any portion of the body; while *the* distinctive feature of a worm is, that transverse sections of definite thickness do repeat themselves so remarkably that there seems no reason why a worm might not be 6 feet or 6 yards long, instead of 6 inches. Besides, in most of the rotifers that I have studied—for instance, in *Brachionus*, *Synchæta*, *Pterodina*, *Asplanchna*, *Rhinops*, *Triarthra*, *Polyarthra*, and others—the main muscles, *viz.* those for retracting the disk, are attached to the integuments at their extremities only, and freely traverse the perivisceral space, at a good distance from the integument.

As to the possession of "hollow segmented limbs," there the rotifers have a weakish case; still it has not been left to *Pedalion* alone to fight the battle for his less-gifted brethren. *Synchæta* has two hollow rudimentary limbs, each worked by two long powerful muscles, attached to spots inside of them, as may be seen in my drawings in the fourth volume of this Journal; and by placing these powerfully ciliated limbs at different angles to its body, is able to perform many of its curious antics. *Triarthra* and *Polyarthra*, too, have processes that might pass muster among the Greenwich Pensioners; but I am afraid they do not help the case much: *Polyarthra*, however, has distinct condyles (as Gosse has shown), on which its curious processes work; and *Dinocharis* has equally distinct joints.

Doubtless all general statements in Natural History must be taken with wide allowance; definitions, classifications, systems are, after all, but useful pegs to hang our knowledge on; and it must often be a matter of taste almost, how some odd creature is to be ranked and ticketed. If it is desired to establish its genealogical relation to other animals, then indeed the matter ceases to be one of taste, and becomes one of intense interest, and one which may well give rise to warm controversy; but such discussions I must leave to those very few who have the requisite knowledge and capacity. I would only venture to hope that, as my readers weigh the various

reasons for or against the promotion of the rotifers, they will admit that, when *Pedalion's* six limbs are thrown into the scale, the beam decidedly inclines towards the Arthropoda.

EXPLANATION OF PLATE XXXIII.

FIG. 1.—Side view of the female of *Pedalion mira*, showing the internal organs. The two side limbs have been removed, as well as all the muscles. *a*, buccal funnel. *b*, lips. *c*, gastric glands. *d*, mastax. *e*, stomach. *f*, intestine. *g*, aperture of oviduct. *h*, anus. *k*, loop of depuratory tube, with two vibratile tags close adjoining. *l*, another loop of depuratory tube, with one tag. *m*, nerve mass. *n*, dorsal antenna, with nerve thread. *o*, shoulder antenna, with nerve thread.

FIG. 2.—The same view of the same to show the muscular system: the plumes of the side limbs and all the internal organs removed. *a*, thinned portion of trochal disk. *b*, *b*, *b*, muscles for retracting the disk. *c*, very long ditto, traversing the centre of the body and fastened by two branches to the centre of the trochal disk. *d*, *e*, muscle for opening oviduct. *g*, aperture of oviduct.

For full description and figures of the rest of the muscles, see the October number of the 'Quarterly Microscopical Journal' for 1872.

The figures, however, given in that number, as well as those accompanying this paper, are rather diagrams than drawings. For drawings giving the best representation of *Pedalion's* appearance, see the sixth volume of the 'Monthly Microscopical Journal,' page 121.

II.—*On the Structure and Development of the Crow's Skull.*

By W. K. Parker, F.R.S.

(*Read before the* Royal Microscopical Society, *October* 2, 1872.)

Plates XXXIV., XXXV., and XXXVI.

Introductory Remarks.

My first materials for working out this type of skull were obtained for me by our esteemed member, W. H. Ince, Esq. Other specimens came to me from friends in different parts of the country, no few of these having been laid under contribution for hatched and unhatched chicks of various birds. The Crow is the great subrational chief of the whole kingdom of the Birds; he has the largest brain; the most wit and wisdom; albeit, at times the cruellest of the birds of wing. He, or at least some of the secondary chieftains of his tribe, can speak with man's voice, and, full of drollery and satire, make mockery of all and every of the many voices that are in the world.

Half the known breeds that are, owe allegiance to the kingly Crow: of a full and literal *myriad* of feathered species, a good half are ennobled by Mr. Huxley's title, namely, that high-sounding and yet hoarse name—"Coracomorphæ."* This term — the *Crow-form*—is not to be taken as interchangeable with the name of the Crow family—"Corvidæ"—but as the expression of a sweeping generalization which gathers together from the very ends of the earth all those birds that have the same morphological characters as the Crow. Even birds that are to all intents and purposes *Crows* have to be broken up into many "sub-families,"—there are the *Northern* and the *Southern* Crows; the Starlings, Choughs, Nutcrackers, Birds of Paradise, and a host of others; all Crows, not, strictly speaking, "Corvidæ," but "Sturnidæ," "Paradisæidæ," and the like.

Now the term "Coracomorphæ" may be used to express such a general physiognomy as would make an ordinary observer say of a bird, "That form reminds me of a Crow:" such a remark would not be made concerning a Grouse, a Plover, or a Cormorant.

But there underlies the visible form and fashion and colour of a living bird some modification of its plan of structure which is always the correlate of certain outward characters; and an anatomist familiar with the construction of the various types seldom makes a wrong guess as to what morphological group he will have to refer any new bird, when he has taken a good survey of its outward form. Professor Huxley, in the paper referred to, has seized hold of the great controlling portion of a bird's structure,

* See "On the Classification of Birds," 'Proc. Zool. Soc.,' 1867, pp. 415–472.

namely, its head, and especially its *face:* by the structure of the face, which must have all things else in harmony with it, he has sought to give the birds a true morphological grouping. Such a system of taxonomy has great and evident value; but in the present state of our knowledge the physiognomical groups do not admit of simple superposition on the morphological groups. In some cases they fit accurately; in others they come short in one place and overlap in another; and thus the zoologist has to "drive two-in-hand" through the various ornithological territories; and as the various regions in a geological map cannot be made to correspond with political or with ethnological divisions, so it is in our present attempts at making the two taxonomic modes of *bird-survey* agree.

If the palate of a Fowl be examined, it will be seen to be deeply cleft; if that of a Goose, the cleft will be found to be closed in front: the palate of a *Podargus* (Giant Goatsucker) has its two sides bound together by two bony plates—the hard palate. The cleft in the Fowl and Goose is divided into two, by the "vomer," or ploughshare-bone, which lies at the middle.

In the Fowl neither the upper maxillaries nor the palatines meet at the mid-line below; in the Goose the maxillaries do thus meet and combine; in *Podargus* both the maxillaries and palatines coalesce, as in Man and the other Mammalia.

For the perfectly cloven palate of the Fowl, Professor Huxley has coined the term "Schizognathous"; for the half-closed palate of the Goose, as well as for the well-floored palate of the *Podargus*, the same *Mint* has yielded the term "Desmognathous."

But the *Crow-form* stands in need of another name for its type of palate than either of these; and, as the Common Sparrow well displays this third kind of mouth-roof, our bold group-maker calls it the "Ægithognathous" palate. Now I could have wished that either this bird or the Crow had given both names, that is to say, to the general type and to the palate; then we should have had the Coracomorphous birds with Coracognathous faces, or the Ægithomorphous birds with Ægithognathous faces.

Nevertheless, the two groups are almost at one, in reality, and it does not weigh a feather with me how the thing is put, if so be the student will get his mind clear upon the subject.

The cleft in the Sparrow or Crow's face is stopped-up in front, not by coalescence of the maxillary bones, which are similar to those of a Fowl, but by having a double vomer, which unites at the mid-line in front, where it is broad, emarginate, and winged.

These antero-lateral wings are supplied on each side with an additional bone. The two bones are grafted upon the cartilaginous nasal walls and their long scrolls—the "ali-nasal" or "*anterior* (not *inferior*) turbinals."

This metamorphic fusion of parts, essentially distinct in morphological origin, is very remarkable; and to add to the mystery, the vomerine moieties are each of them formed in a distinct plate of cartilage, which, as a rule, does not exist in other birds.

The "Coracomorphæ"—five thousand known species there are of these—are characterized by this structure, at least as far as I can find, for I have worked out this part in scores of types,* not only of the natives of the Old World, but to an equal or even greater extent in South American, Malaysian, and Australian forms—the feathered denizens of the "Notogæa," or Southern World.

The Swifts ("Cypselus") have this structure as completely as their Passerine relations the Swallows. *Just above* the Ostrich order, the *Hemipods* ("Turnicomorphæ") show this structure on a very large scale, but the bony substance is only strongly tied to the cartilage.

Leaving out the Tinamous, which are merely little Ostriches struggling to become of the winged order, there are certain *climbing* birds which refuse to conform to my friend's Greek rules, "and he whose heart is as the heart of a lion, did utterly melt" in presence of this difficulty; these birds, the Woodpeckers, retaining the strongest marks of any of a reptilian parentage, I beg to call the "Saurognathæ."

Differences of so remarkable a kind existing in the fore-face of the great groups of birds, and correlated with such exquisite specliazations as we find in the Woodpeckers on one hand, and the Singing-birds on the other—to say nothing of other groups, each member of which is a marvel of adaptive modification—such peculiarities as these may well decoy the student into these enchanting zoological territories.

On the Development of the Skull and Face of the Genus Corvus.

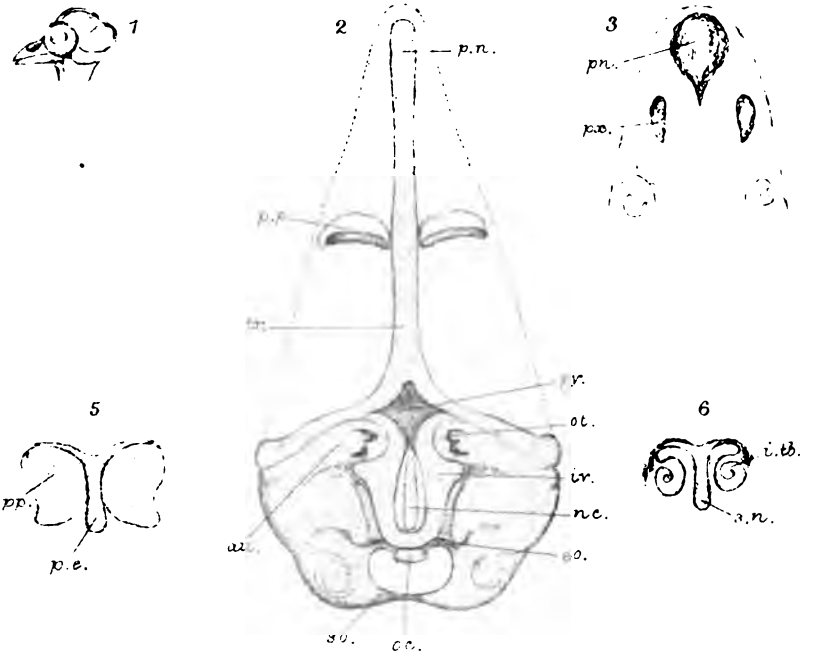
My youngest Crow-embryo is the Hooded Crow (*C. cornix*) at about the sixth day of incubation. The natural size of its head is shown in Plate XXXIV., Fig. 1. A palatal view of the skull (Fig. 2) at this stage shows that the first "præ-oral arch" (*trabeculæ tr.*) has coalesced throughout nearly its entire length, the posterior part (apices of the rods) alone being distinct. These apices diverge round the pituitary body (*py.*), and then coalesce with the "investing mass" (*i. v.*) of the notochord (*n. c.*). The distal ends, besides coalescing by a continuation forwards of the median commissure, have sent forwards an azygous rostrum, the "pre-nasal cartilage"; this single cartilage answers to the double cartilages in which the mandible terminates.

* Through the kindness of my friends, Osbert Salvin, Esq., and Mr. W. J. Williams, Assist. Sec. Zool. Soc.

On each side, in the indifferent tissue that covers this rod *above*, the præ-maxillaries are found, as small oval patches of osteoblasts (Fig. 3, *p. n.*, *p. x.*). The cartilaginous cranium is already well formed, and the auditory sacs (*au.*) with their enclosed otoliths (*ot.*) are well seen in this delicate skull when examined as a transparent object. The pituitary space (*py.*) is still freely continuous with the space in which the notochord (*n. c.*) lies—"the posterior basi-cranial fontanelle"—but the halves of the "investing mass" (*i. v.*) have united on each side with the auditory mass, and also with each other behind; where they have formed the bilobate occipital condyle (*o. c.*). From the investing mass on each side, a little in front of the condyle, there is a crest of cartilage, the "ex-occipital" (*e. o.*), which is growing towards its fellow in the "super-occipital" region (*s. o.*), and is becoming coalesced to the hinder margin of the auditory sac.

A side view of the cranio-facial axis, from the "sella turcica" forwards, is shown in Fig. 4; it takes in the whole trabecular bar. Here we see how large the terminal cartilage is, notwithstanding its temporary character, and also that the trabeculæ have sent upwards a crest of cartilage which is continuous with the keel growing from the cranial *floor* and nasal *roof* from the optic foramen (2) to the commencement of the pre-nasal rostrum (*p. n.*). Not only has continuous metamorphosis of the mother-cells into cartilage made these various morphological parts one, but the roof of the nasal sacs and the floor of the hemispheres have undergone the same change. Between the eye and nose the nasal wall has become a large uniform plate of cartilage; this is the "pars plana" (*p. p.*) or "ecto-ethmoid." A transverse section through this region (Fig. 5) shows the relation of the "ecto-" to the "meso-ethmoid." In another section (Fig. 6) the septum nasi is cut through, and the ali-septal cartilages are seen each to give off an elegant scroll with two turns; this is the equivalent of the human "inferior turbinal," although its position is in *front* of and not *below* the true olfactory region. A section made between the eye-balls (Fig. 7) shows the cartilaginous continuity of the two trabeculæ; their ascending crests, the anterior sphenoidal keel (præ-sphenoid), and the "orbito-sphenoidal laminæ."

Most of the investing bones can now be seen. I have already spoken of the præ-maxillaries, and others will be better shown in an embryo more advanced, but four out of five of the mandibular splints can now be well observed. On the inner side of the "articulo-Meckelian rod" (Fig. 8, *cr.*) the "coronoid" is seen at the top; and the "angulare" (*ang.*) on the inner side, and part of the "surangulare" (*s. ag.*) below. On the outer side (Fig. 9) the whole of the large "surangulare" is shown, as also, much further forwards, the "dentary" (*d.*): the "splenial" (*sp.*) is shown in a more advanced stage.



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This more advanced stage has been obtained in an embryo of the Rook (*C. frugilegus*), two days older, or about the eighth or ninth day of incubation. The basal view of the skull (Plate XXXV., Fig. 1) shows several things not present, and others not figured in the earlier stage. The cartilaginous skull is well formed, and its *first* bony instalment is gained, namely, the "basi-occipital" (*b. o.*). This is formed as a delicate bony investment to the cranial part of the notochord, and then spreads laterally into the halves of the "investing mass" (*i. v.*). The whole occipital cartilage is now well seen with its condyloid foramen (9), and the passages for the "vagus" nerve (8). The condyle (*o. c.*) is more typically ornithic, being nearly hemispherical; the great tympanic alæ also grow widely from the ex-occipital region (*e. o.*), which is not ossified as yet. The super-occipital cartilage (*s. o.*) is a thin commissure uniting the huge ear sacs (*au*). This is still better seen in Fig. 2, where the anterior, horizontal, and posterior semicircular canals have stretched the auditory cartilaginous coat almost to bursting. The "ali-sphenoidal" cartilages grow up *directly* from the pituitary part of the "investing mass" (Fig. 1, *py. iv.*), but the space itself, embraced in front by the *top ends* of the trabeculæ, is membranous below. So much of the "basilar plate," or investing mass, as is not ossified by the notochordal bony sheath still forms a broad floor with a free front edge. The "posterior sphenoid" here borrows—*afterwards*, from the symmetrical ossifications formed in the thick sub-mucous cushion which is stuffed between the fauces and basis-cranii.

These bones, my "basi-temporals," are beautifully shown in this stage (Fig. 1, *b. t.*), as also the grooved bony style which yields hardening material to the fore-part of the basi-sphenoid; this is the "parasphenoid" (*pa. s.*). Further forwards, the cartilaginous base is seen between the palatines (*pa.*), and this becomes cleft, vertically, to form the hinge between the face and skull. The rest of the cartilage is the base of the nasal septum (*s. n.*), and the "præ-nasal rostrum" (*p. n.*). Besides the parasphenoid, the trabecular arch has the præ-maxillaries; these are very much increased in development since the last stage (Figs. 3, 4, 5, *p. x.*). Seen from above (Fig. 4), they already meet, nearly cover the "præ-nasal," and send their nasal processes to the ethmoid, above.

The vomerine splints were not well made out in this preparation, although I obscurely saw the division into two halves; but in a Brown Linnet at this stage, I found them to be formed as "endostoses," in an oval patch of *cartilage*. Above the trabecular region (Fig. 3) the "ali-nasals," "ecto-ethmoid," and "meso-ethmoids," are shown as increasing in size and becoming perfect in form, and between the ethmo-presphenoidal plate (*eth. p. s.*) and the trabecular crest a thin longitudinal cleft has been formed, the

membranous interorbital space (*i. o. s.*). The "nasals" (Figs. 3 and 4, *n.*), the frontals (*f.*), the parietals and squamosals (Figs. 2 and 3, *p. sq.*), are now well demonstrable; but the great "fontanelle" (*fo.*) occupies most of the cranial roof. The second pair of *præ-oral* facial bars are undergoing rapid development; at first delicate crescentic bars of soft mother-cartilage, or indifferent tissue, they are now partly *true* cartilage, but principally bone. Behind (= *above*) the pterygoids fit by a cup to a ball on the fore-face of the pier of the next arch, the quadrate, and then, converging to the mid-line, they send a "meso-pterygoid process" over the upper edge of each palatine. This *process* afterwards becomes a separate bone.

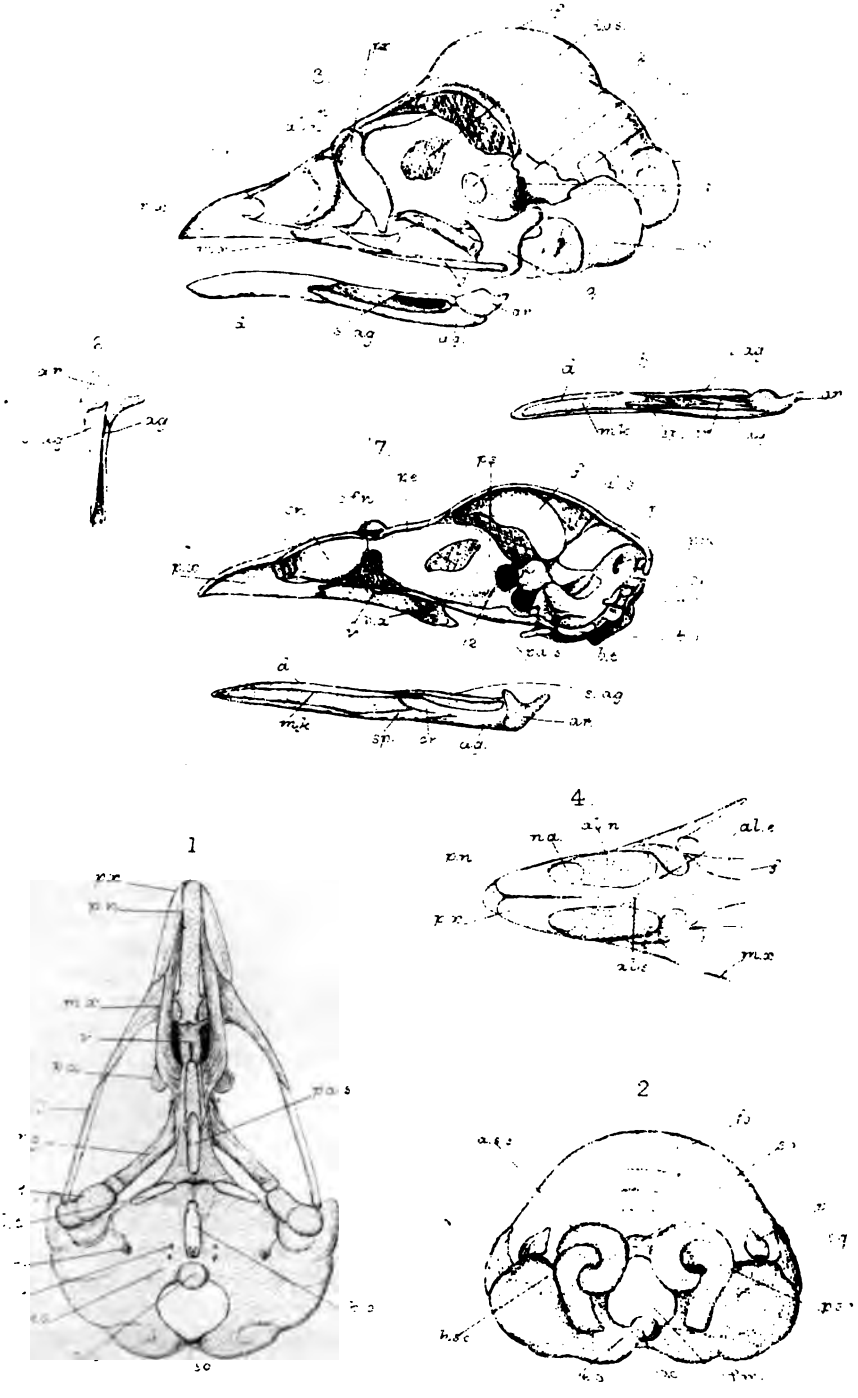
Where the palatines (*pa.*) are bowed out behind, they send outwards a triangular plate, which, not becoming infected by the bony matter of the main bar, develops rapidly into hyaline cartilage, to have, afterwards, its own endosteal centre—the "trans-palatine."

Two splints are seen on each side, belonging to the pterygo-palatine arch. These are the very small *ichthyic* maxillaries (*mx.*), and the slender "jugals" (*j*) which clamp the quadrate without the intervention of a "quadrato-jugal," as in many birds. The retral process of the maxillary (maxillo-palatine of Huxley) can already be seen passing towards the mid-line, *above* the palatines.

The hyoid arch, well developed as to cartilage, but with no bony deposits, was not wholly worked out in this stage. It will be described in the next.

But the mandible is now very instructive. Seen on the inner side (Fig. 5), the dentary (*d.*) is seen to have surrounded all but the inner face of Meckel's cartilage (*mk.*) in front; farther backwards the most splint-like of the splints—the splenial (*sp.*)—is now evident, and the coronoid (*cr.*), the "angular" (*ag.*), and the surangular (*s. ag.*), are growing rapidly; the two latter are shown in relation to the articular cartilage (*ar.*) in Fig. 6. Ossification had commenced in the proximal portion of the rod. The articular region (*ar.*), which, as seen from below (Fig. 6), sends backwards the "posterior angular process," and inwards the "internal angular process." This latter is the otherwise-used symmorph of the "Manubrium Mallei" of Man.

My next stage is supplied by the ripe chick of a Carrion Crow (*C. corone*, Plate XXXV., Fig. 7, and Plate XXXVI., Figs. 1–6). The process of development during the last five or six days of incubation is very rapid, and indeed very rapid is the whole process of growth in these high, noble, hot-blooded birds. The study of their growth and metamorphosis, as compared, not only with that of a Reptile or Fish, but even with that of the young of the slow-growing "Præcoces," suggests the possibility of a compromise between the doctrine of *Development* and that of *Creation*.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From the stored-up wealth of the egg things new and old have been required, and an architectural process has freely applied the *lime* which was ready at hand. This hardening of the walls has given us new bony centres, and has obliterated the distinction between others that were separate. Turning to the basal view, we see that the minute bone-sheath of the notochord has become a recognizable "basi-occipital" (Plate XXXVI., Fig. 1, *b. o.*); the "basi-temporals" (*b. t.*) have become one broad unbent-bow-shaped piece, and the parasphenoid, or "sphenoidal rostrum" (*r. bs.*), has got its *ichthyic* "temporal wings," has grafted itself upon the skull-base, and is being fused with the basi-temporal. The lateral occipital region has now its proper ossifications, the ex-occipitals (*e. o.*); and the upper part of the arch has begun to ossify by another pair of centres, the supra-occipitals (Plate XXXVI., Figs. 4 and 5, *s. o.*). On the roof (Fig. 3, *p.f.*), the parietals and frontals meet at the mid-line, and the nasals (*n.*) and præ-maxillaries (*p. x.*) have begun to acquire their permanent form. In the side view (Fig. 2), the squamosal (*sq.*) is seen flanking the roof-bones; the ali-sphenoid (*al. s.*) has a bilobate osseous centre, evidently by the fusion of two smaller ones. Attached to the large "ecto-ethmoidal" flap, or "pars plana" (*p. p.*), is a small rod-shaped lachrymal (*l.*); in this side view also the fish-like character of the maxillary (*mx.*) is well seen, as also the ectosteal patch on the quadrate (*q.*). Below, the pterygo-palatine arcade (Fig. 1, *pg. pa.*) has developed greatly, and the free cartilaginous "trans-palatine" flap has become typical in form. The mandible (Fig. 2, outer view; and Plate XXXV., Fig. 7, inner view) is now in a characteristic *reptilian* condition, with its one endosteal bone and its five parosteal investments. In the hyoid arch the shaft of the stapes (Fig. 2, *st.*), and the proximal pier of the "cornu major" (Fig. 6, *br.* 1), have each acquired an ectosteal sheath. The small cerato-hyals (*c. h.*), the "basi-uro-hyal" (*b. h., u. h.*), as well as the distal part of the long rod (third post-oral), are still mere cartilage.

A longitudinal section (Plate XXXV., Fig. 7) shows much that is instructive. The exact relation of the roof-bones to the cranio-facial axis; the large ear-capsule, with its prootic (*pro.*) and opisthotic ossifications (*op.*)—the only centres I have found in this type; and the notch and fenestra that re-differentiate the trabeculæ.

The "interorbital fenestra" (*i. o. s.*) is suboval, and somewhat tilted backwards. The cranio-facial cleft (*c. f. c.*) has severed the thick trabecular bar and its crest, and is creeping upwards so as to divide the ethmoid (*eth.*) from the nasal septem (*s. n.*). The vomerine moiety (*v.*) is seen beneath the hinder half of this notch; and beneath it the palatine (*pa.*). The "rostrum," or parasphenoid, nearly touches the basi-temporal (*b. t.*); and partly ossifying the "sella turcica," has behind it the "spheno-occipital synchondrosis."

This is followed by the growing basi-occipital (*b. o.*); this by the condyle (*o. c.*), and this is surmounted by the "ex-" and "super-occipitals" (*e. o.*, *s. o.*).

My next stage is a nestling of the Carrion Crow (*C. corone*), one week old.

The most noteworthy thing in these further illustrations is the great increase of size, every tissue having undergone rapid interstitial growth; but the relative proportion of the various bones and tracts of membrane and cartilage is much the same as at the time of hatching.

Certain morphological changes of great importance have nevertheless occurred, for the "super-occipitals" (Plate XXXVII., Figs. 1–5, *s. o.*) have coalesced at the middle, and the "meso-ethmoid" (Figs. 2 and 4, *p. e.*) has acquired a bony centre.

The lachrymal (*l.*) is larger, is creeping up the fore-edge of the ecto-ethmoid (Fig. 2): the "articulare" (Figs. 2 and 6, *ar.*) now embraces the internal angular process. The elements of the posterior sphenoid, and of the auditory capsule, all these are becoming more closely related to each other.

The hyoid cartilages (Fig. 7) are only larger, altogether, than in the ripe embryo; the elements of the mandible (Figs. 2 and 6) are now closely similar to what is seen in the adult Lizard or Crocodile.

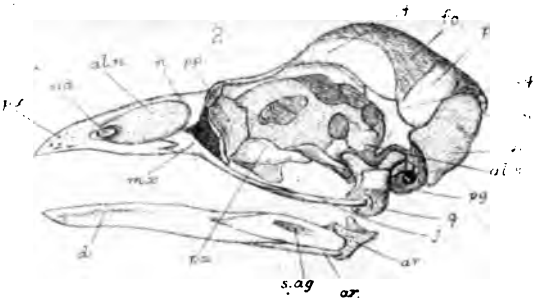
The fifth stage is that of the young Rook (*C. frugilegus*), ready to fly. It is replete with interest.

The basal and lateral elements of the occiput, &c. (Plate XXXVIII., Figs. 1, 4, *e. o.*, *b. o.*) have coalesced; but the upper piece (*s. o.*) still forms an elegant, distinct keystone above (Plate XXXIX., Figs. 1 and 2). The basi-temporals have coalesced with the overlying "para-basisphenoidal" ossification, and at their outer angle the internal carotids (Fig. 1, *b. t.*, *i. c.*) are seen entering.

The fore-part of the "sphenoidal rostrum" (*r. b. s.*) is embraced by the forks of the vomer (*v.*), and its sides by the "meso-pterygoid process" of the pterygoid, and by the "post-palatine processes." The "trans-palatine" (*t. pa.*) is still solid cartilage, without bony deposit; it is a large and well-defined morphological element.

The grooved upper surface of the vomer receives the thick part of the base of the septum-nasi; this base is then two-winged, and these wings are a part of each trabeculæ, unaltered in shape although coalesced together. In front of this part is the shrinking "præ-nasal rostrum" (*p. n.*) which lies between the recurrent ali-nasal folds.

The ali-nasal floor (*al. n.*), over which lies a "turbinal," is seen on each side of the base of the nasal septum; behind, it is continuous with the vomer on each side, the moieties of which are grafted upon the cartilage.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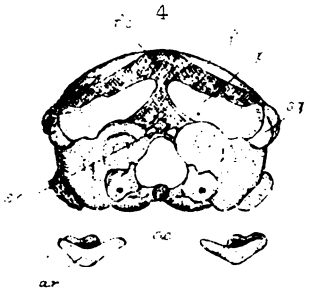
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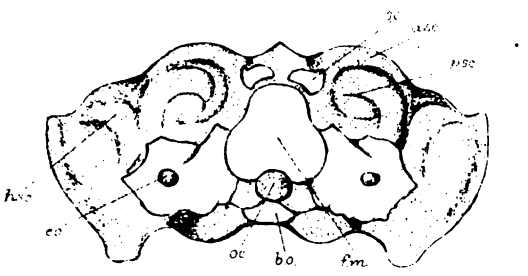
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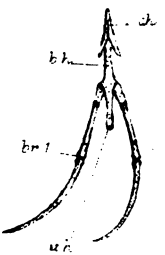
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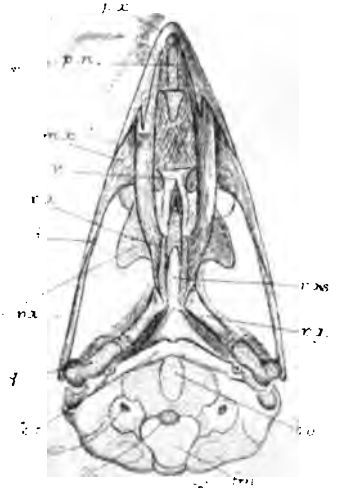
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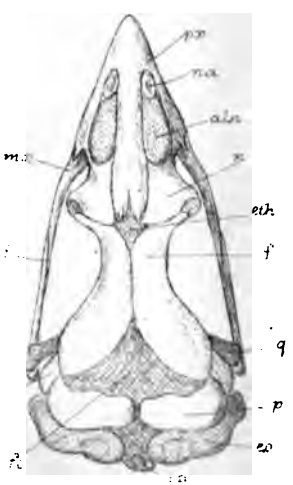

3

The large coalesced præ-maxillaries (*pa.*) are seen overlapping the maxillaries (Plate XXXVIII., Fig. 1, *mx.*), and the retral process of these latter bones has now acquired its elegant ear-shape. Seen from above (Plate XXXVIII., Fig. 2), the cranial roof is well filled in; a little of the ethmoid (*eth.*) is seen, and two remnants of the great fontanelle (*fo.*); and the top of the "ecto-ethmoid" (*e. eth.*) projects on each side; afterwards it gets its own osseous centre, distinct from that of the "meso-ethmoid" (Figs. 2 and 4, *pe.*), and from that of the "pars plana" (*p. p.*). This latter part is still unossified (see also Fig. 2). The lachrymal (*l.*) now occupies nearly all its anterior edge. The "meso-ethmoidal" bone (*p. e.*) nearly reaches the notch (Fig. 4, *c. f. c.*) in front, and the interorbital fenestra (*i. o. s.*) behind. The septum-nasi is still soft, as also all the anterior sphenoidal region, which has its orbito-sphenoidal lips very small. In more advanced young I find a distinct præ-sphenoid perched above and between the huge middle ethmoid and basi-sphenoid, but no orbito-sphenoids; the orbital plates of the frontals do duty for them in these high, perching types. The auditory capsule (Fig. 4) has coalesced with the ex-occipital (*e. o.*) and its own opisthotic (*op.*) and prootic (*pro.*) pieces are no longer distinct; the ali-sphenoid (*al. s.*) is now well ossified; the knob of cartilage projecting from it in the post-frontal region is part of the ear-sac—the "sphenotic."

The double nasal sac seen from above (Plate XXXIX., Fig. 3) shows the ali-nasal, ali-septal, and ali-ethmoidal regions (*al. n.*, *al. s.*, *al. e.*); in the latter is seen the broad top of the "meso-ethmoid" (*p. e.*). This part ends as a blunt spur, above the "orbito-presphenoid" (*o. s.*). The transverse sections are through the middle ethmoid, and behind the lateral ethmoids in Fig. 5; and through the unossified part of the middle ethmoid in Fig. 4. This latter section is through the upper and lower turbinals (*u. tb.*, *l. tb.*); the "middle turbinal" is a mere rudiment on the front of the "pars plana," or antorbital. This same section is through the nasals (*n.*), nasal processes of the præ-maxillaries (*p. x.*), the maxillaries and their maxillo-palatine processes (*mx.*), the forks of the vomer (*v.*), and the palatines (*pa.*). A longitudinal section outside the septum (Fig. 6) shows the ali-nasal turbinal (*a. n. t.*), the inferior turbinal (*l. tb.*), and the upper turbinal (*u. tb.*).

The mandible (Figs. 2 and 4) shows the "articulare" much more ossified, the investing elements much more developed; Meckel's cartilage (*mk.*) nearly hidden; and the coronoid (Fig. 4, *cr.*) ankylosed to the "articulare."

The "os hyoides" (Plate XXXIX., Fig. 7) shows an osseous tract in each "cerato-hyal" (*c. h.*), and both the pieces of the third post-oral (*br.* 1) have thin osseous shafts. The azygous piece (*bh.*, *uh.*) is still soft.

After another week or two the pointed fore-end of the pterygoid becomes detached as a distinct "meso-pterygoid," which after a few weeks coalesces with the palatine. The "trans-palatine" cartilage gets endostosis, completely ossifies, and then ankyloses with the palatine, so also do the vomerine forks; and at their fore-end, on each side, a new bone appears—the "septo-maxillary."

The septum-nasi ossifies in some degree, but most of the nasal labyrinth is permanently cartilaginous, yet the ecto- and meso-ethmoidal elements are well ossified and quite ankylosed. Almost every part of the skull and face undergoes this intensive and extensive ossification, but the quadrate bones and pterygoids keep distinct from their surroundings, and the palatines from each other behind. In their "ethmo-palatine" regions they are tied together by the vomer which coalesces by its forks; in front the palatines are fused with the rest of the face. The twisted, fusiform lachrymals are permanently distinct, and a trace or two of sutures can be seen over the "cranio-facial hinge." All the *twelve* pieces of the lower jaw become one V-shaped bone.

In the second post-oral the medio-stapedial is ossified, and all but the tips of the distal cerato-hyals; the upper ends of the third arch, and the greater part of the "uro-hyal" remain soft.

In conclusion, if anyone shall ask me why this multitude of bones should so soon melt into so few? I have only one answer, namely, that the reason lies, I suppose, first, in their genetic history in the "auld lang syne," and then in slow adaptive modifications during epoch after epoch.

DESCRIPTION OF PLATES.

PLATE XXXIV.—Development of skull in Hooded Crow (*Corvus cornix*), fifth or sixth day of incubation.

PLATE XXXV.—Figs. 1–6. Skull of Rook (*Corvus frugilegus*), eighth or ninth day of incubation. Fig. 7. Skull of Carrion Crow (*Corvus corone*), end of incubation.

PLATE XXXVI.—Skull of *Corvus corone*, end of incubation—continued.

[It is necessary to state that only three of Mr. Parker's Plates—Plates XXXIV., XXXV., and XXXVI.—appear in this number of the Journal. The remainder will be given in the succeeding number.—ED. 'M. M. J.']

III.—*Remarks on the Resolution of the Nineteenth Band of Nobert's Plate by certain Objectives, especially by a new Tolles's Immersion $\frac{1}{18}$th.* By Dr. J. J. Woodward, U. S. Army.

(*Taken as read before the* Royal Microscopical Society.)

In the 'Monthly Microscopical Journal' for September, 1871 (p. 150), I published a short note on the resolution of *Amphipleura pellucida* by a Tolles's immersion $\frac{1}{8}$th. In a subsequent note dated November 21, 1871, and published by the same Journal in January, 1872 (p. 27), I mentioned that I had engaged Mr. Tolles to construct a higher power objective for the Museum, which had not yet been received, and remarked, "should it fulfil Mr. Tolles's expectations it will give me as much sincere pleasure as it can to him or any of his friends, and I will promptly make the facts public." The objective finally supplied in accordance with this order was put into my hands in August of this year. Mr. Tolles, however, informs me that the optical combination was completed and tested by him in May. This objective is so surpassingly excellent on the plate, that I hasten to make the facts public, as promised.

The objective is made to work wet or dry at pleasure. Wet, it is marked by the maker $\frac{1}{18}$th. The dry front at fifty inches distance from micrometer to screen magnifies 800 diameters uncovered, 900 covered. Angle of aperture 125° uncovered, 155° covered. The wet front can also be used dry for the first half turn of its screw-collar from the extreme open point, the range of powers and angles being very nearly the same as with the dry front. The next half turn of the screw-collar is intended for work wet, the maximum angle as measured in air being above 170°. An arrow on the tube of the objective, and a zero on the screw-collar, indicate the limit proposed for uncovered wet. At fifty inches distance the powers are 900 diameters when the collar is brought to the zero point, 1050 diameters when completely closed. On trial, however, I could not satisfy myself that the zero point was accurately marked. I first noticed that on several slides of diatoms, mounted with extremely thin covers, the best definition wet was obtained when the objective was opened to the zero point, or even yet farther; then testing the objective wet on a broken Nobert's plate, mounted with the lines uppermost and uncovered, I found that the bands were best seen when the correction was one-sixth of a whole turn more open than the zero point marked. At this correction the magnifying power at fifty inches distance was 845 diameters. At ten inches by lamplight it was 170 diameters, the best correction on the naked plate by lamp being sensibly the same as by sunlight, or if anything a trifle more open. I made the latter determinations with the utmost care for the benefit of those who think the several fronts of

objectives should be named by their performance uncovered measured by lamplight at ten inches, and who attach an extreme importance to these conditions, which I confess I do not appreciate. Nor do I attach any blame to the distinguished manufacturer by whom this objective was constructed for the trifling deviation from his intended standard; and I may add that having published the above facts, I consider my duty in the matter discharged, and whether others choose on the above data to call the objective $\frac{1}{17}$th or $\frac{1}{18}$th, I shall continue to do as I have done in similar cases, namely, pending the adoption of a rational system of nomenclature, I shall continue to designate it by the name given by its maker, and having published its actual magnifying power under definite conditions, shall confine myself to a description of the work it will do.

With regard to the performance of the dry combinations of this objective, I will merely say that they gave me the striæ of *Amphipleura pellucida* rather better than any dry objective I have ever tried. I have not tested them as yet to any extent on other objects, because of the manifest superiority of the immersion work.

The behaviour of the objective, when used wet, is certainly admirable. In illustration I forward a new series of views of Nobert's plate from the lowest to the highest bands taken by it. These pictures, which certainly excel all my former work on the plate, were purposely taken from the ordinary thick-bottomed plate, accessible to most microscopists, in order that those interested might compare what they can see by their objectives with the work now submitted. Most of the negatives were made with a power of about 1200 diameters; but as paper prints of the 19th band, with this power, are so indistinct as to be practically valueless, I have added one of the 19th band from a negative taken with about 1800 diameters, one of the 15th, for comparison, taken at the same distance, with the same cover correction; and lastly, an enlargement of the 19th band picture, which I hope will serve to illustrate the subject of spurious lines on the edges of the band, as well as to display the fine resolution of the real lines obtained by the objective employed.

It will of course be expected that I should say something of the comparative merits of this new objective of Mr. Tolles, and the immersion front of the Powell and Lealand $\frac{1}{18}$th, which has done so much good work for me since 1869.

Certainly I must give the new Tolles's objective the preference on the plate, and on *Amphipleura pellucida*, both by sun and lamplight. On other tests I have not as yet done enough work to particularize. As to magnifying power, it will be remembered I have already published the fact that the wet front of the Powell and Lealand's $\frac{1}{18}$th, belonging to the Museum, magnifies at its

uncovered point 900, at covered 1100 diameters, at fifty inches from micrometer to screen.

Yet these figures would not give the true relations between the magnifying powers of the two objectives as actually used; for that of Mr. Tolles will work through thicker covers than that of Powell and Lealand, and for any given cover works nearer the open point, so that its magnifying power on covered objects is less than might be anticipated from a comparison of the open points of the two objectives.

I have next to state that, Mr. Tolles having been kind enough to send me during the past year a number of objectives ranging from $\frac{1}{10}$th to $\frac{1}{20}$th for study, I have since February of the present year satisfied myself that several of them resolved the 19th band of the plate in a very satisfactory manner. These objectives differed from each other in many particulars, not merely in construction, as to which I know nothing, but in performance. My examination of these objectives has satisfied me that a high angle, with its accompanying advantage of separating power, can be used with less detriment to working distance, penetration, flatness of field, and other advantages usually supposed to pertain to low angle, than has hitherto been supposed, provided the glass is left much under-corrected for colour.

I observed that those of these glasses which were quite under-corrected as to colour, not merely gave the best photographs, but did the best work by lamplight. This result I may say corresponds with what I have been able to observe also with certain objectives of Powell and Lealand, Hartnack, and Gundlach. I do not claim this observation as a new one, for I have reason to believe that the fact has long been well understood in various quarters, but if I am not misinformed the average purchaser of objectives demands approximate achromatism above all else. Now, in view of the irrationality of dispersion, absolute achromatism is impossible, and in aiming to approach it as closely as may be the corrections for spherical aberration are inevitably sacrificed. It appears important that this fact should be more generally known, and I have devised the following simple test, which may serve to illustrate the subject:—

If the microscope be placed horizontally on the window-sill of a dark room, and illuminated by a solar mirror or heliostat, placing a plano-convex lens of about two inches in diameter and ten inches focal length so far behind the achromatic condenser that the rays cross in the principal focus just before they reach the bottom lens of the condenser, it will be found possible with any glass of $\frac{1}{10}$th of an inch focal length or less, combined with suitable eye-piece and distance, to project images on a white cardboard screen with powers of from 1000 to 10,000 diameters.

Now let the test object be *Pleurosigma formosum*. It will be found quite impossible to get a distinct view of the beads with any considerable power, say 3000 to 5000 diameter or upwards, unless the colour corrections of the lens are such that the beads appear of brilliant red on a greenish ground, that is, unless the objective is considerably under-corrected. When this is the case good photographs can be made after the interpolation of an ammonio-sulphate cell, but when the glass is more nearly achromatic not merely the photographs are unsatisfactory, but with white light it will be found impossible to separate the beads distinctly on the screen.

I do not wish to lengthen unnecessarily this already long article; but so much has been said of late with regard to "the advancing powers of microscopic definition" that a few further facts and considerations appear to be demanded.

I note, first, that certain Continental makers would appear to have worked out the problem of high-power definition to about the conclusions indicated above. I have had recently in my hands a new No. 10 immersion by Hartnack, the property of Dr. A. Litton, of St. Louis, Missouri, which handsomely resolved the 19th band, and gave on *Pleurosigma formosum* the colour indications commended above. This objective measured at fifty inches 700 diameters uncovered, 770 covered.

I have, moreover, recently examined a new Gundlach No. 8, the property of Mr. W. H. Walmsley, of Philadelphia, which also resolved the 19th band in a satisfactory manner. This objective measured at twelve inches distance 200 uncovered, 220 covered. Tested on *Pleurosigma formosum* the colour corrections were almost identical with those of the Hartnack objective.

I may add to the above, that during the past six months, four objectives by Powell and Lealand marked $\frac{1}{16}$th have passed through my hands. Three of these were at the time the property of a well-known importer of optical apparatus, the fourth had been made to order for a distinguished microscopist in New York. All these glasses compared favourably with the Museum $\frac{1}{16}$th, though there were some small differences in magnifying power. All had the same colour corrections. I saw, however, in none of them any superiority over the objective furnished me in 1869.

It seems probable to me therefore that the distinguished makers last named have made no substantial progress since 1869, and this view is confirmed by Dr. Royston-Pigott, who mentions in this Journal, February, 1872 (p. 66), that Powell and Lealand had placed at his disposal "for a fortnight" a $\frac{1}{16}$th "similar, though *perhaps slightly* superior, to the celebrated immersion $\frac{1}{16}$th signalized by Dr. Woodward," and which he says the makers were "unwilling to dispose of," on account of its excellence.

It would appear therefore that the recent advancing powers of

microscopic definition have, as a rule, since 1869 at least, been rather shown in the advance of various makers to the highest standard then attained, than in any actual progress beyond what had then been done. This qualified advance should, however, be most satisfactory to microscopists, since it promises a competition likely to bring down the present extravagant prices of good objectives.

But beyond this general movement, which is so creditable to all concerned, I must believe that Mr. Tolles* has been so fortunate as to make a real advance, and I shall continue to hold this belief until some one can produce an objective which, in my hands, or those of some other observer, shall succeed in doing better work on the plate than that herewith submitted.

In conclusion, I mention that the price paid Mr. Charles Stodder for the objective above described was one hundred and seventy-five dollars U. S. currency.

IV.—*Aperture of Object-glasses.* By F. H. Wenham.

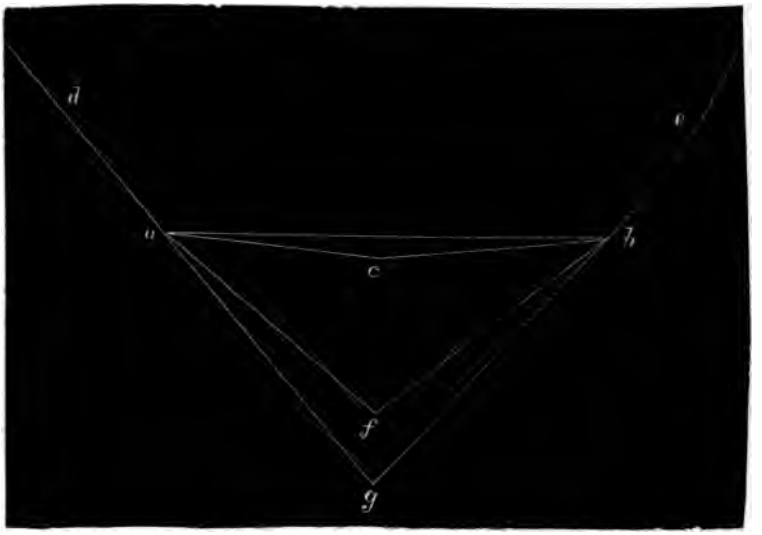
In declining to continue a discussion directly with Mr. Tolles on the question of angle of immersed objectives, I meant no discourtesy, fully appreciating the temperate manner in which he has endeavoured to maintain his point. My withdrawal was in consequence of his diagrams,† in which rays were drawn through lenses in a direction so adverse to my belief in optical theory, and against plain rudimentary principles that I never could abandon, further argument therefore became useless.

But as I agree with the remark in the last Journal, that the question of aperture has not been "quite satisfactorily determined," I offer some further information. The vexed question has been extended over such a long period, and scattered over so many pages, that the main point at issue from the first announcement of the fabulous increase of aperture of immersion lenses has perhaps faded from memory. I therefore condense the principle in a brief demonstration. Let *a*, *b*, represent the maximum working diameter, or actual surface of the front lens, that is, sufficient for the passage of a pencil of rays of 170° from the focal point *c*. By the law of refraction these rays will take the direction *d*, *e*, in the body of the front lens, completely filling the nearly hemispherical back surface to its edge. Now let the front lens be immersed in water. This of course lessens the refraction at *a*, *b*. The focus is extended to *f*,

* These words—"that Mr. Tolles"—are omitted from the MS., but we presume they were intended to have been used by the author.

† Page 117, 'M. M. J.,' March, 1872.

and the angle diminished to 98°. Still the rays after the first refraction take the same direction, *d*, *e*.



Instead of water, let us immerse the lens in Canada balsam. The index of this being taken as the glass of front, *no refraction* will occur at the first surface, but the rays will proceed straight in the direction *d*, *e*, extending the focus to *g*, and reducing the aperture near to 80°. In this demonstration it is needless to complicate the question by carrying the rays through the entire combination, as we may take them at their final emergence at the longest conjugate focus possible, or just within parallelism. *d*, *e*, *c*, must in that case be fixed in direction and position for the dry lens, *d*, *e*, *f*, for the water lens, and *d*, *e*, *g*, for the balsam lens.

If we *decrease* any of the focal distances, *c*, *f*, *g*, from the front surface, so as to increase these angles, the conjugate focus must become extended correspondingly; and having already taken the extreme length, the posterior rays would no longer be parallel, but *divergent*. The effect in this case would be negative, and no image be formed in the eye-piece. Now, with a shorter conjugate focus at the back, perfect images are obtained; but the points *c*, *f*, *g*, would be more distant, and less aperture the certain consequence. We now come to the question of the immersion *versus* the dry principle. This simply means that a film of air shall intervene, either at the surface *a*, *b*, or elsewhere in the cone of rays through the medium. For the sake of simplicity, we will take this film as one of infinitesimal thinness—a case of cover contact in fact. Now, when the

refractive index of films of air, that of water, and again of balsam or glass, are dragged in as an attempt at a would-be scientific demonstration, I denounce it as a muddle and downright nonsense. Thin parallel plates of either of these intervening in the cone of rays will in no way alter the angle, as they always emerge in their original direction. A degree of lateral displacement according to the thickness is the only result; and taking, as I have here done, the constantly occurring case of an infinitesimally thin film of air (as when the object-glass is in contact with the cover), neither calculation nor diagram will show a perceptible difference in the angle, which is the *same* whether the air is there or not, wet or dry.

I may put forth this statement without being deemed arrogant by those conversant with optics. The ground is safe, and anyone that ignores or denies such a definite principle must expect to forfeit all respect for his optical knowledge.

Let us now proceed to the indefinite, wherein the best of us are still at fault. I allude to the present unsatisfactory mode of measuring the angle of aperture of object-glasses by means of the well-known "sector." For anything beyond 90° this is by no means trustworthy. The microscope body with the objective is swung round from a pin near the focal point as a centre, and the measure of the angle of the sweep is indicated by the light from a flame some few feet off bisecting the field of view in the eye-piece. With large apertures a definite bisection cannot be obtained, and the limit has to be guessed at by the position at which the light seems to vanish. A border of diffused light frequently exists, and is thus often mistaken for aperture. Under the most favourable circumstances this mode of measuring extreme apertures is so faulty that when statements are brought forth such as 172° "*and a half*," I cannot help smiling at its absurdity, as a mere piece of brag. This inaccuracy has long been known, and attempts have been made from time to time to provide a remedy. I consider that the most correct of all is that proposed by Professor Robinson, which consists in passing the parallel rays of the sun through the back of the objective, and then by means of a white screen in a dark room or camera intercepting the rays as a disk of light. The angle taken from the diameter of this to the focal point will give the true aperture. Any extraneous or diffused light is either dispersed, or can be detected from its different appearance on the screen. The reason why this plan is not generally used is on account of the trouble attendant upon the arrangement.

Effective aperture of course means the production of a *distinct image* up to the extreme marginal ray. If this image at last becomes so distorted as scarcely to merit the name (such is frequently the case), it may as well be stopped altogether, as the object-glass will perform better with a less angle.

Professor Govin has proposed to associate the measureme
the angle of aperture with the simultaneous view of an o
distinctly defined, like the flames of two candles placed asunde
two white strips separated on a black screen, to the limit of dist
visibility; the angle from these two points to the focus of
object-glass will represent the aperture. The microscope is t
converted into a kind of telescope, by means of a pair of lens
over the eye-piece similar to what is known as Ross's "examini
glass." Unfortunately for the success of this plan, different optic
combinations at the eye-piece give different results by elongatin
or shortening the conjugate focus, otherwise the measurement
would be very satisfactory. I am in hopes of finding a remedy fo
this. Proposals have also been made for testing aperture by
placing a lamp-flame at the back of the object-glass, which remains
a fixture, and rotating a compound microscope round the coincident
foci as a centre, and measuring the arc traversed. This is exceed-
ingly inaccurate, as the aperture of the rotating glass is superadded
to the other. I am, however, constantly in the habit of using this
plan for testing the perfection of the image formed by the oblique
pencils, in order to ascertain whether they are good enough to remain
or should be stopped. When the aperture of the object-glass is very
large, and the marginal pencils free from error, a singular optical
illusion is produced. On looking towards the front of the object-glass
under test, in a line with its surface, a distinct image of the flame
will appear to stand out suspended in air; this appearance is caused
by the degree of aperture or the oblique pencils of the examining
microscope forming an image where none should apparently be
seen. I offer these remarks as a caution that whether apertures
are tested either in air or water, errors are liable to creep in. At
present I have no remedy to suggest, as the investigation remains
to be completed.

Since writing the above, I have to announce that Mr. Tolles'
object-glass has been placed in the hands of Dr. Lawson. I shall
not, however, test the aperture in either water or balsam, till the
return of Professor Markoe from the Continent, in order that he
may be present, by which time everything will be in readiness, as I
perceive that a special contrivance will have to be adapted for the
aperture in balsam, to avoid drowning the glass, or prevent it
from getting into the fittings, as I should not like to take the risk
and trouble of cleaning it out again. This may be avoided by
confining the balsam to a small spot on the front, leaving the sides
intact.

PROGRESS OF MICROSCOPICAL SCIENCE.

ture of Connective Tissue and Tendon.—A paper contained in n-Séquard's 'Archives de Physiologie' for 1869, by M. Ranvier, ed to be the starting-point for a new series of researches on cture of connective tissue and tendons, which has recently en up by M. Boll and Dr. Mitchell Bruce, and which has led ription of this tissue materially different from that contained in books of some twenty years ago, as well as from the more modern s of Virchow. The latter observer maintained that connective as originally composed of cells, the walls of which became ed and condensed, forming the intercellular substance, and ely underwent fibrillation. The central portion both of the d of the processes given off from them he believed remained and by their intercommunication constituted a canalicular ministering to the nutrition of the tissue. The researches of nghausen on the cornea to some extent effected a modification chow's views, by showing that the true cells of that membrane t form, but were contained within, the stellate cavities and through which they moved by virtue of their amœboid activity. esearches of Kühne on the intermuscular connective tissue of og showed that in this animal the cells of that tissue are destitute cell wall, and are not contained in canals, but wander freely gh the meshes of the connective tissue. It followed that, in the at least, the general idea of a plasmatic system, afforded by the y of the cornea after impregnation with nitrate of silver, could be extended to the connective tissue generally. Ranvier com- nced by examining the tendons in the tail of young rats, which, r removal from the body, were stretched on the slide, kept in position a drop of sealing-wax, stained with carmine, and acted on with lute acetic acid. On examination with a magnifying power of 100 ameters, the tissue was found to present parallel continuous red riæ, which, under higher powers, proved to be cylindrical tubes; and hese were seen to be crossed by transverse lines, dividing them into early equal segments. On slight pressure being exercised, each of hese little segments or cylinders split longitudinally, and unrolling itself, proved to be a square cell-plate or lamina, with a nucleus near its centre. Around the cell tubes is an elastic tube or sheath. The tendons are thus, according to Ranvier, permeated by a system of parallel tubes, the walls of which are composed of square cells rolled round till their edges meet, and placed end to end. The stellate figures insisted on so strongly by Virchow as representing corpuscles, he regards as being merely sections of the superficial layer of connective tissue investing the several fasciculi of the tendon, and in these septa his cellular tubes run. The stellate figures brought into view by staining in the subcutaneous connective tissue he also regards as purely illusory; and endeavours to prove it by producing artificial œdema with injection of various fluids into the living animal, and

then freezing it before making sections. A somewh
has been lately advanced by Boll. Boll describes t
nective tissue as granular, elastic, quadrilateral pl
arranged in rows and bend around the fasciculi of the
plate has a median stripe, which he calls the elastic
caused by a thickening of the cell substance parallel t
of the tendon, and is stained deeply with carmine. It
a row of cell plates will give a corresponding row of
that is, a continuous stripe; and this elastic stripe
straight or wavy appearance, varying with the condition
of the tendon. Dr. Mitchell Bruce followed Ranvier's
gives a different interpretation of the appearances present
ing the views of Boll on the relation of the cells to
bundles, he describes fine lines crossing the fasciculi a
them into quadrilateral spaces, in the centre of which a nu
into view on the addition of acetic acid; whilst, after a
longed influence of this reagent, the fasciculi become co
swollen and transparent, and the quadrilateral plates present
ragged borders. With nitrate of silver staining, an inve
cells, beneath which a system of serous canals appeared
places, is brought into view. The cells are rendered still n
tinct by the chloride of gold method, or by a combinatio
silver and gold methods. His view is that the cell element
young tendon are plates so arranged as to bend round the
bundles, and that they consist of granular protoplasm enclo
nucleus. The cell plates are of nearly uniform size, and by
union form a connected whole, which may be aptly likened to a
composed of a number of stripes; and the fibril bundles a
enveloped in this cloth that each is surrounded to the extent of
its circumference by one stripe. As we understand it, the contin
expansion pervading the tissue formed by the cells resembles a si
of corrugated iron. If two such sheets were superimposed on
another longitudinal spaces would remain, and these in the tendon a
occupied by the tendinous fasciculi.—*The Lancet*, September 7.

The Degenerative Process in Nerves.—The last part of Virchow
'Archiv' contains a paper by Dr. Benecke, of Königsberg, under th
title of "Histological Processes in Divided Nerves." His investi-
gations, he says, have extended over many years; and he has experi-
mented upon cats, rabbits, crows, fowls, pigeons, and various small
birds. Of all these he finds young cats the best adapted for research,
as the nerve trunks in those animals contain very little connective
tissue in their interior. The mode of operating consisted either in
dividing the nerve with the knife, or in applying a fine silk ligature
for a few moments, which is almost equally efficacious in cutting
through the axis cylinders. The changes induced were examined at
various intervals of time, ranging from two hours to six months, and
both in the fresh state and after maceration in different preserving
fluids. The first changes observed, and these take place within a few
hours after section, are that the two extremities of the nerve adjoining
the cut become of a greyish or yellowish-red colour, cloudy aspect,

and more or less strongly swollen. These changes are due, says the 'Lancet,' Sept. 21, 1872, to congestion of the severed blood-vessels, and to the imbibition of the serous transudate. In a short time the part of the centric extremity lying near the cut and the whole peripheric part assumes a cloudy-yellow tint, becomes somewhat attenuated, and tears easily. The sheath of Schwann undergoes, at first at least, very little alteration; the medullary sheath, on the other hand, coagulates, and breaks down into a finely granular detritus. The axis cylinder early appears enlarged and swollen, but soon attenuates, and ultimately disappears entirely. In the later stages of the process of disintegration it is impossible by any means to bring it into view. Benecke has never been able to find that the degenerative process progresses in an orderly manner in either direction from the lesion; but believes that it takes place *coincidently* throughout the whole length of the peripheral extremity, and in the immediately adjoining portion of the centric end. But even whilst disintegration is advancing, the process of regeneration has commenced, in the form of multiplication by fission of the nuclei of the neurilemma, or sheath of Schwann. These soon become converted into elongated fusiform bodies, and after a time form the only contents of the empty and collapsed sheaths, which then, he thinks, disappear. The protoplasmic processes of the new elongated nuclei now undergo fusion, and unite to form pale slender bands coincidently in the cicatrix and in the peripheral end of the divided nerve, which is thus brought into organic connection with the central stump. In the further progress of the regenerative process a medullary sheath is gradually formed around the primitive bands or cylinder axes originating from the nuclei. These last no longer retain their ordinary appearance, but become homogeneous, a few only remaining in each fibre, from which the normal sheaths of Schwann are afterwards developed. Upon the whole, therefore, it would appear that after section the proper nervous tissue of the entire peripherical portion of the nerve undergoes disintegration and disappears, whilst regeneration is effected by a process precisely analogous to that which many observers have demonstrated to occur in the natural development of the nerves in the tail of the tadpole and in the embryo of the fowl; the cylinder axis being first formed by the coalescence of nuclei, to which the white or medullary substance and the sheath of Schwann are subsequent additions.

Muscle as a Constituent of Nerve Tissue.—Signor Tigri has shown the presence of muscular fibres among the nerve fibres of the cerebro-spinal nerves and the ganglia of the sympathetic.* When an irritation is applied, contraction is caused, and the liquid contents of the nerve fibres are caused to vibrate, and the motion is conveyed thus centripetally or centrifugally. In reference to these anatomical and physiological peculiarities, he calls the ganglia of the sympatheticus magnus, as well as the large ganglia of the brain, "nerve hearts." He considers that the so-called Remak's nerve fibres are muscular fibres.

* 'Gaz. Med. Ital.'

Grey Substance of the Cortex of the Cerebrum.—The structure of this substance has been quite lately investigated by Prof. Gerlach.* He applied the gold method as a hardening means, and found—1. That besides the well-known white medullated nerve fibres running from the white into the grey substance, and which are arranged in fasciculi radiating towards the periphery, there are also numerous horizontally-running, medullated fibres, which communicate both with one another and with the radially-disposed fibres, forming a coarse plexus visible even with a power of sixty diameters. 2. That in the meshes of this plexus are contained the ganglion cells, and a second far finer plexus of extremely delicate non-medullated fibres which can only be brought into view with the highest powers of the immersion system. This second plexus is formed of the finest protoplasmatic processes of the nerve cells, whilst the medullated fibres appear in part to take their origin in it, the individual fibres becoming surrounded by medulla. 3. Some of the protoplasmatic processes of the ganglion cells develop directly into medullated fibres without branching, and thus such fibres originate partly from the cells themselves, and partly from a plexus of non-medullated fibres.

Brain Tissue in General Paralysis.—The 'British Medical Journal' states that at the meeting of the British Medical Association, Dr. J. Lockhart Clarke exhibited microscopical sections of brain tissue illustrating general paralysis; and he explained that he had found the conditions of brain tissue to be in the first place that of wasting, then a widening of the perivascular canals, and a separation of the sheath of the same. This sheath (apparently hyaloid) becomes corrugated and contracts round the vessel, which is seen to run within it in a tortuous course, the tortuosity often being so great that the vessel becomes twisted into knots or kinks. Dr. Clarke further explained how he found the white tissue disintegrated, and the grey matter wasted; the grey cells lost their sharp contour; and their granular contents, as a little heap, ready to fall asunder.

Whence Come Fat-cells?—This interesting subject has been lately taken up abroad, and a very interesting summary of the result is given in the 'Lancet' of August 17. The question of the origin of the fat present in every well-nourished person is not easily answered. Putting aside the debated point of the direct conversion of the carbohydrates into fat, there are three theories that have obtained a certain amount of credit. The first—resting on the fact that, with free supply of oleaginous food, an abundant deposit of fat takes place in the subcutaneous connective tissue, in the omentum around the kidneys, and elsewhere—maintains that the fat enters the economy in the ordinary manner, and is directly stored up in the interior of the fat-cells. This view entirely ignores the mechanical and physiological difficulty of the passage of an insoluble substance through the cell walls. The second view rests on the now well-ascertained fact that albumen and its analogues are capable of undergoing disintegration in the course of which, besides other substances, fat is certainly

* 'Centralblatt,' No. 18.

Nothing can demonstrate this more clearly than the growth
gs of the common bluebottle on meat or in defibrinated blood.
efore maintained by those who hold this view, as Toldt and
, that the cells originally take up soluble albuminous com-
which, undergoing disintegration within the cells, split into
compounds that readily escape from them, and fat, which
behind. These authors hold that all the fat of the food is
d or burnt off during its passage through the economy, and
exceptionally applied to increase the natural stores present in
. A third view, to a certain extent intermediate to the other
held by Radziejewski, who maintains that an important pro-
of the fat of the food is converted into soap by the pancreatic
d the alkali of the biliary acids, and in this state easily
s the intestinal epithelium, as well as the membranes of the
fat-cells. These cells, again, have the power of converting the
nto free fat, which remains in their interior. This view is
y supported by various micro-chemical and physical researches
g the importance of saponification in facilitating the passage of
rough animal membranes. Radziejewski himself has demon-
—first, that soda soaps are absorbed in their passage through
testines; secondly, that soaps made of oils foreign to the
ny are deposited but slightly, if at all, in the form in which
re ingested. Whilst the experiments of Matteucci and others,
ng the effects of alkalies on the osmosis of fats, are well known.
whole subject is well reviewed by M. Hofmann in a paper just
ished in the 'Zeitschrift für Biologie,' giving an account of a
s of experiments in which he attempted to ascertain whether any,
if any, how much, fat was stored up from the fat consumed in the
. With this end in view, he starved a dog till it had consumed
or nearly all, the fat in its body. It was then supplied freely
h fat mingled with a small quantity of albumen. After a few
ys it was killed, and the total amount of fat in the body determined.
certain amount, it is evident, might proceed from the decomposition
the albumen; but if the quantity found exceeded this amount,
stimated as highly as possible, it is equally clear that the excess
ust proceed from the fat consumed as food. This, however, did not
prove that the fat was stored up in fat-cells, since it might be con-
tained in the blood; and a special set of researches were instituted to
show that the blood, under these circumstances, was not extraordinarily
rich in fatty compounds. The general result of M. Hofmann's
experiments, as far as they have yet gone, seems to prove definitely
that the fat stored up in the body cannot possibly be entirely derived
from the albumen consumed as food, but must be in part obtained
from the fats. M. Hofmann does not, however, attempt to explain
the mode in which the fat gains entrance into the cells of adipose
tissue—a point on which we are at present completely in the dark.

NOTES AND MEMORANDA.

Misnaming Objectives.—The following note from Mr. C. Stodder appears with the above heading in the 'American Naturalist' for August: we have added to it some remarks from Mr. Wenham:—"The brief remarks of mine, printed over the initials C. S. in the March number of this journal, were copied essentially in the 'Monthly Microscopical Journal' for April. In the May number of that periodical Mr. Wenham writes a reply. It is a remarkable paper, not only from the eminence of the writer as an authority on microscopy, but from his evident loss of temper, and by the terms to which he refers to Mr. Bicknell and to C. S. Under these circumstances I must ask for a little space for a rejoinder to my share; I have nothing to say for Mr. Bicknell, as he is able to take care of himself. Mr. Wenham commences his paper, which *he calls* a 'reply,' with this,—'to correct a misstatement that I [Mr. W.] wrote a paper in reply to one of Mr. Bicknell's; I did not commit myself to such an extent.' This is a mere quibble, unworthy of its author. The very caption of the paper had Mr. Bicknell's name in it. I should not have noticed this, had not Mr. W. unfairly, as I think, charged me with a misstatement. Next, Mr. Wenham couples C. S. and Mr. Bicknell together as if they acted in concert, and were joint writers. I can assure Mr. Wenham that it is not so. Mr. B. is not responsible for anything I have written, nor am I for him. Neither had seen the other's writing until it was public. Next, I have no 'plea or atonement' to make 'for expressions hastily or inconsiderately written.' My expressions were used deliberately and after full consideration of their import. I still hold the same opinion, namely, that selling an objective by a name that does not *approximately* indicate its focus (*i. e.* $\frac{1}{8}$ for $\frac{1}{4}$, $\frac{1}{20}$ for $\frac{1}{16}$, or, as I have known, $\frac{1}{20}$ for $\frac{1}{40}$, as in the case of an eminent French maker; or, as in another instance, a $\frac{1}{16}$ for a $\frac{1}{30}$; or, as in the case of an English objective that I have recently heard of, a $\frac{1}{20}$ for a $\frac{1}{8}$) is an 'imposition,' or a fraud if that term is preferred, not applying it, however, as Mr. Wenham represents, to a particular firm, but to all, of any country, who practise such 'imposition'; and that Mr. Wenham in his paper, by stating that '$\frac{1}{8}$ths were $\frac{1}{9}$ths or $\frac{1}{10}$ths, and some now approach $\frac{1}{12}$ths in power,' without disapproval, was practically defending the custom, and that he does not now deny. His paper in reply to Mr. Bicknell was published in December. In May he writes, 'no one knows better than myself the difficulty of adopting a nomenclature that shall exactly denote the power of all the highest object-glasses sent out'—something has evidently produced some effect on him since that time. The complaint was not of want of 'exactness,' but of gross misnamers of 20 or 50 per cent., such as he named in the December paper, not in regard to the highest powers alone, but applicable to the lowest powers as well. Such was what I called an imposition, and I call it so now. In the 'Quarterly Journal of Microscopical Science,' October, 1862, Captain Mitchell gives the measurement of the focus of several London objectives, most of them being undernamed. Captain M. complains of this. He says, 'when I buy a $\frac{1}{4}$th, I want a $\frac{1}{4}$th, not something else.'

He calls those correctly named, *honest;* by implication, those not so named, dishonest. Dr. Wm. B. Carpenter* says, 'the designations given by the opticians to their objectives are often far from representing their focal length as estimated by that of single lenses of equivalent magnifying power, a *temptation* to *underrate* them being afforded by the consideration that if an objective of a certain focus will show a test object as well as another of higher focus, the former is to be preferred. Thus it happens that what are sold as ¼-inch objectives are often more nearly $\frac{1}{5}$, and that what are sold as ⅛ are not unfrequently more nearly ¼.' I presume that I am justified in assuming that Mr. Wenham was fully aware of both the above, that Captain Mitchell termed the custom dishonest in 1862, and Dr. Carpenter that it was the result of 'temptation' in 1868, yet he did not feel called on to 'practically defend' the want of honesty, or the yielding to temptation. Was he not then as now 'a witness in behalf' of those *he* calls the 'most respectable portion of the body'? Was it only censure from this side of the Atlantic that was 'worth caring for'? It certainly looks so. For some twenty years I have watched Mr. Wenham's contributions to microscopy. I have used and admired his ingenious inventions and appliances, and have looked upon him as one of the foremost leaders and authorities in the mechanical and theoretic departments of the science. It was with regret that I saw that he did not disapprove of the fictitious nomenclature. It is with greater regret that I find that he has in his haste used arrogant expressions. The question of nomenclature is now being agitated, the attention of microscopists is attracted to it, and one consequence will be that the 'honest' makers will be appreciated."

Appendage to Mr. Stodder's Remarks from 'American Naturalist,' by Mr. Wenham.—Mr. Wenham sends us the following reply:—I should not have taken time to notice the above long comment on my short letter, appearing on page 234 of this Journal for May, 1872, but for the remark that my letter was written with "evident loss of temper"! Quite the reverse; it was penned in a spirit of "chaff," and Mr. Bicknell, in his brief note in reply, seems to have caught the vein; at which no one, perhaps, laughed more heartily than myself. On the other hand, it has drawn C. S. out of his shell, with horns erect, in his proper name or colour. I have nothing further to say on the question, which leads to no scientific discovery, and is one to be settled between the makers of object-glasses and purchasers, who are now sufficiently warned. No particular reform can be anticipated by pages of a controversy having from its very basis such full scope for personalities, of which *this* and the above may be taken as a sample. The tone is becoming silly and tiresome; and having contributed my share, I must drop the subject, with the remark that no one would be more willing to induce the makers to adopt a nomenclature having a definite reference to actual magnifying power than myself, could I see the possibility of doing so. Numerals such as those adopted by the Continental makers would perhaps partly meet the difficulty; but I believe that no English optician would consent to name his glasses this way.—F. H. WENHAM.

* 'The Microscope,' 4th edition, 1868, p. 184.

A New Slide for Exhibiting Infusoria is described by Dr. Hunt, who read a paper at a late meeting of the Biological and Microscopical Section of the American Academy of Natural Sciences. In this paper Dr. Hunt called attention to an ingenious slide invented by Mr. D. S. Holman for exhibiting infusoria, especially under the gas microscope. These slides are made by grinding a deep depression in a thick slide, and then making a very shallow circle all round the deeper central cavity. If the central depression be filled with the organization in water which is desired to be studied, and covered with a thin glass, in a short time the shallow circle all round the margin will be filled with minute bodies, often zoospores or other objects in the life history of the object in the deeper cell. The pressure of the atmosphere retains the cover in position.

Photo-mechanical Printing.—Incidentally to a pathological report to the Surgeon-General, says the 'American Naturalist,' Sept. 1872, Dr. J. J. Woodward calls attention to the familiar disadvantages of the usual means of representing in publications the magnified appearance of microscopic objects by etchings, lithographs, and woodcuts. All such hand work is laborious and wasteful of time if done by the investigator, and liable to omit the most important points if entrusted to another artist. Even the microscopist himself, being unable to represent all that he sees, is obliged to select what he conceives to be of importance, and thus represents his own theories rather than severe facts. [If, however, his theories are correct, and his delineation skilful, this very power of selection and construction enables him to give a distinctness and completeness which is lacked by the photographic camera.] The advantage of truthfulness is on the side of photomicrography, but silver prints are expensive, inconvenient, and not permanent, and the reproduction of the negatives in permanent inks is greatly desired. Two such methods are now successfully used in the United States. By the Woodbury method a relief surface of gelatine is produced from the negative by the action of light, and from this a metal "intaglio" is produced by pressure. In this a series of suitably coloured gelatine films, which constitute the prints, are formed by mechanical means. Thus was produced the photograph of *Amphipleura pellucida* in the last April number of the 'American Naturalist.' In the Albertype process a printing surface (not a relief) is produced through the negative on a gelatine film by the action of light. The prints thus produced are, at present, less expensive than the Woodbury prints, and more convenient for book work, but the edition is less uniform. On enamelled paper the prints are handsomer, but will not bear much handling.

The Angular Aperture of Immersion Glasses. — In the 'American Naturalist' for September, Dr. Josiah Curtis says that all lovers of progress in scientific research feel much pleased with the labours of investigators who make good use of the extensive means at their control, as well as also with the results which have emanated from the patient thought and close study of such men as Mr. Wenham and others abroad. "We are very apt, however, to give too little credit to the intelligent instrument-maker himself. The mind which combines science with practice in its application has great advan-

tages, and should be both respected and encouraged. How we have been forced to modify our opinions, since an angle of aperture of, say, 150° in microscopical objectives was considered absolutely unattainable! No doubt the very men who honestly and firmly believed those things impossible which are now quite familiar, were as glad as any one, when they became convinced, by facts, that they had been in error. Equally gratified, probably, will be Mr. Wenham, when he shall see for himself that an angle of more than 82° can be attained through balsam. Within a few days, I have had a good opportunity to see a $\frac{1}{10}$th objective of Mr. Tolles' make give an angle of 92° through balsam with tank arrangement of Mr. Wenham. I feel disposed, however, to let Mr. Tolles speak of this in his own words, the more so as my time is extremely limited:—

DR. JOSIAH CURTIS, BOSTON, *July* 8, 1872.

Dear Sir,—At my request, you were present recently when I measured the angular aperture of an immersion $\frac{1}{10}$-inch objective when immersed in balsam. You verified the results gained at that time. We used the tank method of Mr. Wenham.*

The $\frac{1}{10}$-inch tested, I stated to you, had in air angular ap. of 170° (upwards).

In water we found the angle to be 110° +.

In balsam the angle was fairly 95°, using petroleum lamp flame, thin, for light, in a darkened room. As you will remember I remarked that with sunlight I got two degrees more.

It will, I know, be of interest to you, and I am sure to some others, to hear of results of test of angle in some other cases. I will, therefore, set down here the angle of aperture found to pertain to some of my immersion objectives in balsam, in water, and in air.

	Air.	Air.	Water.	Balsam.
Single front	$\frac{1}{8}$ in. ..	170° ..	120° ..	87°
„	$\frac{1}{16}$ „ ..	170° ..	110° ..	88°
Compound front	$\frac{1}{10}$ „ ..	175° ..	117° ..	95°
„ „	$\frac{1}{16}$ „ ..	175° ..	105°	
„ „	$\frac{1}{8}$ (high)	172° ..	106° ..	88°
Single „	$\frac{1}{8}$ in. ..	175° ..	127° ..	110°

The varying differences between the water and balsam angles can, in a general way, be accounted for from the formulas of construction differing considerably, each one from any other.

Of all these objectives the most effective (especially when its low power is considered) is the $\frac{1}{5}$-inch of 110° *in balsam*. This is true of its use for objects mounted in balsam, as Rhomboides, *small*. But notably so as to its work on dry A. pellucida. My London specimen of this, received through the U. S. A. Medical Museum, is resolved into lines that *shine*, I may say. The illumination I used was petroleum lamp flame, no condensation. With the same means *all* the objectives show A. pellucida with the same illumination, but with a difference.

With sunlight and a blue cell no doubt the higher powers would have their proper advantage.

* See 'M. M. Journal,' August, 1871.

It is proper to mention that the ⅛-inch of 110° balsam angle was constructed on the plan proposed by me in the London 'Monthly Microscopical Journal' for March, 1872, where I have made use of a diagram by Mr. Wenham of a ⅛-inch of his construction, to *indicate* modifications such as would give more than 81° or 82° in balsam.

With proper appliances below the balsam-slide (as pointed out by me in the London 'Monthly Microscopical Journal,' for July, 1871), we can with this objective (⅛-inch) utilize 110°, instead of about 81°, the limit of the amount of angle otherwise available. *In the first place* the large angle must have access to and through the balsamed object *from below;* in the next place the objective must be capable of receiving and transmitting that dimension of pencil to the eye, which thing previous to my own demonstrations has not been shown to have been done.

With much respect, yours truly,

ROBERT B. TOLLES."

CORRESPONDENCE.

MR. STODDER AND MR. TOLLES, AND THEIR OBSERVATIONS.

To the Editor of the 'Monthly Microscopical Journal.'

October 7, 1872.

SIR,—In the July number of this magazine a letter appeared from Mr. Stodder containing strictures on an article I had contributed; of which I took no notice at that time, partly from the intimation it contained that something more would be forthcoming on the same subject. Perhaps, therefore, you would now allow me space for a few observations in reply.

Mr. S. thinks it a breach of literary etiquette that I should have so unceremoniously put his error down to the account of Mr. Tolles instead of himself. That he was in fact prompted by Mr. Tolles, he admits, the prompting being, however, in this case not oral but by printed catalogue. But though, as it happened, correct as to the fact, I had no "right," he considers, to assume it, assuming, as it were, that he has not sufficient knowledge to originate a mistake for himself. But here Mr. Stodder has forgotten—owing no doubt to the multiplicity of his literary works—that he had himself published the information which took him so much by surprise. In the sixth volume of this Journal, at p. 201, he will find notice given, on his own authority, that he is to be considered only as the exponent of Mr. Tolles, and, with special reference to this particular question, that he does not presume not only to express, but even to possess, an opinion of his own.

As Mr. Stodder has not time to read his own compositions, it would perhaps be too much to expect him to read those of others. I had expressed an opinion that a ⅛th of Mr. Tolles might have an angle as high as 160°. Afterwards seeing Mr. Stodder's announcement that this glass has, with water, an angle of 140°, I intimated, by notes of

admiration, that he ought to have known this was absurd and impossible. Bringing these two together, Mr. S. announces his discovery of a fatal contradiction.—"In November he would not be surprised by an aperture of 160°; in April he is astonished by an aperture of 140°." Upon this he proceeds to comment with severe irony that I must have made progress in the study of Optics, and then dismisses my contribution as a "ridiculous lucubration."

Now the thing—the one thing—asserted in this controversy from the beginning is this, that an aperture, whatever it may be when the object is in air, is abruptly changed when the object is immersed: so that the angle of a glass, *the same glass*, is totally different when used in one way from its angle when used in another way. Entirely unconscious that this is itself the "thesis," Mr. Stodder writes to announce his discovery,—that he has detected a contradiction. He is ready at a moment's notice to decide the controversy;—that he does not know, and has never asked, what it is about, does not seem to have given him the smallest concern.

This is Mr. Stodder's contribution to the discussion. It is his own contribution, written without prompting; as from another passage we learn incidentally that Mr. Tolles was absent, and had no share in it.

I have thought it worth while calling attention to this episode, because it is instructive beyond the immediate question concerned. Mr. S. has the pen of a very ready writer. His name is familiar to the readers of many journals in many lands. From his many appearances he has come to be looked on by casual readers, latterly also by himself, as an authority on the more recondite questions of optical science; and has performed the difficult feat of building up a reputation for profound learning on a knowledge derived from the perusal of printed catalogues. His creed is, that on every disputed point Mr. Tolles is right, and the other side wrong: a fidelity the more to be admired when we find him ready to prove his position not only without being able to understand the question, but without even knowing what it is.

As I observe from your editorial notice that Mr. Wenham declines any further controversy with Mr. Tolles, I add a few observations on his lately-published table, the last probably I shall have occasion to make in connection with it. This table Mr. Tolles offers as "proof enough" of the position he has maintained. Now assuming for a moment the accuracy of his figures, of what are they a proof? The position which he undertook, which he attempted by diagrams to prove, and which now in this his latest paper he reasserts in express words, is this, that by an immersion lens *the whole angular pencil can be transmitted*. In proof of this we are offered measurements of nine glasses; and how much do they give? They differ a little, but taking them on the average we find just 90° for each. And this is offered as "proof enough" that the whole pencil, or 170°, can be transmitted.

90° however is something greater than 82°, the limit scientifically assigned. And what this extra 8° shows is only this, that Mr. Tolles cannot be trusted to take an observation correctly. Of this we shall know more when the glass which he has so kindly sent over shall

have arrived. Meantime, as a preparation for what we may expect, a comparison of his published figures supplies sufficient material for estimating his accuracy as an observer.

In the first place it will be remembered that, having expressly tested a glass of his own, he wrote to say he had found its angle to be 100°. But on the following day his secretary writes to say that having looked again he found it was a mistake; the angle was really 110°.

Again looking at last table we find him claiming for one of his glasses an aperture of 178°. (By-and-bye, for it is in the nature of Art to progress, we may expect to hear one announced having 179°, to be followed, doubtless, in due time, by another of 180°.)

Again, in the same list (p. 106) we find the figures contradicting themselves. We have, *e. g.*, two glasses with apertures of 175°, having different angles in water, the first giving 117°, against the smaller angle of 113° for the second; but going on to the column for balsam, the reduction is inverted, the second, which was lower in water, being now the higher in balsam,—95° against 93°. *Both* results cannot be true. For the loss for different media is connected by a fixed relation (though not of simple proportion); a diminution for one answering necessarily to a corresponding diminution for the other. All four measurements therefore cannot be right. (As a matter of fact, all four are wrong. The true aperture for both sets is in water 97½°, in balsam a few minutes over 80°; on the supposition, that is, that the angle in air has been correctly taken.)

Further, I have only to observe, that Mr. Tolles in this paper repeats mistakes formerly pointed out, and repeats them without apology or explanation, as if they had never been corrected. Under these circumstances it may perhaps be allowed that as much attention has now been given to these "demonstrations" as can in courtesy be required, and that further notice of them will not be necessary.

It is perhaps not fortunate for the "general reader" that so much of this subject has been elucidated controversially; for the style naturally used in proving a disputed truth is not generally the best that might be chosen for making clear what it is. I think it not unlikely, therefore, that one result of these discussions may be the appearance of some obscurity as to the application of the principle, on account of the variety of combinations which occur. The object, *e. g.*, may have a cover, or it may be without one; it may be in air, or it may be immersed in water, or immersed in some denser medium; and the object-glass may be dry or it may be immersed. These combined in different ways give an appearance of complexity, which however is apparent only, and not real. I therefore take the opportunity of this letter to re-state the principle in its application, which may be done simply enough; for whatever the number of cases, the same invariable principle governs them all.

Every case that can arise comes under one or other of two classes; the object itself is either dry or immersed.

Suppose, firstly, that it is dry. It may be either with or without a cover; this will not affect the aperture. The glass itself may be either

immersed or dry, but neither will this affect it; it is only a case of a thicker or thinner front. In all these cases the aperture will be given, with approximate accuracy, by the ordinary method.

Next, suppose the object immersed. It may be covered or uncovered, the glass may be immersed or dry, but in every case alike the common method will fail, and must be set aside. There is a limit, easily determined, beyond which the aperture cannot pass; and this limit will depend on the medium in which the object is placed. Take the angle whose sine is the reciprocal of the refractive index; twice this angle is the limit of the aperture. If we wish also to determine experimentally the actual aperture in a given case, or how much it falls short of the limiting value, we must use the method described by Mr. Wenham, which inexperienced workers will do better not to attempt. Otherwise, however, it may be ascertained also by calculation, by a method which it is not necessary to specify here.

Briefly, therefore, the limitation or non-limitation of the aperture is governed by the immersion or non-immersion, not of the object-glass, but of the object.

Your obedient servant,

S. L. Brakey.

Using the $\frac{1}{8}$th Binocularly.

To the Editor of the 'Monthly Microscopical Journal.'

35, Upper Park Street, Clifton, Bristol, *Oct.* 19, 1872.

Sir,—I was greatly surprised to hear at a meeting of our Microscopical Society that Mr. Wenham was engaged in perfecting an arrangement of his Binocular Microscope, by means of which the $\frac{1}{5}$th object-glass could be employed binocularly, since I have been in the habit of using my Ross's $\frac{1}{8}$th with Mr. Wenham's instrument (the work of Mr. Husbands, of this city) for some time past, with excellent effect. My "modus operandi" consists in withdrawing the prism until the dark outline of its edge crosses the centre of the field exactly, and placing the instrument in such a position that the light falls upon the mirror from the left; by this method, and especially by the aid of the condensing lens and petroline light, I can examine with very satisfactory results objects suitable for the $\frac{1}{5}$th. That the object is seen with both eyes may be easily proved by placing a circular diatom, an arachnoidiscus for example, in the centre of the field, so that the dark line before referred to crosses its centre, and by closing one eye, and looking down the tube opposed to the other, and *vice versâ*, it will be at once apparent that opposite halves of the diatom are seen, and by now employing both eyes simultaneously the diatom presents itself in its entirety, and its markings can be made out perfectly.

Apologizing for trespassing upon your valuable space,

Believe me, Sir, yours truly,

Samuel Smith,
Surgeon.

PROCEEDINGS OF SOCIETIES.

Royal Microscopical Society.

King's College, *October* 2, 1872.

W. Kitchen Parker, Esq., F.R.S., in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations was read, and a vote of thanks passed to the respective donors. The Secretary called the attention of the meeting to a series of photographs of Nobert's lines, which had been sent to the Society by Dr. Woodward, of the U. S. Army; the 19th band having been defined by Dr. Woodward with Tolles' lens in a far better manner than in any previous attempts to resolve it. A note from the Dr., not in the room at that moment, would be printed in the Journal, but he (the Secretary) would just state that the lens with which such successful results were obtained is one that is not perfectly corrected for chromatic errors. Lenses more perfectly corrected did not show the lines so well. This was an important fact, and bore out a practice (of many years' standing) by Messrs. Powell and Lealand. Their recent lenses, which are admirably corrected for chromatic errors, have been found better adapted for most microscopical work of a very delicate description, and yet a lens having a certain quantity of chromatic error left in it produced a better photograph of the 19th band.

Mr. Ingpen inquired whether the glass was over-corrected or under-corrected.

Mr. Brooke thought it was probable that the lens was corrected for the rays which have the greatest luminosity, and that the darker ones were left to take care of themselves.

Mr. Ingpen: If there was a slight over-correction for the blue, great power would be obtained in photographic work.

The Secretary said he had received from Mr. Tegetmeier a request that the opinion of the Fellows should be obtained upon a pigeon's feather which presented the abnormal condition of a second growth, or outgrowth upon an already formed feather.

Dr. Braithwaite thought the condition of the feather resembled somewhat that which is seen in the vegetable world, where a continuation of the axis of a leaf takes place, the growing point continuing itself and producing a repetition of the previous growth.

The President concurred with Dr. Braithwaite in his opinion.

Mr. Lowne thought the peculiarity was very much like what was seen in the peacock's feather, where a similar separation of parts was exhibited. The condition was due to the bird being in a state of starvation at the time when the growth was going on. The quill of a feather enclosed a quantity of cellular matter which developed as the quill grew; and in sickness, or when the bird was in a starving state, the cells died. But it was not unusual to see a fresh quill, as it were, growing out of the first quill. The appearance presented was the result of a morbid process in some degree.

Dr. Hudson then read a paper, "Further Researches on *Pedalion mira*; a new Six-limbed Rotifer."

Mr. Slack said that having had the advantage of seeing specimens of Pedalion which Dr. Hudson had kindly sent him, he was in a position to state that the results laid before the Fellows that evening gave no notion of the immense amount of trouble Dr. Hudson must have taken to understand the creatures. Those exhibited before the Society that evening were sufficiently hungry and empty to be comparatively intelligible. The interior organs of specimens he (Mr. Slack) had examined were practically invisible, owing to the presence of a quantity of coloured matter, and they did not live long enough to permit of their being reduced to the requisite condition of attenuation. The creature, in a free-swimming state, exhibits a peculiar jerking motion like *Triarthra*, which was the nearest approximation to it in being furnished with articulated spines. Pedalion's rapid mode of swimming renders it extremely difficult to follow its movements. After alluding to the unsatisfactory definition of the term Rotifer, Mr. Slack continued: If all the oral cilia were taken off from Pedalion, anyone looking at it in that condition would take it for a small crustacean. Many naturalists, Gosse among them, had pointed out the affinities of Rotifers with Articulata, and certainly the creature under discussion approached that type more nearly than any that had been previously found. From the perfection of its swimming organs and their complexity, there was an evident resemblance to the *Arthropodous* type of animals. The specimens he saw had no active cilia at the tail bifurcations, and it was very probable that Dr. Hudson's explanation that they had been shed was correct.

Mr. Lowne said he thought that some forms of Arthropodous Rotifers had been described in Kölliker's 'Zeitschrift' about twelve months ago. These rotifers had what he (Mr. Lowne) took to be Arthropodous legs, and the interior arrangement of the animal had been set forth in nearly the same words as those Dr. Hudson had used in describing Pedalion. The memoir was written by a Russian.

Mr. Stewart said he thought it possible there might be some difficulty in distinguishing between the Rotifers and some of the more minute forms of *Crustacea*. He would only mention one of the definitions to be found in text-books, namely, that no *crustacean* has, in any parts of its body, cilia; whereas these creatures which Dr. Hudson had described in his admirable paper had articulated limbs, and the great majority of them had cilia, although some exceptional forms might be deprived of them.

Dr. Hudson said he had seen the drawing referred to by Mr. Lowne; it had been published shortly after he (Dr. Hudson) had made his own public; but it differed essentially (as he demonstrated by figuring it) from Pedalion. This creature, as to its classification, he would place simply among *Arthropoda*, hollow-limbed animals. What, broadly speaking, was the difference between a worm and an insect? It was the *homonomy* of one and the *heteronomy* of the other. Take a transverse section of a worm and you get the sections repeating themselves throughout the extent of the form; but in the transverse sections of

insects you get heteronomy; so if Pedalion be divided transversely you can get no homonomy; and its structure he thought fitly entitled it to rank among the *Arthropoda*.

The President asked whether it was certain that there were no ciliated larvæ of Crustacea.

Mr. Stewart said he never heard of such a thing; he had examined a good many Crustacea, and had never seen larvæ.

A vote of thanks was then passed to Dr. Hudson.

The President then read a paper "On the Development of the Skull of the Crow."

Mr. Hogg wished to impress upon the Fellows that the paper which had been read to them was the result of many months' hard work at the microscope. The President took the embryo from the time that it was a day old, and watched its development day by day until the whole structure has been worked out. He merely mentioned the fact, as the President scarcely did himself justice by merely placing the crude results of his work before the Fellows. He (Mr. Hogg) had had the pleasure of seeing the President at his labours, and he could not help testifying to the wonderful patience and skill with which he conducted his observations. He concluded by moving a vote of thanks to the President.

The next ordinary meeting of the Society will be held on the first Wednesday in November, and a scientific evening for the exhibition of objects and conversation will be held on the 11th of December, particulars of which will be sent to the Fellows in the usual way.

Donations to the Library and Cabinet, from June 5th to Oct. 2nd, 1872:—

	From
Land and Water. Weekly	*The Editor.*
Nature. Weekly	*Ditto.*
Athenæum. Weekly	*Ditto.*
Journal of the Society of Arts. Weekly	*Society.*
The Lens, No. 3	*Editors.*
Smithsonian Report for 1870	*Smithsonian Institution.*
Report of the Commissioner of Agriculture for 1870	*Ditto.*
Monthly Report Department of Agriculture for 1871	*Ditto.*
Gibson's Flora of Essex	*Author.*
Two Cases of Cancer. By J. J. Woodward	*Ditto.*
Popular Science Review, No. 44	*Editor.*
Journal of the Quekett Club, No. 19	*Club*
Quarterly Journal of the Geological Society, No. 111	*Society.*
Acta Universitatis Lundensis, 1869 and 1870	*University.*
The Canadian Journal, July, 1872	*Institute.*
Berichte des naturwissenschaftlich-medizinischen Vereines in Innsbruck, 1872	*Univ. Buchhandlung.*
Transactions of the Linnean Society, Vol. 28, Part II.; and Vol. 29, Part I.	*Society.*
Journal of the London Institution, No. 16	*Institution.*
Series of Photographs, showing the Resolution of Nobert's 19th Band with a Tolles' Lens. By Dr. Woodward	*Author.*

Walter B. Cole, Esq., was elected a Fellow of the Society.

WALTER W. REEVES,
Assist.-Secretary.

Brighton and Sussex Natural History Society.

September 27th.—Microscopical Meeting. Mr. G. Scott, President, in the chair.

Mr. Wonfor announced that the Lewes Natural History Society would hold a Microscopical Soirée on the following Monday, to which members of the Brighton Society were invited. He hoped some would be able to attend with their microscopes, and return the kindness rendered by the Lewes Society at the British Association Soirées.

Mr. Wonfor then remarked that Dr. Carpenter, F.R.S., very kindly gave him a small quantity of the Atlantic chalk dredged up in the Porcupine Expedition. Some of this he had brought down for distribution, and, if time allowed, he would show that it contained foraminifera identical with those obtained from the Chalk Downs.

Mr. Hennah then drew attention to a new series of objectives constructed by Mr. Wenham. Some would recollect seeing the new patent objectives at the Association Soirées; but Mr. Wenham kindly promised to send some down for exhibition to the Society. They were on a perfectly new method of construction, two or three of the lenses of the older construction being dispensed with, and a single front and back lens used. It was hoped thus to obtain a cheaper series of object-glasses; but that would be no advantage unless better performance was also secured.

Those sent down were all very good,—the high powers were especially so; but he would advise the members to wait for still greater improvements. The $\frac{1}{15}$th was an exquisite glass when properly adjusted, which, except in the hands of an experienced microscopist, was not easy of accomplishment. He considered the $\frac{1}{10}$th a perfect glass, with a good working distance. They could be used as dry or immersion lenses. At present there was an inequality, for, while very good as dry on some objects, they were not so good when wet, and *vice versâ*; but on some objects the performance was equally good, whether wet or dry.

He had written to Mr. Wenham on the subject of an objective with moderate angle of aperture for naturalists' work, because he did not believe in the high-angled glasses at present in use, and he would read what Mr. Wenham said:—"I agree with you that a $\frac{1}{20}$th of about 95°, with corrected aperture (I do not mean a large aperture *cut down* to 95°), but a system just worked up to that aperture, so as to have a long focal distance, say $\frac{1}{30}$th of an inch, would be a valuable glass in the hands of the naturalist, enabling him to *see into* things instead of a mere surface observation of a few diatoms, for the sake of performing the feat of defining the difficult marking of some half dozen of them, and this is only what such a glass is at present used for."

He considered this a wonderful admission, and any new system which would give them objectives for physiological and natural history investigations would be a great advance in the utility of the microscope. Another altogether perfect objective sent down for exhibition was a $\frac{1}{2}$-inch. While the magnifying power was very great,

the definition was clear and as deep as in an inch or inch-and-a-half. It was of 85° aperture, had a good working distance, was coned down to admit the pencil of light for bull's-eye or any other illumination, with a very large Lieberkuhn for opaque objects. There was also the patent reflex illuminator, for getting opaque illumination, with very high powers, which did its work remarkably well. Mr. Wenham, as an amateur, had done more than anyone else for the microscope; he had devised the binocular and given it to the world. While directing some improvements in the series of objectives of Messrs. Ross he had hit upon the form they have patented. These experiments have cost them a large sum, but Mr. Wenham had no pecuniary interest in the patent.

Mr. Wonfor inquired whether the higher objectives worked readily through ordinary covers.

Mr. Hennah replied that they did. He would exhibit a *P. angulatum* with a comparatively thick cover.

Votes of thanks were then given to Messrs. Hennah and Wenham. The meeting then became a conversazione, when Messrs. Hennah, Glaisyer, and Wonfor exhibited various objects with the new objectives. Their performance was pronounced admirable. The $\frac{1}{15}$th and $\frac{1}{10}$th showed *P. angulatum*, and other diatoms equally well, with Podura scales, and ultimate fibre from the pig, while the ½-inch gave beautiful definition and as great a depth as most 1-inch objectives. Later in the evening Mr. Hennah strained up the ½-inch with the highest eyepiece, and showed *P. formosum* marvellously.

Some very interesting objects were also exhibited by Messrs. F. E. Sawyer, W. H. Smith, and Shillingford; and Mr. Wonfor during the evening showed identical forms of foraminifera from chalk from the interior of a flint nodule picked up on the Downs and from *Atlantic chalk*.

Reading Microscopical Society.*

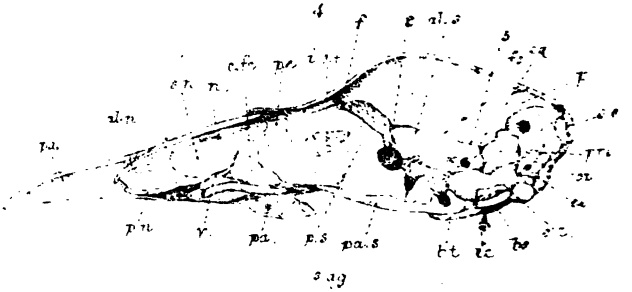
October 9th 1872.—Dr. Shettle presided; Captain Lang being unavoidably absent.

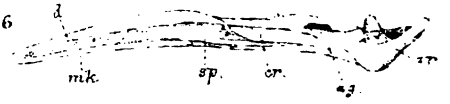
Mr. Tatem laid before the Society "Notes on new Acarelli," accompanied by drawings and balsam-mounted specimens of *Acarellus pulicis* and *Acarellus muscæ*; both being transitional forms which would doubtless have developed into perfect *Acari*.

Mr. Austin exhibited mycelium and spores of *Peronospora infestans*, with sections of sound and diseased potatoes, and of Dodder, showing the manner of its parasitic attachment to plants.

Mr. Tatem exhibited mounts of *Trombidium* and entire insects.

* Reported by Mr. B. J. Austin, 107, Oxford Road, Reading.





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THE

MONTHLY MICROSCOPICAL JOURNAL.

DECEMBER 1, 1872.

I.—*Description of the Plates illustrating Mr. W. K. Parker's Paper on the Development of the Crow's Skull.*

(*Continued from page* 226.)

[N.B.—The description of the various stages of the Crow's Skull, and the first three Plates, will be found in the November number of this Journal, pages 217–226.]

DESCRIPTION OF PLATES.

PLATE XXXVII.—Structure of the skull of the Nestling of the Carrion Crow (*Corvus corone*), one week old. The figures are enlarged one-half. Fig. 1. Basal view of skull and face. Fig. 2. Side view of skull and face, including the mandible, showing its outer aspect. Fig. 3. Upper view of the skull, upper jaw, and nasal sacs. Fig. 4. A longitudinally vertical section of skull and face, showing the brain-cavity, auditory bones, septum nasi, and cranio-facial axis. Fig. 5. End view of skull and lower jaw. Fig. 6. Inner view of right mandible, or lower jaw. Fig. 7. "Os hyoides," or distal part and base of 2nd post-oral arch, and the whole of the third, with its basal element.

PLATE XXXVIII.—Figures illustrating the skull of a Fledged Nestling of the Rook (*Corvus frugilegus*), natural size. Fig. 1. Basal view of skull, face, and nasal capsules. Fig. 2. Side view of skull and face, including outer view of mandible. Fig. 3. Upper view of skull and upper jaw. Fig. 4. A longitudinally vertical section of skull and nasal capsules, showing cranial cavity, auditory mass, septum nasi, cranio-facial axis, and inner view of mandible.

PLATE XXXIX.—Skull of Fledged Nestling of Rook—continued. Fig. 1. End view of skull of a scarcely fledged Nestling. Fig. 2. Ditto of an older Nestling. Fig. 3. Nasal labyrinth, dissected out and seen from above. Fig. 4. Transversely vertical section of nasal capsule in front of lateral ethmoids. Fig. 5. A similar section, seen behind the lateral ethmoids. Fig. 6. Inner view of nasal capsule, as seen in a longitudinally vertical section. Fig. 7. "Os hyoides."

II.—*Proposal for a Standard of Comparison of the Magnifying Powers of Compound Microscopes.* By J. E. INGPEN, Esq.

(*Read before the* ROYAL MICROSCOPICAL SOCIETY, *Nov.* 6, 1872.)

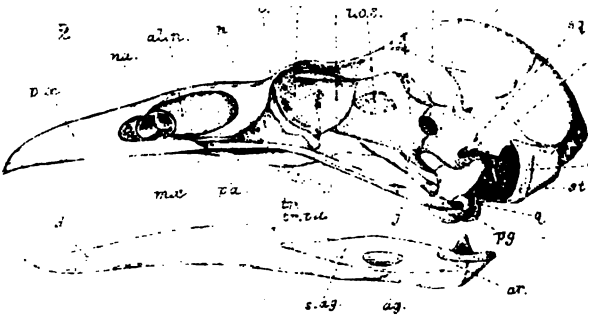
EVERY worker with a telescope finds the necessity of ascertaining with considerable accuracy the magnifying powers of his instrument with its various eye-pieces. This is done by measuring the diameter of the reduced image of the object-glass in the eye-piece, when the telescope is focussed for parallel rays. The aperture of the object-glass, divided by the diameter of its reduced image, gives the magnifying power. The advantage of this method is, as

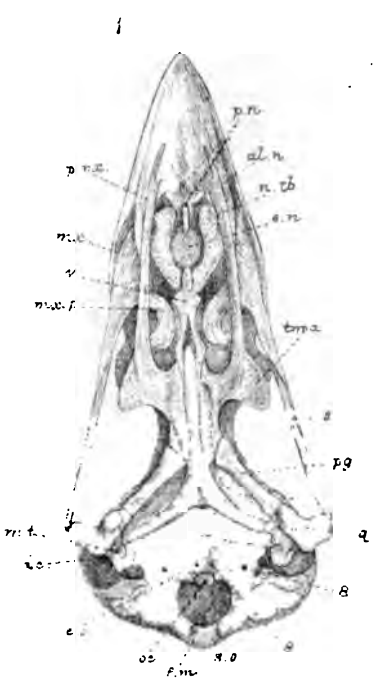
Admiral Smyth says, that "it does not require any knowledge of the thickness and focal lengths of any of the lenses employed in the telescope, nor yet of their number and relative positions;" and he quotes Dr. Pearson thus: "One operation includes the result arising from the most complicated construction, and sets theory at defiance with respect to calculations that must take into consideration the previous determination of all the preceding requisites, the obtaining of which is attended with practical difficulties almost insurmountable."* I can hardly imagine but that some one interested both in telescopic and microscopic research must have long since applied the same method to the microscope that has been used for the telescope with perfect success for more than a century past. So far as I know, however, this has not been done, or at any rate, published, and I therefore venture to bring this matter before the Royal Microscopical Society, in the hope that it may be of interest and utility to some few amongst us, and perhaps lead to further inquiry.

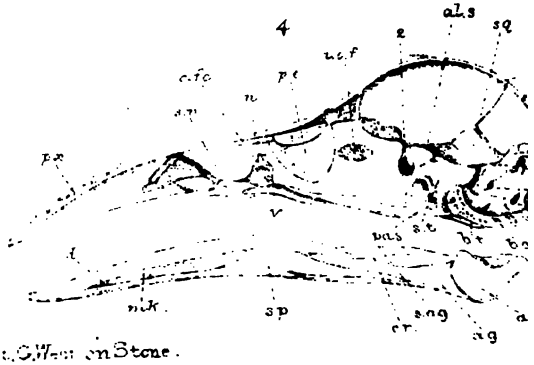
The conditions under which we use this plan of obtaining the magnifying power are not quite similar in the two cases. In the astronomical telescope we deal with parallel rays, and the focus is therefore constant for each instrument: in the microscope we use various objectives as well as eye-pieces, and conjugate foci instead of parallel rays. But if we fix upon a standard object and view it at a known distance the method is equally applicable. The standard I propose to use is the diminished image of a disk one inch in diameter, placed at ten inches below the position of an object upon the stage of the microscope; then the real diameter of that disk divided by the diameter of its reduced image will give the standard magnifying power with the objective eye-piece and length of body employed, and if the length of body is such as to make the distance between the front surface of the objective and the diaphragm in the eye-piece *ten inches*, then the real as well as the standard magnifying power is given.

The reduced image of a disk one inch in diameter viewed at ten inches below the stage is too small to be measured with accuracy when high powers and deep eye-pieces are used, but any multiple of one inch or divisor of ten inches can be employed. For most purposes the following plan seems to answer: A revolving diaphragm, capable of being adapted to the stage, is pierced with apertures of $\frac{1}{4}$ inch, $\frac{1}{8}$ inch, and $1\frac{1}{4}$ inch, and fixed to a tube at $2\frac{1}{2}$ inches from a plate of thin glass, upon the upper surface of which the objective can be focussed. This will allow of the objective being used at "uncovered," at "covered," with any known thickness of covering glass, and with a wet or dry front. On the same, or another diaphragm, some minute holes, at known distances apart, such as $\frac{1}{4}$ inch and $1\frac{1}{4}$ inch, can be pierced, and used in the same manner. It is evident

* Smith's 'Celestial Cycle,' i., 382.







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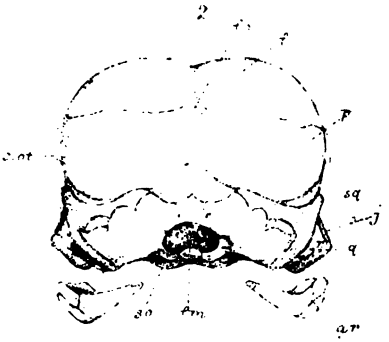
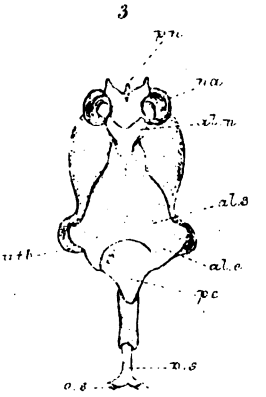
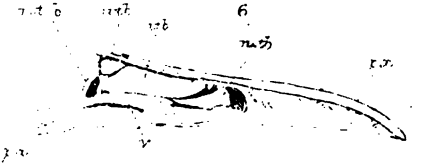
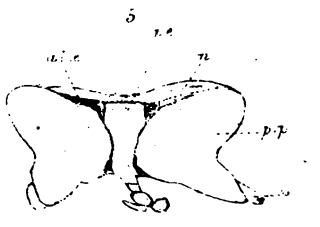








P. deladrier & Watson Stone.

W. Thomas & Co.

Development of Crow's Skull.

that the $\frac{1}{4}$-inch aperture, or the holes $\frac{1}{4}$ inch apart, represent the 1-inch disk, the $\frac{1}{2}$-inch aperture a 2-inch disk, and the $1\frac{1}{4}$-inch aperture a 5-inch disk at ten inches distance. In the latter cases the result would have to be divided by 2 or 5 to obtain the power.

The next point is as to the mode of measuring the reduced image in the eye-piece. For low powers the ordinary divided scale dynamometer may suffice; for higher powers a double-image dynamometer, or one with a compound microscope, would be necessary. With high powers a measurement with the lowest eye-piece would suffice, as the *relative* powers of the eye-pieces can be very exactly obtained: the mean of several observations should be taken. It may be as well to subjoin an example to show the method of working:—

Disk $1\frac{1}{4}$-inch at $2\frac{1}{2}$ inches = 5-inch disk at 10 inches distance.
Diameter of image ·028) 5·00 (178·6 power nearly.

I venture to think that this method will be found well adapted for a standard of measurement. It is simple, showing the power with the smallest amount of calculation, whatever may be the foci and construction of the objective and eye-piece, and length of body, adjustment for cover, &c. If the body is made, as before suggested (by means of suitable adapters), to measure ten inches from the front surface of *each* objective to the stop in *each* eye-piece, the standard will agree with the real measurement; and even if the difficulty of measuring the minute disks of light be considered too great with high-power objectives, even approximations may be useful, if based upon a standard the conditions of which can be easily remembered and reproduced.

There are two uses of the *optical* portion of the dynamometer, which, though long known, seem to have been too much overlooked of late years. In the form of Ross's centreing eye-piece, *viz.* a positive eye-piece of moderate power, means are afforded of ascertaining very exactly the condition of the inside of the tube, the positions of stops, existence of stray light, extent and centricity or obliquity and direction of illumination, &c. The other use is that of obtaining the *real working* angle of aperture of an objective when direct light is used and the eye-piece is in position, and not merely the angle at which a burr of oblique light may be *just* visible. This plan was used in 1862 in examining the objectives at the International Exhibition.*

If a double-image dynamometer is used, it must be ascertained if astigmatism exists in the eye of the observer. This defect (which is much more common than is generally supposed) will greatly vitiate the measurements, unless corrected by the use of proper (cylindrical) lenses.

* See 'Quarterly Journal of Microscopical Science,' IV., N.S., 1864, p. 84.

III.—*On the Structure of the Valves of Eupodiscus Argus and Isthmia enervis, showing that their silicious deposit conforms to the general plan of deposition in simpler forms.* By Henry J. Slack, F.G.S., Sec. R.M.S.

(*Read before the* Royal Microscopical Society, *Nov.* 6, 1872.)

Plate XL. (Middle portion).

The observations now brought before the Royal Microscopical Society may be regarded as a continuation of those previously communicated on Pinnulariæ, as they have the same object, and tend towards the same conclusion that the silicious deposition in diatoms probably follows one uniform plan, and that the silex is deposited in spherules, varying in dimensions and in degree of proximity; thus according with Mr. Wenham's researches.

Since the publication of the paper on Pinnulariæ numerous fresh observations have been made, confirming the results then mentioned, and efforts were occasionally directed towards elucidating the structure of such forms as *Isthmia enervis* and *Eupodiscus Argus*, and showing their agreement with the ordinary types; but without satisfactory results so long as only old means were employed.

Lately, with the help of Mr. Wenham's "Improved Reflex Illuminator," described in a paper read before this Society last May, and published in the June number of this Journal, the markings of *Eupodiscus Argus* have been so clearly displayed as to leave little doubt of the real structure, and those of *Isthmia enervis* have been exhibited so as to indicate that the aberrant character shown under ordinary illumination is the result of a peculiar arrangement of minute beads viewed under circumstances of optical confusion.

No thoughtful microscopist could be satisfied with the views *Eupodiscus Argus* obtained by ordinary means, and corresponding in general effect with the figure given in the 'Micrographic Dictionary,' pl. 12. In Smith's work on the Diatomaceæ a better figure was given, but far from being right, though at the time of its publication there was probably no particular reason known for doubting its correctness.

The figures in the 'Micrographic Dictionary' (2nd edition) would suggest to anyone familiar with the confusion arising from defective illumination and the superposition of transparent surfaces that it must be wrong. It represents a number of bright objects something like white stars with short rays, on a dark ground of general confusion. In the text of the same work the genus Eupodiscus is described as having valves that "appear either distinctly areolar, the depressions being large: granular from their

being minute; or striated."* In the last edition of 'Pritchard's Infusoria' we are told that *Eupodiscus Argus* is "easily recognized by its irregular cellules and intervening puncta, which give the disk a clouded appearance very unlike the usual transparency of Diatomaceæ."

Until Mr. Wenham's researches settled the old disputes † as to whether the markings on ordinary diatoms were elevations or depressions such phraseology as the above might be admissible, but careful examination of the objects with the best optical means now at the command of the microscopist may be expected to banish such terms as "areolar," "cellules," &c., from the descriptions of diatoms; the so-called "cellules" being spherules of silica, and the "areolæ" (*little spaces*)—not at all corresponding with what is generally meant by such a term.

At the end of Mr. Wenham's paper on the new illuminator, he remarks that "in English-mounted slides of diatoms and insect scales, some are always found detached from the cover and on the slide, and are shown well with it; but such is not the case with Möller's and others: they raise their covers off the slides by a ring of cement, and in many of them, I have searched in vain for a solitary specimen that has become detached." In my specimen of Möller's type slide some diatoms were found that could be well exhibited by Mr. Wenham's plan, though not by the same action of his instrument as when dry slides are used. Among these were the *Eupodisci* placed at the corners of each series. This result was not, however, arrived at without a good deal of trouble and practice in using the apparatus, and microscopists who take it up for the first time must not be disappointed if they frequently fail in its employment until they have spent a good many evenings in testing its action at various points of rotation, with the sub-stage mirror throwing up the light, and at various angles.

When successfully employed upon *Eupodiscus Argus* a considerable portion of the circular valve will become clear and plain, and it will be seen that it is entirely composed of spherules of different sizes and varied aggregation. Radiating from a central portion, occupied by minute and closely-packed spherules, bands will be seen proceeding to the circumference, each one composed of minute spherules that appear in close contact under an $\frac{1}{8}$, with eye-pieces up to D of Ross' system. Between these bands are larger spherules, frequently, but by no means universally, arranged in *fours*, so that when seen with powers too low for their distinct separation they

* In the edition of the 'Mic. Dic.' now issuing in numbers, pl. 12, an additional figure is given of part of a valve of *E. Argus* a little nearer the mark, but far from being right.

† This was written, not anticipating Mr. Stewart's remarks in discussing this paper. The exact refractive power of various diatoms—for they seem to differ—should be ascertained.

appear to form patterns like the Gothic quatrefoils, and look like holes when the light passes straight through them.

To see this structure well the mirror must be inclined so that the light given by Mr. Wenham's apparatus all passes a little askew through the spherules. When, through refraction or reflexion, any light passes straight, or nearly straight, through the larger spherules, they look like holes through which a glare penetrates, and all minutiæ of structure disappear. A slight change in the angle of the mirror, or of the position of the Wenham illuminator effected by partial rotation, will leave the larger spherules in a quiet glow, as if they were incompletely transparent, while the smaller spherules of the radial bands glisten a pearly lustre. A $\frac{1}{5}$th of Beck's with a deep eye-piece is sufficient to show this structure, but an $\frac{1}{8}$th will do it better, and an excellent effect was obtained with Powell and Lealand's new pattern and dry front.

Good specimens of *Eupodiscus Argus* should be examined as uncovered objects both with Mr. Wenham's apparatus and with that devised for opaque objects under high powers by Professor Smith. The writer has hitherto had no opportunity of trying these plans.

With regard to *Isthmia enervis*, the 'Micrographic Dictionary' describes the valves as "without linear thickenings, uniformly covered with depressions," and the valves of the genus generally are spoken of as having "depressions so large as to produce a distinct reticular or cellular appearance when viewed by ordinary illumination." This last remark is quite correct, and the "distinctness" of the appearance has caused it to pass unquestioned.

In a last edition of 'Pritchard's Infusoria' the valves of *Isthmia enervis* are described as having "large, somewhat quadrate cellules," which corresponds with the appearances ordinarily seen. In the same work, at page 42, is a quotation from 'Silliman's Journal,' describing experiments of Professor Bailey, who viewed diatom valves with the microscope while they were being acted on with dilute hydrofluoric acid. "In Isthmia," he said, "the spots on the surface which appear like granular projections, are in reality thin portions of the shell, and under the action of the acid they soon become holes." It is obvious that the parts which dissolve first must either be the thinnest or so constructed as to present the greatest surface to the acid. Professor Bailey's observations must therefore guide us in deciding which of several appearances the valves can be made to assume under high powers and various modes of illumination.

The writer has not yet been fortunate enough to find specimens of Isthmia so situated as to show in the best manner with Mr. Wenham's illuminator, but with a fine $\frac{1}{8}$th objective and that apparatus the simple cellular or reticulated appearance disappears, and great numbers of minute spherules at different levels can be

discerned; and its structure is thus seen to conform to ordinary types.

In Plate XL., a portion of the valve of *Eupodiscus Argus* is shown as seen in the manner described. No figure of Isthmia is given, because, though enough was seen to justify the preceding remarks, some important points in the arrangement of the minute spherules were left in doubt.

IV.—*Notes on the Development of the Nervous System of the Annulosa.* By B. T. Lowne, M.R.C.S., F.L.S., Lecturer on Physiology at Middlesex Hospital Medical School.

(*Read before the* Royal Microscopical Society, *Nov.* 6, 1872.)

Plate XL. (Lower portion).

A year and a half ago (January 11, 1871) I had the honour of reading a paper before you "On the Anatomy of *Ascaris lumbricoides*," and I then ventured to express the opinion that the nervous system of the Invertebrata is developed, like that of the Vertebrata, from the serous layer of the blastoderm. I had at that time set myself the task of working out the subject, but time has not sufficed to do what I had wished, and as a year or more may yet elapse before I have worked out this interesting matter, I have been induced to give you a *résumé* of what is at present known on this head, and to furnish you with the principal facts which have led me to believe that such is really the case.

In June, 1871, Dr. E. Metschnikoff published an essay in Kölliker's 'Zeitschrift' on the development of scorpions, which has confirmed my opinion, as his observations are a connecting link between my own as yet imperfect researches. This essay changed what was little more than suspicion in my mind into a strong belief.

DESCRIPTION OF PLATE XL. (Lower portion).

Fig. 1.—The ventral surface of an embryonic scorpion, showing the chain of ganglia, *a a*, on either side of the primitive furrow, one being situated at the base of each appendage. After Metschnikoff.

„ 2.—A transverse section through one of the ganglia and lateral appendages of a younger embryo. *a*, ganglion in the epiblast; *b*, rudiment of a leg outside the ganglion; *c*, mesoblast; *d*, hypoblast. After Metschnikoff.

„ 3.—The extremity of the abdomen of a scorpion embryo, showing the splitting of the mesoblast to form the body cavity. *a'*, epiblast; *c*, mesoblast; *d*, hypoblast; *e*, body cavity. After Metschnikoff.

„ 4.—A section of the entothoracic epiphysis of the mesothorax of a fly, showing its relation to the nerve ganglia. *a*, process of integument; *b*, nerve centre bound down to the thorax; *c*, nerves passing from it.

I cannot do better than give you a brief account of what Dr. Metschnikoff has done, before passing to the more general consideration of the subject which I have in view.

According to the observations of that author, the first trace of the embryo scorpion is a longitudinal groove in the blastoderm: this groove afterwards disappears for a time, but subsequently becomes visible again as a deep groove, on either side of which the nerve ganglia of the ventral chain appear as thickenings of the external layer of the blastoderm. Nerve fibres are next seen penetrating the median layer, or mesoblast. The mesoblast splits, as in vertebrates, to form the body cavity, its inner layer becoming incorporated with the inner or visceral layer to form the alimentary canal.

In connection with these observations, the remarkable internal skin growths which support the principal nerve centres of insects and crustaceans are of the highest significance; as it is quite clear that they are the highly-developed remains of the primitive fold in which the nervous system is formed. Fig. 4 is a section of one from the thorax of the fly, and it shows the close relation of the nervous and cutaneous systems.

One of the most striking circumstances in the development of the nervous system is the bifurcation of the neural groove and chain at its anterior or cephalic end, so that it embraces the mouth and pharynx in a kind of fork, the anterior extremity of which gives origin to the great præsophageal or prestomal ganglia.

The ganglia of the Articulata are chiefly developed at the bases of the lateral appendages; the rudimentary legs, antennæ, &c., may be conveniently divided into prestomal and poststomal ganglia. The prestomal nerve centres, like the brain of the vertebrate embryo, make a hæmal curve, which is most decided in the most highly developed forms, both in the Crustacea and the Insecta. The hæmal curvature of these nerve centres was pointed out by Professor Huxley as early as 1857 in the following terms:—"It is exceedingly interesting to remark the correspondence between the embryonic structure of the head of Mysis (and, I may add, of the other Articulata) and that of the vertebrate embryo. The procephalic processes resemble in a remarkable manner the lateral cranial processes of the vertebrate embryo; and the cephalic flexure of the crustacean, or insect, has its analogue, if not its homologue, in the cranio-facial inflection of the higher Vertebrata."*

Dr. Weissmann, in his 'Embryology of the Diptera,' has described the same primitive ventral groove in the early embryo of the fly, and my own observations on the development of the blow-fly not only confirm Dr. Weissmann's, but they go far to establish the fact that in the highest and most modified forms of the Insecta the development of the nervous system takes place in the serous layer

* Lectures on Gen. Zool., 'Med. Times and Gaz.,' 1857, p. 638.

of the blastoderm. As yet a complete series of investigations are wanting, but I have traced the steps of development sufficiently to allow me to state that the great procephalic lobes which exist in the half-developed embryo become folded inwards, and lie one on either side of the alimentary canal during the whole period of larval life, and that the nervous system of the larva, by the modification of which that of the imago is developed, is formed as a thickening of that portion of them which originally bounds the primitive fork at the anterior extremity of the ventral groove. These involuted procephalic lobes—and they are nothing else—form a portion of the imaginal disks of Weissmann, whilst the eyes, antennæ, and mouth-organs, are ultimately developed from cellular outgrowths at the bases of the same structures, just as they are in the Crustacea.

The nerve centres of the higher insects, such as the blow-fly and the higher social Hymenoptera, are even more remarkably like those of the Vertebrata, for a pair of cerebroid ganglia are developed in them in the most anterior portion of the procephalic lobes. These cerebroid ganglia were first pointed out in the Hymenoptera by Dujardin.* The ganglia in question are pedunculated convoluted nerve centres, situated at the anterior and upper part of the prestomal nerve centres; they are united by a well-marked commissure, never give off nerves directly, and are further connected with the great ganglia of sensation by peduncles, just as the cerebrum is in the Vertebrata.

At present I have not, it is true, succeeded in tracing the development of these cerebroid ganglia, but do not doubt I shall ultimately do so. From their position it is not difficult to see that they must originate from the anterior (in after-development posterior by flexion) portion of the procephalic lobes.

Having thus pointed out to you very briefly the present condition of research as to the relation of the nervous system of the Arthropoda and Vertebrata, I will draw your attention to the relation of the nervous system of the higher Annulosa to that of the Annelida and Annuloida generally.

The nervous system of the Echinodermata will serve well as a starting point. It consists, as you well know, of a band of nerve tissue surrounding the pharynx, and composed of nerve fibres with nerve cells scattered amongst them. From this circular ganglion band radiating ganglion bands (usually five in number), having the same structure, pass with the ambulacral vessels towards the antambulacral, or, as it is sometimes called, dorsal region, that is, towards the anal pole of the animal.

If I may be allowed to speculate on this subject, it seems to me probable that the primitive nerve ring is connected with that involution of the dorsal integument of the embryo from which the ambulacral system is developed, and is hence derived from an external

* 'Ann. Sc. Nat.,' Series iii., T. xviii., p. 231.

layer of the embryo. I throw this out, however, as a mere suggestion.

The next type of nervous system to which I would draw your attention is common amongst the Scolecida, especially those which, like the Nematoids and Gordiaceæ, bear many resemblances to the Echinodermata. The nervous system consists of a ring of nerve tissue around the pharynx, histologically like that of an Echinoderm, and closely related to the cellular layer of the integument, of which I believe it to be a differentiation. From this two or more bands of similar structure pass, according to my own observations,* to the anal pole of the body. This seems to be the case in the Ascaris at least, where I have traced short rudimentary intermediate bands passing back from the oral ring between the main nerve bands; these apparently represent the three missing radiating nerves of the typical Echinoderm. Some forms, as Mermis, have three instead of two well-developed nerves passing back from the œsophageal ring.

In the earth-worm, according to the elaborate researches of E. Claparède (whose much-lamented death is one of the greatest losses zoology has met for years), the structure of the nervous system follows a precisely similar type. In this case, however, only two bands pass back from the œsophageal nerve ring, and these are closely united and lie in the middle line on the ventral aspect of the body. Histologically, their structure is nearly like that of the Echinodermata, but the nerve cells show a tendency to become aggregated in places; this is the first indication of the formation of distinct ganglia.

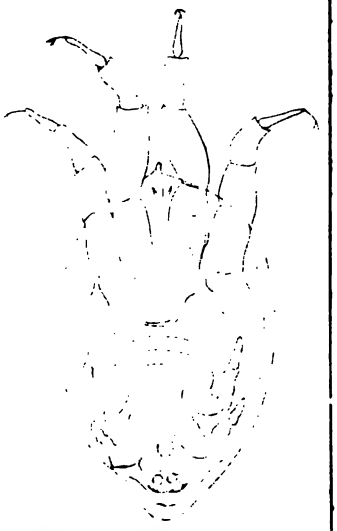
The transition from this condition to that observed in the higher Annelida, Insecta, and Crustacea, is not difficult. The nerve cells become collected in separate groups, or ganglia, and the remaining portions of the cords form the longitudinal commissures between them. The nervous system also becomes more central by the folding inward of the serous layer of the blastoderm to form the internal skin growths already mentioned. Thus we arrive at the highest forms, and we trace connecting links between the whole great series of organisms grouped under the sub-kingdoms Annuloida and Annulosa.

Before concluding, I cannot help referring to the visceral system of nerves so well marked in many Articulates, which is clearly developed from the mesoblast, as the sympathetic system is in the Vertebrata. I would draw attention to the great similarity of the complex visceral plexuses and the chain of visceral ganglia in the two great sub-kingdoms. These latter are undoubtedly differently placed, but their development from the mesoblast is a strong indication that they are to some extent homologous as well as analogous systems.

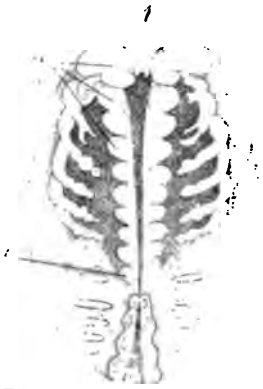
* 'Monthly Microscopical Journal,' January, 1871.

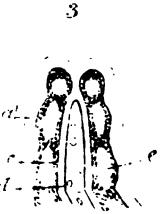
Acarellus pulicis.

Acarellus muscæ.



Portion of Valve of Eupodiscus Argus





Development of Nervous System in Arthropoda

W. Ross & Co. Ltd.

V.—*Notes on New Acarelli.* By J. G. Tatem.

Plate XL. (Upper portion).

In exhibiting balsam-mounted slides of new species of Acari, I wish to call the attention of the members to a fact of great significance in the life history of the creatures, and one, too, of much value generically considered. We all know that the young of the Acarine division of the Arachnid family are excluded from the ovum in an immature condition, possessing but three pairs of legs, though they acquire the normal four pairs after one or more subsequent moults. The common harvest bug, of which the adult form has yet to be recognized, is but a too familiar example of this hexapod stage of Acarine existence. The subjects I have embalmed have, however, entered upon the mite world in a still more imperfect state—they have but two pairs of legs!—but to prove that they would ultimately be as capable as any of their congeners of exercising four pairs, they have two posterior pairs neatly packed up in their trunks ready for evolution in the progress of growth!

Two years since, at the July meeting of the Entomological Society, Professor Westwood gave descriptions of two species of minute four-legged Acari, the one detected in the unopened buds of black currant trees, the other in pustules on the leaves of pear trees, referring also to a third which had been previously found in France. He considers them a distinct tribe, and that they must at all events be placed in a separate genus, for which he proposed the name of *Acarellus*. To this group our specimens unquestionably belong, and although their obviously imperfect development more than justifies the suspicion that the genus is based on immature forms alone, it still constitutes a good and natural division of the family.

In that example to which I have ventured to assign the specific name of *A. pulicis*, Plate XL. (its habitat having been the abdominal cavity of a dead flea), the hinder pairs of limbs are almost perfectly and completely formed, but still included within the cephalo-thorax, evidently awaiting the nearly approaching moult for extrication; while in the other, which on like grounds I have called *A. muscæ*, Plate XL. (detected clinging tenaciously in company with half-a-dozen others to the thorax of a small dipteron), the hinder pairs are more rudimentary, and with its much smaller size indicates an earlier stage of existence and more remote ecdysis. In both instances, however, it is quite apparent that in the process of development they would sooner or later have transferred themselves from the new genus *Acarellus* to that of *Acarus* proper.

Of their life histories I have no knowledge. Discovered by accident, and not until after many hours of maceration in liq. potassæ,

I have yet to gain the opportunity of observing them in life; and though, as I suspect, the species may be numerous and examples abundant, minuteness ($\frac{1}{750}$ to $\frac{1}{500}$) and parasitism will oppose so many obstacles to the inquiry, that by vigilant and patient research alone may we hope to gain any further acquaintance with them.

VI.—*On a Proposed Standard Micrometer Eye-piece, or Eidometer; and on a uniform and easily applied Method of Naming Microscopic Objectives.* By GEORGE FINDLEY.

WITH a proper micrometer eye-piece, nothing can be easier than to *see* the exact magnifying power of any microscopic objective * at any interval of object and image, and of naming it accordingly.

I have had one of my own adapting in use for over six months, calculated according to the usual formula for eye-pieces, $\frac{f \times e}{f + e - d}$; where f is the focal length of the field-lens; e, that of the eye-lens; and d is the distance between them. I think it would help to settle some of the disputes among microscopists as to the solar focal length of objectives. Some of the uses it may be put to are the following:—

1. Place on the stage of the microscope a micrometer ruled to $\frac{1}{100}$ths and $\frac{1}{1000}$ths of an inch, and the eye-piece shows at sight how many $\frac{1}{100}$ths or $\frac{1}{1000}$ths in the eye-piece micrometer are covered by the image of $\frac{1}{100}$th or $\frac{1}{1000}$th on the stage micrometer. The size of the object $\frac{1}{100}$th or $\frac{1}{1000}$th is known; the objective makes a magnified image of it, which is measured by the eye-piece micrometer. The ratio of the size of the *seen and measured magnified image*, to the size of the known unmagnified object, gives the magnifying power of the objective. If there be a draw-tube, there will be shown the increase of size given by the increase of length of tube.

2. My eye-piece of itself magnifies the image just 10 times, making the total magnifying power with it, just 10 times that of the objective alone.

3. It furnishes us with the means of knowing the magnifying power of any other eye-piece.

4. It enables us to tell the *real size* of the original object.

5. It enables us to calculate the principal focus of the objective, from the seen magnifying power and the interval of object and image, *i. e.* the distance of the conjugate foci from each other.

6. Its power to magnify 10 times may be shown to the eye.

7. It renders practicable the method of naming objectives according to their *seen magnifying* power at a standard distance, say

* Simple or compound, dry or wet, object covered or uncovered.

10 inches,* of object and image, or of the two micrometers. If an objective is seen to magnify at the standard distance 5, 10, or 20 times, call it objective No. 5, 10, or 20, or O_5, O_{10}, O_{20}.

8. It also renders practicable the method of naming eye-pieces according to their real magnifying power. Thus, if the micrometer eye-piece makes $\frac{1}{10}$th or $\frac{10}{100}$ths in the eye-piece micrometer cover 1 inch on a rule at 10 inches from the naked eye, call it eye-piece No. 10, or E_{10}. Then since in the compound microscope the whole power (M) is the power of the objective (O) × the power of the eye-piece (E), we have

$$O \times E = M,$$
$$O = \frac{M}{E},$$
$$E = \frac{M}{O},$$

or

$$O_5 \times E_{10} = M\ 50,$$
$$O_{10} \times E_{10} = M\ 100,$$
$$O_{10} \times E_{20} = M\ 200,$$
$$O_{40} \times E_{10} = M\ 400,$$
$$O_{40} \times E_{20} = M\ 800, \&c., \&c.$$

Any two of these three (O, E, M) being given, the third is easily found.

9. With this eye-piece there need be no disputes as to whether a certain objective be $\frac{1}{8}$th or $\frac{1}{10}$th, or whether a $\frac{1}{18}$th be or be not $\frac{1}{19}$th, since it shows us how much it magnifies; and here "seeing is believing."

It would also save Dr. Royston-Pigott *and others* the trouble of taking the glasses out of their eye-pieces; placing a micrometer in the empty eye-piece and examining it with a 1-inch lens;† and also Dr. J. J. Woodward the trouble of having to use a dark room, a screen, and direct sunlight, to get the magnifying power of his objectives, in order to calculate their solar or principal focal lengths.‡

I do not pretend to a totally new invention; but only to a new adaptation of an already existing instrument whose many uses in this direction seem to have been lost sight of.

If not out of place, I should like to say that though not indebted to Dr. Royston-Pigott for my eye-piece (which grew out of my own wants), I am very much indebted for various useful hints scattered

* I prefer 10 inches; and one reason is that, *at* 10 *inches*, the real magnifying power is just 2 (and a fraction which may almost always be neglected) less than the nominal magnifying power.

Focus	1 in.,	Nominal power	10 times,	Real power	8 times nearly.		
"	$\frac{1}{2}$ "	"	20 "	"	18	"	
"	$\frac{1}{3}$ "	"	30 "	"	28	"	
"	$\frac{1}{10}$ "	"	100 "	"	98	"	

and conversely the seen real power + 2 = nominal (within the before-mentioned fraction).

† 'Quart. Jour. Mic. Sci.,' July 1, 1872, p. 270.

‡ 'Mon. Mic. Jour.,' Aug. 1, 1872, p. 72.

through those published articles of his which I have seen. He teaches much, and suggests more.

The use of this eye-piece is not confined to the microscope. It will measure the size of the image in the focus of the objective of the telescope; also the breadth of the pencil emergent through any telescopic eye-piece; and so lead to the magnifying power of the telescope.

It also measures the distance apart of the Fraünhofer lines in the spectroscope; and, of course, would show the relative dispersive power of any prism or combination of prisms.

VII.—*On a Method of ascertaining Magnifying Power and Minute Magnitudes by Miniatures.* By Dr. Royston-Pigott, M.A., M.D. Cantab., M.R.C.P., F.C.P.S., F.R.A.S., M.R.I., F.R.M.S., late Fellow of St. Peter's College, Cambridge.

That quaint old writer Dr. Kitchener says:—"An easy way of judging of the relative magnifying powers of various eye-tubes and lenses is to hold them up about 12 or 18 inches from the eye, and three or four feet from a window—that in which the panes of glass appear least, magnifies most: or place two candles nearly together and hold the lenses about a foot from them, that in which the candles appear nearest together magnifies most."

Upon this principle I now propose the following simple method of ascertaining the magnifying power of an object-glass, which *I call the miniature method.*

Place the object-glass in the position of a condenser. At exactly ten inches from the stage adjust a scale of inches and tenths. Bring the image of the scale upon a ground-glass slide, with its ground surface downwards. The ground glass now shows a miniature of the scale. Now replace it by a micrometer slide, and viewing it *with a low power,** readjust the condensing object-glass so as to bring the image of the scale into a focus accurately coincident with the divisions of the stage micrometer. The actual size of the miniature scale can be read off upon the stage micrometer, which immediately gives the amount of diminution, and therefore of the enlargement.

Ex. (1). A micrometer on stage showed a tenth on scale miniatured by an object-glass to be 19 thousandths. The magnifying power therefore

$$= \frac{1}{10} \div \frac{19}{1000}$$
$$= 100 \div 19 = 5{\cdot}26.$$

* A power of 100 is convenient.

If now this object-glass be employed at 10 inches, with an eye-piece C magnifying 10 times, the magnifying power will be 10 times increased, or 52·6.

Ex. (2). Suppose it be required to find the size of an image formed at 15 inches instead of 10, by a lens nearly $\frac{1}{20}$th of an inch focal length, which magnifies 200 times at 10 inches.

Then first the real focal length (see footnote) $f = \frac{d}{m+2}$

$$= \frac{10}{200+2} = \frac{5}{101}.$$

Then the magnifying power at 15 inches will be

$$15 \text{ divided by } \frac{5}{101} \text{ less } 2 = \frac{15 \times 101}{5} - 2$$
$$= 301 - 2$$
$$= 299.$$

---

* A question often arises such as the following:—If a given lens magnifies 20 times at 5 inches, what would it magnify at 10? Now this will not be just double. The law of images is somewhat complicated, and I find that if (d) the distance between the image and object (say 10 inches) be divided by the focal length (f), it will for *a single small lens* give the magnifying power m too great by 2. But for a lens of considerable focal length, such as $\frac{1}{2}$ inch, m = new distance divided by focal length less 2, to be further diminished by reciprocal of this result, which will afford a closer approximate value. Those who desire to see the solution of the problem will be able to follow the following equation, given by the writer, 'Phil. Trans.,' II., 1870 (p. 594):—

$$m + 2 + \frac{1}{m} = \frac{d}{f}, \left(\text{or nearly } m = \frac{d}{f} - 2\right) \qquad \text{(I.)}$$

$$m^2 + 2m + 1 = \frac{md}{f}$$

$$m^2 - m\left(\frac{d}{f} - 2\right) + 1 = 0.$$

Completing square,

$$m^2 - m\left(\frac{d}{f} - 2\right) + \frac{1}{4}\left(\frac{d}{f} - 2\right)^2 = \left(\frac{d}{f} - 2\right)^2 - 1.$$

Taking the root (positive sign),

$$m = \frac{1}{2}\left(\frac{d}{f} - 2\right) + \sqrt{\frac{1}{4}\left(\frac{d}{f} - 2\right)^2 - 1}. \qquad \text{(II.)}$$

The formula will give the magnifying power, in the same way for a distance $d = 20$ inches between object and image; for a focal length 1·8 as found in *Ex.* (3): which is a satisfactory application of the principle of verification:—

Thus, since $d = 20$ and f now $= 1{\cdot}8$,

$$m = \frac{1}{2}\left(\frac{20}{1{\cdot}8} - 2\right) + \sqrt{\frac{1}{2}\left(\frac{20}{1{\cdot}8} - 2\right)^2 - 1}$$

$$= \frac{16{\cdot}4}{3{\cdot}6} + \sqrt{\left(\frac{16{\cdot}4}{3{\cdot}6}\right)^2 - 1}$$

$$= \frac{16{\cdot}4}{3{\cdot}6} + \sqrt{\frac{256}{(3{\cdot}6)^2}} = \frac{16{\cdot}4}{3{\cdot}6} + \frac{16}{3{\cdot}6} = \frac{32{\cdot}4}{3{\cdot}6} = 9.$$

If the lens be nearly 2 inches focus, and magnifies 9 times at 20 inches, this method will not be sufficiently correct for so large a lens. In order to find its focal length, the distance of the "centre" of the lens from object and image is generally required; but my formula obviates this difficulty.

Ex. (3). To find the magnifying power at 36 inches of a supposed 2-inch lens, which magnifies 9 times at 20 inches distance between object and image:

First approximation, ('Phil. Tr.' *loc. cit.*)

$$m = \text{distance} \div \text{focal length less } 2 = \frac{d}{f} - 2.$$

But focal length (f), since $m + 2 + \frac{1}{m} = \frac{d}{f}$

$$= \text{distance divided by } \left(m + 2 + \frac{1}{m}\right)$$

$$= 20 \div \left(9 + 2 + \frac{1}{9}\right) = 20 \div \frac{99+1}{9}$$

$$= \frac{180}{100} = 1{\cdot}8 = f.$$

Then

$$m = \text{new distance} \div \text{focal length less 2 (approximately)}$$

$$= 36 \div 1{\cdot}8 \text{ less } 2 = \frac{36}{1{\cdot}8} - 2 = 20 - 2$$

$$= 18 \text{ nearly.}$$

This is slightly too much. The square root of 1 less than the square of $\frac{18}{2}$ or $\sqrt{\frac{1}{4}\left(\frac{d}{f} - 2\right)^2 - 1}$

$$= \sqrt{81 - 1} = \sqrt{80} = 8{\cdot}944;$$

therefore

$$m = \frac{1}{2}\,18 + \sqrt{80} = 17{\cdot}944, \text{ formula (II.) note.}$$

The magnifying power at 36 inches, when it magnifies 9 times at 20 inches, is $17{\cdot}944 = 18$ times nearly.

On estimating Minute Magnitudes.

When very great magnifying power is employed, as 4000 or 8000, two thousandths upon the stage micrometer cannot be seen, as the field of view in general cannot embrace two consecutive lines of the micrometer. In order to overcome this difficulty I devised the following method. A very coarse scale is drawn on glass with diamond lines, or photographic lines, and is mounted in a cap or tube that fits upon the under part of the sub-stage in a fixed distance; into the upper part a good half-inch objective is inserted,

and by the method already described a fine miniature of the lines is thrown upon the field of view.

Better still, I fix a parallel wire micrometer below, and throw the image of the wires upon the stage. The half inch, if the micrometer wires be 5 inches below the stage, will diminish it nearly 11 times; if $\frac{1}{4}$ be used the scale will be diminished nearly twenty times, and the wires will appear to be separated about the 20th part of the actual space, which, however, can be previously ascertained. The great superiority of this method consists in the absolute steadiness and the preservation of the light and definition. I have often found it quite hopeless to measure accurately minute bodies of difficult definition, whilst using the Jacksonian micrometer in the eye-piece, the intervening micrometer glass plates on which the divisions are drawn entirely obscuring and destroying a fine definition displayed at last, perhaps by very great pains. Further, the positive eye-piece micrometer, by the vibration it causes upon the least touch of the divided milled head, renders the distinct measurement a work of great difficulty. But by this method the micrometer lines can be reduced to any degree of fineness, and solid wires of any required diameter be substituted for the spider lines.

The advantages are these. In observing a delicate object we are frequently changing eye-pieces or objectives, altering the cover adjustment, or using water or dry lens; under these circumstances the final measurement of a delicate object, with great difficulty seen at all, requires to be done without any change whatever of the observing instrumentation. It is at that point that a delicate measurement is most desirable, when often the means fail us. But if we can apply a miniature micrometer imaged at once upon the field of view, the object is instantly obtained without disturbing the oculars or objectives.

Remark.—A covered moderator lamp, a white cloud, or diffused illumination, is necessary to define the miniature micrometer properly, and a half-inch or inch object-glass for condenser.

VIII.—*On a Method of estimating the Optical and Actual Thickness of Microscopic Covering Glass.* By G. W. Royston-Pigott, M.A., &c.

The importance of covering glass in microscopic research is daily becoming more evident: and having occasion to use a new 1–50th immersion of Powell and Lealand, the only one in existence, it became necessary to devise a means of readily measuring the

thickness of the cover of any mounted object I possessed. I therefore proceeded to find data for a ready estimation.

First of all it was necessary to become possessed of an accurate gauge. Two methods are then necessary: to solve the problem. Actual thickness of various specimens should be tabulated by the gauge (I write on them with a diamond). I can strongly recommend the Swiss gauge sold by Messrs. Ehnhuus and Co., Frith Street, Soho. The instrument in my possession readily measures, with two jaws and a dial-plate and index, to the $\frac{1}{1}$th of a thousand.

I find 16 divisions make seven thousandths, and a whole circle of 100 divisions, 45.

The thinnest glass in my possession measures $2\frac{1}{2}$ thousandths.

Having done this, the next step is to ascertain the interpretation of the divided head of the "fine screw adjustment": mine is divided into 20 parts, and by measurement the screw has 50 threads to the inch. By means of a micrometer microscope, applied to the Powell and Lealand stand, I ascertained that—

14 turns of milled head measured 65·25 divisions.
11 microscope micrometer divisions measured ·02 inch exactly.

By rule of three—

14	turns equal	·1186 inch.
1	turn equals	·0085* „
$\frac{1}{20}$th	of a turn equals	·0004 „
2	divisions equal	·0008 „
3	„ „	·0012 „
4	„ „	·0017 „
7	„ „	·0029 „
14	„ „	·0059 „
19	„ „	·0080 „

Now, in consequence of the lever of the fine adjustment acting in a curve tangentially, the values of the divisions on the milled head fine adjustment cannot be uniform.

To ascertain these variations I examined a thick cover optically:—

Readings ..	13 divisions	Milled head fully out zero.
	13·3 „	Median position.
	12·5 „	Fully home zero.
	12·5 „	At one turn out.
	13·0 „	At two turns out.

Conclusion; for the first six or seven turns of the milled head when fully out, the divisions may be considered as representing equal spaces in the axial line of fine adjustment.

I now selected three different covers, and very carefully mea-

* ·84740 is the exact result.

sured by the Swiss gauge their actual thickness; secondly, the optical thickness; and thirdly, the full thickness, reading both by the fine adjustment divisions. Tabulated results in the three cases:—

	Cover I.	Cover II.	Cover III.
Actual thickness	6″ (thousandths)	4″·6 ..	2″·2
Image reading, fine adjustment ..	9 divisions	6″·9 ..	3″·3
Full thickness, „	19 „	14 ..	7

The average reading by the table previously calculated would have been considerably at variance with these results. In my Swiss gauge the image reading is just half as much more than the thickness in thousandths or the actual thickness measured by the divisions in the fine adjustment at the rate of 3 divisions to 2 thousandths of *real* thickness.

Some interesting facts here appear: that the optical thickness measures the actual for the same kind of glass in every thickness of cover; and that the refractive index is so high in this kind of glass that the image rises considerably more than one-third of the thickness, as commonly stated loosely enough, on the assumption that the index of refraction is 1·500.

Sir John F. Herschel gives the following measures of the rays of *mean refraction* of different kinds of glass:—

Plate glass	1·500 to 1·540	For the line D in the Spectrum, or for mean rays.
Crown „	1·525 „ 1·563	
Flint „	1·576 „ 1·642	
Glass lead 3 / Flint .. 4	1·732	
Glass lead 2 / Sand .. 1	1·987	
Glass lead 3 / Flint .. 1	2·028	

I have frequently asked opticians who supplied me with the best glass covers, what was the nature of the glass, but could get no answer. I believe it is flint glass. Messrs. Chance and Co., of Birmingham, could tell us what its composition is, and give us perhaps information as to its refractive index.

NEW BOOKS, WITH SHORT NOTICES.

Manual of Human and Comparative Histology. Edited by S. Stricker, &c. Translated by Henry Power, M.B. Lond., F.R.C.S., &c. Vol. II. The New Sydenham Society. London, 1872.—The second volume of this work has made its appearance in England during the past few months, and we are promised the third volume in a comparatively short period. It is a peculiar work, having its several articles written by different authors, but for this reason it is eminently an excellent and yet it is also an unequal book. There is apparently a want of Editorial guidance, and it rather looks as though Herr Stricker allowed each individual author to write just as much or just as little as he pleased, and upon whatever plan seemed most desirable to him. The consequence of this is inequality of articles and absence of any definite scheme. For instance, some papers are decidedly too short, while others appear to occupy too long a space; some are accompanied by a copious German bibliography, while others have nothing in the form of a list of authorities. Again we must say that we consider that English work has been almost completely overlooked; of course such articles as those of Allen Thomson, Lionel Beale, Huxley, L. Clarke, and a few others are referred to, but "what are they among so many" German authorities? And, indeed, the only writer who has attempted a full list of authors is Herr Waldeyer; yet from his list of German workers and a few French and English names one would naturally be led to believe that Deutschland was essentially the country where the microscope flourished in the hands of the human anatomist. This is of course objectionable, and it is the more so from the fact that it is an evil of very old growth, and unfortunately it leads most readers to fancy that microscopical anatomy is, so to speak, a dead science in this country, which is *par excellence* the parent of the microscope. Of course Mr. Power bears no blame for this, it is solely the authors of the papers themselves, whose ignorance of English work is very much to be regretted. The Editor has done his duty, as is his custom, with patience and skill, and has rendered into terse and pleasing English what in some instances must have been horribly long-winded German phraseology. On the whole we have every reason to be well satisfied with the volume, for it brings home to us in a condensed form the various opinions of men who are not mere compilers but actual workers at the particular subjects on which they have undertaken to give us their ideas.

We cannot possibly do more than allude very briefly to some of the voluminous contents, leaving to our readers the more pleasant task of devoting themselves fully to any particular subject which may interest them. The paper on the liver, which opens the volume, and is from the pen of Professor Ewald Hering, of Vienna, is in our opinion the least valuable in the volume. This appears to us from its brevity, from its comparative novelty, and from the fact of the author's negligence (in print at least) of existing workers; for the few he

has mentioned in the footnotes constitute but a tithe of those who have worked at this important subject. It seems to us that of all opinions on this subject, those of Dr. Handfield Jones, F.R.S, in his splendid memoir in the 'Phil. Trans.,' are the most worthy of belief; yet Dr. Hering does not appear to be acquainted with it. Dr. Beale's opinions too are dismissed in a word or two. This is essentially unsatisfactory.

The lungs, which are written upon by Franz Eilhard Schultze, are better done. The author has gone more carefully into his subject, and his opinions do not startle us so much as Hering's do. Still we have the fault to find with his diagrams which must be experienced by every genuine student, *viz.* that they are too picture-like. We should venture to say that not one-tenth of them are actual copies of the specimens. In this we think foreign authors are much to blame. In a drawing of Beale's, or Huxley's, or any other English worker, we are introduced to exactly the specimen which was seen beneath the microscope. But most assuredly no student who is familiar with human microscopic anatomy will take the drawings which illustrate the greater part of this book as accurate representations of the object. We think too much cannot be said against this habit, for whenever a detail not present in the object is introduced into the picture, it is almost invariably something which bears on the author's opinions. We therefore object most strongly to such figures as being anything but semi-diagramatic. We observe also in regard to this article that almost the only English author mentioned is Dr. Williams, who wrote the paper in Todd's Cyclopædia so many years since.

The section devoted to the structure of the ovary and parovarium is a very good one. Herr W. Waldeyer has given us an able *résumé* of the views of Henle, His, Klebs, Pflüger, and others. He has entered, too, pretty largely into the comparative anatomy of the subject so far as it bears upon views of the human structure, and his style is clear and to the point. Indeed, we may say that this is one of the best articles in the volume. Among other points of interest is the additional weight which the author gives to Pflüger's view with regard to the share taken by the follicular epithelium in the formation of the *corpus luteum*; he being of opinion, contrary to Kölliker, that "both elements of the follicular wall contribute to its formation." Another important feature in this paper is the very elaborate bibliography which the author appends, which, although it partakes less or more of the character before referred to, nevertheless covers more than eight pages.

Few of the other articles have very much import till we come to those upon the spinal chord by Herr J. Gerlach, and the brain of mammals by Herr Theodor Meynert. These two papers, by very eminent authorities, constitute considerably more than one-third and very nearly one-half of the entire volume. They are papers that redeem the character of the work, which would sink to comparative insignificance without them. And of these two, unquestionably that upon the brain is the best and most exact in the information it contains, and is most valuable not alone for the knowledge it sets forth, but for the excellent bibliographical list which it has appended to it. This,

which covers more than five pages, is arranged in sections corresponding to the following parts: brain as a whole; cerebrum, cerebellum, cerebral ganglia; Crus Cerebri, Pons Varolii, and medulla oblongata. In the first chapter—that devoted to the brain—the lecturer on anatomy will find all that is really known of the structure of the brain, and of the origin of the cerebral nerves traced much further than they are given in our standard works on Anatomy and Physiology. He will observe too that many novel points are suggested in regard to those marvellous organs, which even yet are a puzzle to the most patient student, though infinitely less so than they have been even of late years. In regard to the illustrations, we can only award them the very high praise which they certainly merit. They have less of the Artist and more of the Anatomist about them than any which the work contains, and they have the further merits of being both numerous and novel. We think the author has done well to mention, as he does with praise, the names of his draughtsmen, Herr Dr. Carl Heitzmann, and Herr Adolf Göhre, of Vienna, whose labours most assuredly merit the greatest commendation.

The book as a whole is a very satisfactory *résumé* of the latest research as it is taught in the best schools of Germany, and it is one which we welcome accordingly in the most thorough fashion. We hope soon to have the third volume on our table, and meanwhile our best thanks are due to Mr. Power for the very lucid text which he has given us, and to the publishers—whoever they may be—who have turned out a handsome and creditable volume.

A. E.

PROGRESS OF MICROSCOPICAL SCIENCE.

Pathological Microscopic Specimens.—The 'British Medical Journal' states that a number of the above were exhibited at the meeting of the Association. Dr. J. Batty Tuke exhibited microscopic sections illustrating miliary sclerosis of the brain, colloid degeneration, fuscous degeneration of brain cells, &c. Dr. Herbert C. Mayo exhibited microscopical sections of brain structure in health and disease; and Dr. J. Thompson Dickson exhibited some sections illustrating general paralysis, explaining that the case was one in which no mental symptoms had occurred.

The Embryology of Terebratulina.—Mr. Edward Morse, who is known to be an authority on this group of animals, has just published in 'Silliman's Journal' (October, 1872), a preliminary account of the above, the full account being reserved for the Boston Society of Natural History. For several years past the author has made a special study of the Brachiopoda. The publication of the results of these investigations has been purposely delayed, till he could incontestably demonstrate the genital nature of the Cuvierian hearts, so plainly

shown to be oviducts by Hancock and Huxley, and till something at least could be given of the embryology of some brachiopod. For these two matters he has visited Eastport, Maine, for the third time, and now his heretofore fruitless endeavours have met with success. He had before seen the ciliary lining of the oviducts in Lingula and Terebratulina, but he wished to see the eggs in their actual passage through the tubes. This he has now repeatedly observed in Terebratulina. The eggs were soon discharged from the sinuses in the pallial membrane, afterwards floating freely in the perivisceral cavity; the eggs were then seen gathered at the trumpet-shaped mouth of the oviduct, and have been watched as they were slowly passing through the tube, and have been caught as they were discharged at the external orifice. These eggs have then been followed in their development until they assumed the form of a deeply-annulated embryo, composed of four distinct rings, which had a marked vermian contraction upon each other. At this stage they appeared to be attaching themselves by the caudal segment. During the latter part of this examination his embryos were unfortunately lost. He had not the necessary appliances to keep the water at the frigid temperature they were accustomed to, and the increased temperature of the water led to a rapid development of Paramæcia, and other infusoria, and his embryos were ruthlessly eaten up. He has, however, nearly three hundred outlines of the embryos during their development. Next year it is hoped a complete history of their development will be made, as many things have been observed in their proper management of which he hopes to profit in his next attempt. There were also discovered prominent glands at the external openings of the oviducts in Terebratulina, which he has every reason to believe represent the testes. These glands surrounded the external orifice of the oviducts, which protruded somewhat from the anterior walls of the body, and the glands were invariably found filled with spermatozoa. From Eastport, Maine, he hurried to the St. Lawrence, with the hopes of securing some data regarding the embryology or early stages of another brachiopod found there, *Rhynchonella psittacea*. He was altogether too late for this, but had the opportunity of studying *Rhynchonella alive*, to note the ciliary action in the oviducts driving currents outward, and to establish the correctness of Owen's supposition that the arms of Rhynchonella can be protruded. A jar of specimens dredged by Dr. P. P. Carpenter, who had accompanied him from Montreal, was left standing undisturbed for twenty-four hours, when one of the specimens protruded its arms their entire length from the partially opened shells. He poured the sea water carefully out, and suddenly poured in the strongest alcohol, and the specimen is now preserved in this exerted position. In a forthcoming memoir of the Boston Society of Natural History all the details of these examinations will be given.

Some New Species of Graptolites have been described by Mr. John Hopkinson, F.R.M.S., in the 'Geological Magazine' for November, 1872. The subject may interest some of our Fellows, but the paper is impossible to abstract in any reasonable space.

Tornaria is the young condition of Balanoglossus.—Professor A. Agassiz, writing to the 'American Naturalist,' for November, states that the "development of Tornaria has at last been solved. As is well-known, Müller, Krohn, Fitz Müller, and myself have considered it a star-fish embryo. The analogy between a Brachiolaria and Tornaria seems complete, and no one questioned the position of the latter till Metznikoff in 1870 was fortunate enough to raise Tornaria to a later phase of development—to his astonishment it changed into an annelid. Of course, in view of the affinities (first suggested by Huxley) of the worms and echinoderms, it was most important that Metznikoff's observations should be repeated, and if possible the genus of annelids, of which Tornaria was the young, accurately ascertained. The annelid raised by Metznikoff was most peculiar, and, in absence of other evidence, he suggested the possibility of its being a young Balanoglossus. I have been able this summer to raise Tornaria and to obtain young annelids somewhat older than those observed by Metznikoff, tracing at the same time the development of the branchiæ as diverticula from the œsophagus, and also to find the young annelid of Tornaria a species of Balanoglossus (of which the adult is quite common at low-water mark at Newport and at Beverly, Mass.), but slightly older than those raised directly from the Tornaria stage. The details of this interesting embryology will shortly be published."

NOTES AND MEMORANDA.

How to Measure Angular Aperture.—The following plan, whose novelty is almost questionable, is given in a recent number of the 'American Naturalist':—For those students whose stands have not a graduated rotating base, Professor T. D. Biscoe recommends a plan especially applicable to the larger form of achromatic condensers which cannot be as conveniently arranged for the method advised by Dr. Carpenter. He places the combination horizontally, with a gas flame several feet distant behind it to furnish nearly parallel light, holds a card across the centre of the front lens so as to bisect the cone of light, and with a fine pencil marks the edges of the illuminated portion of the card. A common protractor is used to measure the angle of the lines thus drawn. Though chiefly useful to measure the eye-piece form of achromatic condensers (Webster's condensers, &c.) as actually used with diaphragm, &c., in position, the method may be sometimes convenient for objectives of low power. Professor Biscoe believes it can be applied to objectives as high as ¼ inch of 110° and be reliable within one or two degrees.

A New Erecting Prism.—The 'Journal of the Franklin Institute' (U.S.A.) states that Mr. Joseph Zentmayer exhibited at a meeting of the Franklin Institute a single prism which erects the image com-

pletely and in such a way that the incident and emerging rays are parallel, which, as far as we know, was never accomplished before. In connection with the microscope, as it was shown, it interfered very little with the definition, and, although the light is twice refracted and reflected, the loss of light is much less than one would expect. With the microscope, the prism is placed right above the objective, and the instrument may be used in any inclined position. A pair of such prisms might be used also for an erecting binocular microscope, of which the two bodies have the same inclination to the stage. The projection of the front is a square, that of the profile an isosceles triangle. The angles at the base of the triangle are 27°·19 for crown glass of a refracting index of 1·53, in order to obtain the greatest aperture combined with the smallest prism.

Logwood Staining of Tissues.—I wish now, says Dr. Arnold, in the 'Lens,' to describe a process that seems to do away with the two objections raised against carmine. This method I have made use of for nearly two years, and the excellent results it gives can be vouched for not only by myself, but by many students who have worked under my directions. In fact, it has taken the place of carmine in my laboratory to a considerable extent. The colouring matter of logwood (*Hæmatoxylon Campechianum*) is one of the most valuable staining materials known to me. This substance was, I believe, first used by C. F. Müller, in his investigations on the cornea. The method which is recommended by Müller can be found in the works devoted to microtechnology. I wish to give merely the manner in which the staining solution is made and used in my own laboratory. The ordinary logwood extract is finely pulverized in a mortar and about three times its bulk of alum (in powder) added; the two ingredients are well rubbed up together and mixed with a small quantity of distilled water. The *complete admixture* of the alum and hæmatoxylin is necessary, and this will require fifteen to twenty minutes' vigorous stirring. More water may now be poured on, and the solution, after filtration, should present a clear, somewhat dark-violet colour. If a dirty red is obtained, more alum must be incorporated, and the mixture again filtered. By always having an excess of both alum and hæmatoxylin in the mortar, a saturated solution can be obtained, which, after filtration, may be combined with alcohol,—one ounce of the logwood fluid with two drachms of seventy-five per cent. alcohol. I have found that a much better colour can be had by allowing the mixture, after thorough trituration, to stand for several days before filtering and adding the alcohol. Should a scum form on the surface of the liquid after it has been some time made, a few drops of alcohol and careful filtering will be all that is required. With a strong solution, such as has been described, the colouring is very rapid, requiring but a few minutes; whereas if a slower tinting be desired, the fluid may be diluted with a mixture of one part alcohol and three parts water. Whether the tissue has been previously hardened in alcohol or any of the chrome compounds, it is coloured equally well,—the nucleus of a most brilliant purple, the cell-body of a distinct neutral tint. An overstaining causes an almost perfect blackening of the nucleus, while the pro-

toplasm becomes purple. In many instances where carmine has given but indifferent results, hæmatoxylin will be found to succeed most admirably. As to the stability of the colouring, I need only remark that specimens tinted and mounted in December, 1869, show as perfectly as when first put up.

Nitrate of Silver in the Examination of Tissues.—Col. Dr. Woodward states that a solution of one part of chemically-pure nitrate of silver in four hundred parts of distilled water (by weight) is perhaps most generally useful, though stronger solutions may be needed for special cases. The bottle should be covered with yellow paper to exclude the light. For the investigation of the serous membranes and other large epithelial surfaces, it is merely necessary to pour a little of the solution upon the piece selected for examination, or to brush the surface lightly with a camel's-hair pencil dipped in the reagent. The piece is then thoroughly washed with distilled water and exposed to the light, preferably to the direct rays of the sun. To obtain the best effects, the tissue should be taken just after death, or at least should be as fresh as possible. It should be laid in a little distilled water, in pure glycerine, or in alcohol, during its exposure to the sunlight. The result may be regarded as satisfactory if, after a few minutes' exposure, the boundaries of the cells are everywhere mapped out with delicate black lines without other visible black deposits, either on the surface or in the substance of the tissue. If the same solution is injected into the blood-vessels or the lymphatic trunks, a similar effect is produced, provided the parts are sufficiently thin and transparent to permit a prompt exposure to the light. The urinary bladder, mesentery, lungs, lymph-sacs, skin, and other parts of the frog offer excellent materials for this method. The blood-vessels, however, are apt to contract irregularly when thus injected just after death, and for the display of their endothelium it is perhaps best to employ the solution of gelatine and silver described by Dr. N. Chrzonszczewsky, of Charcow. Half an ounce of fine gelatine is dissolved in four ounces of distilled water, and to this is added a solution of one scruple of nitrate of silver in two fluid-drachms of distilled water. This fluid is to be injected warm, like any other injection-fluid containing gelatine. The syringe should be of silver, or of hard rubber with a silver nozzle. The precise quantity of gelatine must vary somewhat with its quality, the point being to use a fluid which will gelatinize handsomely when cold; the vessels are thus kept handsomely distended, and are consequently better suited for examination under the microscope.

CORRESPONDENCE.

"Using the ⅛th Binocularly."

To the Editor of the 'Monthly Microscopical Journal.'

Padnal Hall, Chadwell, Essex, *Nov.* 6, 1872.

Dear Sir,—Permit me to state that there is no foundation for the assertion contained in the letter by Mr. S. Smith, on page 247 of the last 'Monthly Microscopical Journal,' that I am engaged in perfecting a Binocular Microscope that could be used with a ⅛th. My instrument has always acted perfectly with that power giving a *full* field. With my own ⅛th it acts also very well, simply because the object-glass is "dumpy," or, in other words, has a very short setting, so that the prism comes closer to the back lens. I believe that improvements enabling the Binocular to be used with the highest powers are quite possible. I have not, however, given any attention to the subject for some years past.

Yours sincerely,

F. H. Wenham.

PROCEEDINGS OF SOCIETIES.

Royal Microscopical Society.

King's College, *November* 6, 1872.

Charles Brooke, Esq., F.R.S., Vice-President, in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations was read, and a vote of thanks passed to the respective donors.

Mr. Ingpen read a paper on "A Proposal for a Standard of Comparison of Magnifying Powers of Compound Microscopes."

Mr. Hogg said he did not quite understand how Mr. Ingpen would get rid of the disturbing elements in his calculations, when eye-pieces having different foci were used. The eye-pieces of no two makers were just alike.

Mr. Ingpen said the proposed use of the standard was not so much for competition as for real practical purposes. It was to enable the microscopist to place any objective that might come into his hands under the conditions in which he would ordinarily use that objective, and then to test its aperture by a standard which must, or must not, be recognized by other persons.

Mr. Brooke asked Mr. Ingpen whether there was any practical

defect in the very simple mode of measuring magnifying powers which was proposed by Dr. Beale, and which consisted in placing a scale ruled for 1000th of an inch on the objective. On looking through the oblique glass plate, and placing the ruled scale at ten inches from it, then measuring how many divisions of the scale this known 1000th of an inch occupies; the one divided by the other will immediately give the magnifying power. It seemed to be a much more simple method of ascertaining the point. Were there any optical reasons why the plan was not so correct in its results as Mr. Ingpen's?

Mr. Ingpen said Dr. Beale's contrivance was very good, but there were two objections to it. The neutral tint glass was placed in such a position that it gave a different magnifying power from what would be seen, because the image then was formed outside; in his plan the image was formed in the exact conjugate eye-piece and dynamometer. If we could trust Dr. Pearson, that image does not vary with different eye-pieces. The distortion of area in the Huyghenian eye-piece was so great that no certainty could be felt in measuring according to Dr. Beale's plan.

Mr. Brooke: You simply want the distance between the two parallel lines.

Mr. Ingpen: The distance would vary greatly if those lines were only to occupy say a $\frac{1}{4}$ of an inch in one case and $\frac{1}{2}$ an inch in the other. He meant to say that $\frac{2}{1000}$ths of an inch would not be exactly double $\frac{1}{1000}$th of an inch, in consequence of the great distortion of area which takes place in the Huyghenian eye-piece. It could be very easily tested with a micrometer. Then the thing is where do you measure your ten inches from? A very slight difference in distance would make a very great deal of difference in the accuracy of the measurement; if the micrometer was not perfectly horizontal, and so on, you could hardly get satisfactory measurements except of one kind. He thought Dr. Beale's plan hardly came up to the requirements of a standard. He thought also that a standard of a moderately simple character fitted with a piece of mechanism which could be made very accurately would be of greater value than that. Mr. Ingpen then adduced other reasons why Dr. Beale's contrivance was not likely to give perfectly accurate measurements, and as perfect accuracy was indispensable he believed that the method of measuring magnifying powers advocated in his paper would enable the microscopist to attain such a desirable result.

A vote of thanks was passed to Mr. Ingpen for his communication.

Mr. Slack read a paper "On the Structure of the Valves of *Eupodiscus Argus* and *Isthmia enervis*, showing that their silicious deposit conforms to the general plan of deposition in simpler forms."

In reply to a question from Mr. Wenham, Mr. Slack said he thought the "eye spot" to be a light image at the focus of the minute lenses. The rays from the new illuminator did not show it as well as when more direct light was used.

Mr. Stewart said that his examinations of the markings of the Diatomaceæ led him to a different conclusion as to their nature. He

believed they were usually depressions not elevations, but thought care must be taken to distinguish those belonging to the outer silicious lorica from others appertaining to the lining, or veil, which veil probably differed chemically and structurally from the rest of the valve.

The method adopted was one recommended by an American observer, and published in the 'Annals of Natural History' some years since. It depended on the relative indices of refraction between the diatom and the medium in which it is mounted. The silicious deposits in both animals and plants are of less index of refraction than Canada balsam. When mounted in this medium, they, if convex, will really act as a concave lens would in air, and *vice versâ*. To prove this examine a silicious and a calcareous spicule of a sponge in water and then in balsam. In water they will both appear *bright* when beyond the focus of distinct vision, and *dark* when within it; in balsam, however, the calcareous spicule will appear as before, but the silicious one will be dark when beyond the focus. In this experiment the compound eye of a beetle may be conveniently substituted for the calcareous spicule.

If now one of the larger diatoms be examined it will be found to behave in the same manner as the beetle's eye or calcareous spicule, thereby showing that if the diatom is of the same refractive power as the silicious spicule (which can be proved by other means) its markings must be depressions. The veil, however, is probably of greater index of refraction than the rest of the valve, and if this be the case the same method shows the markings in Heliopelta to be elevations.

Mr. Slack said he thought the markings were spheroidal, from an examination of a great many fractured valves, and from seeing some of the larger spheroids as lenses. He drew some analogy between the real diatom and Max-Schultze's artificial diatoms; and thought that the principle of chemical precipitation was similar in both. He was very much inclined to think from the examination of a great number of objects of the class in question that all were constructed on the same type. The vegetable matter of the diatom acts chemically upon the silex in the water, which is the condition of colloid silica, and the deposition always takes place in the spheroidal form.

Mr. Brooke would like to know how it happened, if the structure of these objects were really hollows and not bosses, that the line of fracture runs between the dots, and not through them. He had fractured a great many diatoms under the microscope, and had never seen a single instance in which the line of fracture did not run equidistant between two consecutive dots, which would hardly be the case if they were depressions and not elevations.

A vote of thanks was then passed to Mr. Slack.

Mr. B. T. Lowne read a paper "On the Nervous System of Insects and Crustaceans compared with that of Annuloida and Vertebrata."

Mr. Ingpen called the attention of the Fellows to a marvellous photograph of a solar spectrum made by Mr. Browning from a lithograph of Fraünhofer, which he thought was likely to prove of great value in spectrum analysis.

Mr. F. Fitch exhibited a specimen of Mr. Dalton's curiously beautiful slides of butterfly scales, arranged as a bouquet of flowers with insects.

Donations to the Library and Cabinet from October 2nd to November 6th, 1872:—

	From
Land and Water. Weekly	*The Editor.*
Nature. Weekly	*Ditto.*
Athenæum. Weekly	*Ditto.*
Journal of the Society of Arts	*Society.*
Journal of the London Institution, No. 17	*Institution.*
Journal of the Linnean Society, No. 55	*Society.*
On Callograptus radicans, *a New Graptolite.* By John Hopkinson, F.G.S.	*Author.*
Popular Science Review, No. 45	*Editor.*
Bulletin de la Société Botanique de France	*Society.*

The following gentlemen were elected Fellows of the Society, *viz.*:—

Arthur Angell, Jun., Esq.
John Thomas Atkinson, Esq.
Joseph F. Gibson, Esq.
Charles Thomas Hudson, LL.D.
Robert Marchant, Esq.
John Matthews, M.D., &c.

WALTER W. REEVES,
Assist.-Secretary.

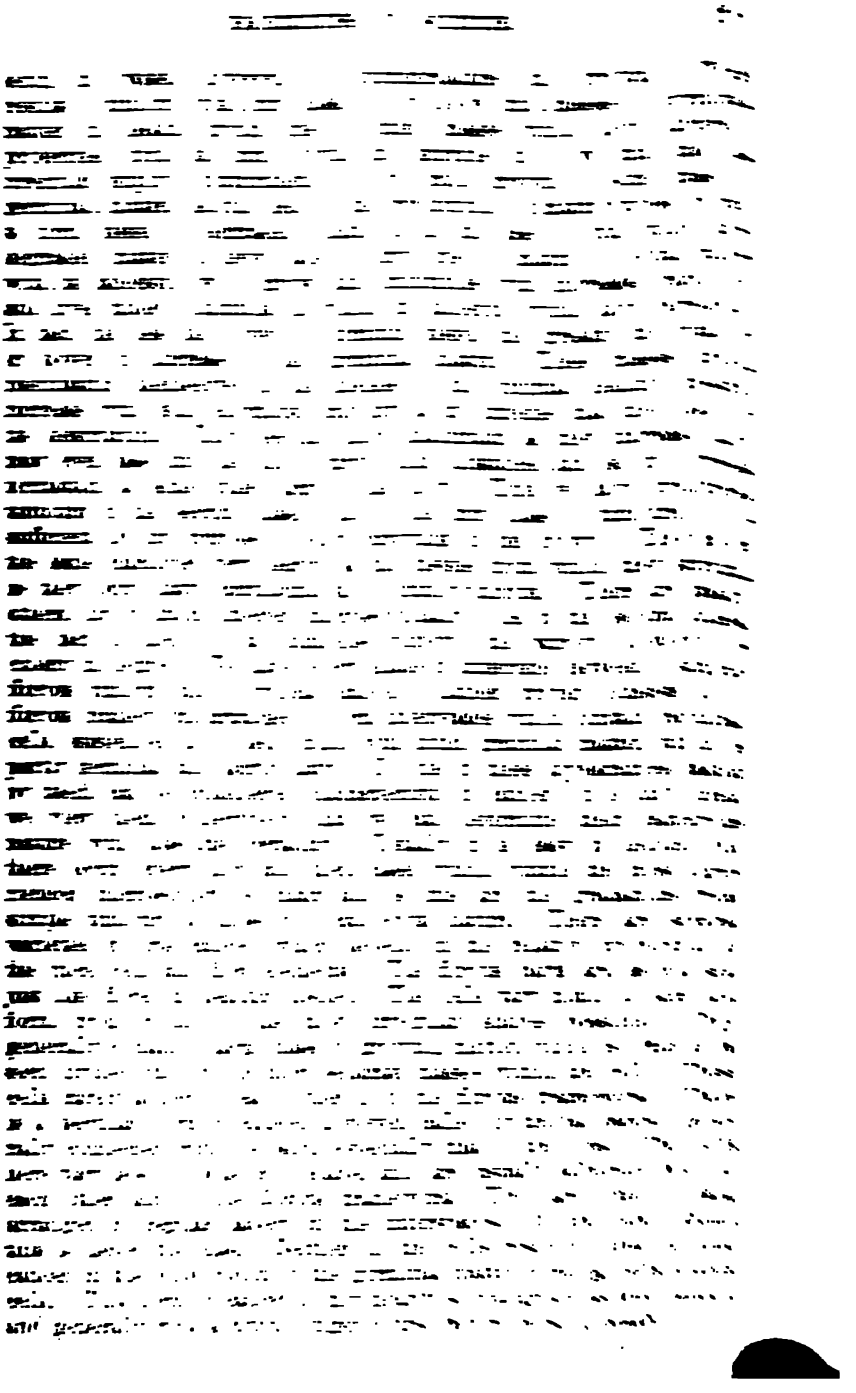
ERRATUM.—On p. 250 of the November number, line 7 from the top, for *larvæ* read *cilia.*

OLDHAM MICROSCOPICAL SOCIETY.

The members of the Oldham Microscopical Society held their second conversazione for the season in the club-room of the Oldham Lyceum, on the 18th of October, when, though the night was stormy, a very respectable company assembled. On entering the room we were struck by the fine display of first-class binocular and other microscopes, under which were displayed the various tissues and deposits indicative of disease; and on the walls numerous well-executed enlarged drawings were hung, intended to illustrate the subjects dealt with in what may be termed the central attraction of the evening—*viz.* the paper by Dr. George Thomson "On the Use of the Microscope in Disease." Among the subjects illustrated we observed under the microscopes several specimens which displayed the peculiar forms of cell which distinguish cancerous growths, with their fibrous framework and distinct nuclei; also pus cells, treated with acetic acid; sarcini, or fungoid growths, found in the vomit under certain conditions, &c., &c. The lecturer dealt with a subject which involved most important issues, and is necessarily of a most extensive character, the difficulty being to present to the non-professional mind sufficient to enable it to intelligently comprehend the subject, at least in outline, and yet not burden it with undesirable details. This happy medium, we think, was most success-

fully realized, and the attention manifested throughout is a proof that such a subject is capable, when properly treated, of being made interesting to the non-medical public as well as to the profession.

Some time having been spent in viewing the objects under the microscopes, and in easy chit-chat upon them, the chair was taken by the President, Dr. A. Thom Thomson, who briefly introduced the lecturer, who said that, while remembering that the microscopical study of disease was almost entirely confined to the profession to which he belonged, he thought such a paper as he was now to read, which aimed simply at giving a sketch of what was interesting in the use to which a busy medical man put his microscope, might prove acceptable to the non-professional members of the Society. The introduction of the microscope, he need hardly say, heralded a new era in medical science. The microscopical examination of tissues in health and disease upset many a pet theory, and cleared up much that was before all obscurity and doubt. Although not entirely to be relied upon, it was undoubtedly a valuable aid in the detection of disease. Suppose, for instance, he was called to a patient who had vomited blood from the stomach, and he had no certain means of determining what was the condition of that organ. Well, he would put a little of the blood under his microscope, and might find that it contained certain cells which characterize cancerous or other diseases, and this discovery would influence the treatment of the case. Before commencing the subject of his paper proper, he would give a brief description of the various structural elements of which the body was composed, namely, granules, cells, fibres, membranes, &c. Passing by the granular matter, he proceeded to describe the various cells of which the tissues are built up. Pus, or matter taken from abscesses, he described as composed of circular cells, somewhat larger than the blood corpuscles, and granular in appearance, and stated that, when treated with acetic acid, several nuclear bodies were visible in their centres. These cells are frequently to be met in the examination of diseased tissues and secretions. When viewed under high powers they display peculiar movements, and have the power to throw out protusions, which rapidly separate themselves, and develop into new cells. Pus cells he viewed as forms of growing matter degraded from its proper use, namely, that of forming tissues, but which is increased in vitality, and, as a consequence, multiplies itself more rapidly. Blood contained a large number of red, and a smaller proportion of white corpuscles, the latter identical in appearance with the pus corpuscle. In health the proportion is about one white to fifty red, while in some diseases the white become much more numerous; in extreme cases being nearly as prevalent as the red. In cholera, and in some kidney diseases, the red corpuscles have a tendency to adhere to each other. The character of the spit, when microscopically examined, frequently enables the practitioner to determine the character of the disease from which a patient is suffering. Healthy spit is often very much charged with extraneous matter; such, for instance, as silk and cotton fibres, from the articles of clothing, feathers, starch, particles of food, dust, &c. In all spit they found mucus, which consists of a viscid mass, containing cells of various shapes and sizes.



Besides, they had special appearances in some diseases; thus, in inflammation of the lungs they had minute oil globules, numerous blood corpuscles, and granular spherical cells. The yellow spit of bronchitis abounds in pus cells and epithelium in all stages of growth. In the spit of consumption they also found pus corpuscles or globules, and peculiar bodies called tubercle corpuscles, of an oval form, but without nucleus; also, at an early stage of the disease, fragments of lung tissue, which were invariable evidence of the disease. Having alluded to the advantages to be gained from a microscopical examination of vomit, Dr. Thomson proceeded to speak of the most important part of his subject—the examination of diseased growths and tumours. This part of the paper we give in full, as follows:—"It becomes often of the utmost importance to determine the nature of one of these growths after removal by the surgeon's knife; and it often rests with the microscopist alone to settle the question as to whether a return of the disease is likely. Even before removal it is sometimes possible, by detaching a minute portion of the tumour, to subject it to the microscope, and to decide as to the probability of an operation effecting a permanent cure. The increase to our knowledge on this subject alone, since the microscope was brought to bear on it, has been simply invaluable; and I have not a doubt in my mind but that we are on the eve of discoveries still more important, which may be the means of averting, in a great measure, some of the most dreadful diseases that scourge mankind. There are two grand classes of tumours. In the one you have the tumour produced by irregular growth of some tissue of the body, and consisting simply of the elements of that tissue. Such are fatty, and bony growth, and many others, of which you have a familiar example in warts, which are simply an irregular growth of the skin, and are composed of the same elements. These are simply deformities, and fraught with no danger to life, unless under extraordinary circumstances. The other class, however, is very different. Here we have structures not comparable to any natural tissue of the body. Such are all kinds of cancerous growths, to which I shall come presently. Now, it is considered a fact beyond dispute that every tissue in the body results from changes that have taken place on germinal or growing matter. The tissue is, in fact, formed matter. Also, that this growing matter was derived from other growing matter which existed before it, and which may even have had different properties from it. Now, in every healthy tissue you will find small collections of growing matter, from which new tissue is formed from time to time, to replace what is wasting away during the performance of its function, or when a portion has been destroyed by disease. And the rate of formation of this new tissue is in proportion to the destruction of the old tissue. Now, suppose that one of these germinal masses should become displaced by an accident, such as a blow, or from some other cause, should become irregularly supplied with nourishing material from the blood, and, in consequence, sprout into active growth when not required, the result would be a shapeless lump of tissue, in fact, a tumour. Now, this tumour having no office to perform, and hence no waste to undergo, continues to grow just as a healthy tissue would do, there

being no waste, however, to counterbalance the growth. Thus results a tumour of the first class. Now, as we find masses of germinal matter in health giving rise to other masses which have different properties from the first, it will not surprise us if we find that an irregular supply of nutriment, or a too rapid growth of some mass of germinal matter, should lead to the production of matter whose power to form tissue is degraded. And, as in the case of pus, while this degraded matter is never able to give rise to matter of equal value with its ancestor, yet it grows and multiplies with increased rapidity, and lives under conditions in which its ancestor would have perished. In fact, the less the power of forming tissue, the greater the vitality or power of increase of the germinal matter. These masses, then, insinuating themselves in the meshes of the original healthy tissue, increase with such enormous rapidity at its expense that they lead to its destruction. They become more malignant as they increase, and may even pass into the blood, travel long distances, and set up a fresh formation at some other part of the body. Thus we have produced tumours of the second class. As to the first class of tumours, it is sufficient for my purpose to show drawings of one or two. They have the same character very nearly as the tissues from which they spring, be they bony, fatty, cartilaginous, or simply fibrous. There are many others, not of much interest microscopically. As to the second class, the fact is there is an immense variety; the variety, however, is chiefly in degree. We have every shade of difference between a simple fibrous tumour and a typical cancer. Cancer proper consists of a fibrous framework, arranged to form interspaces which contain peculiar cells, suspended in a viscid fluid, with much granular matter, which is partly germinal and partly fatty. No one of these appearances, taken by itself, can be considered characteristic of cancer; it is only when we view them collectively that we can determine their cancerous nature with absolute certainty. Usually it is easy to decide, but there occur, every now and then, cases which puzzle the most experienced microscopist, so many and so fine are the gradations from simple tumours to those of a cancerous nature. There are several varieties of true cancer, which depend on the relative proportion of the juice, cell, and fibre elements. The fibrous parts are, as you see, just like fibres in healthy tissues. The cells vary much in size and form, from round to the most irregular shapes possible. They generally contain a large mass of growing matter, which is often to be seen divided into two or more separate masses within the cell. These cells rarely adhere to each other or to the fibrous framework. There is a peculiar form of cancer, however, called epithelial cancer, generally connected with the skin, especially that of the lips. The cells here vary less in size and shape, and are usually adherent both to each other and to the fibrous framework. They are often, in fact, arranged in regular layers in the interspaces. In the other forms this is never the case. Neither do the cells resemble that of true cancer in the subdivision of the germinal matter forming cells within cells. This form of cancer is not nearly so malignant as true cancer, and generally runs a much longer course before causing death."

Various other subjects were alluded to, and at the close a very cordial vote of thanks was passed to Dr. G. Thomson, on the motion of Dr. Platt, seconded by Mr. Pullinger. A very instructive evening was spent by the company.

Liverpool Microscopical Society.

The eighth meeting of the present session of this Society was held last month, in the Royal Institution, Colquitt Street, the Rev. William Banister presiding. An adjourned discussion took place on Mr. Newton's paper "On Spontaneous Generation," and on Dr. Bastian's work, "The Beginning of Life."

In the absence of the President, from indisposition,

The Rev. W. Banister opened the discussion. He said,—The subject for our consideration this evening is one of the most interesting in the whole range of microscopic study. There is a mystery about life—where it is, how it begins, and where it ends (I mean materially, not theologically), which fascinates all students of nature. One objection to Dr. Bastian's views, perhaps, is that they tend to solve this mystery, and to take a good deal of sentimentality out of our notions about it. I do not, however, think there is any probability of our solving the mystery. Even if all Dr. Bastian states is true, it only proves that animal and vegetable life, in its lowest form, is substantially the same, but it does not tell us where the vitality that animates an aggregation of bacteria comes from; still less does it account for the possession of so wonderful a property by each individual bacterium. There are two ways of proving, as far as possible, whether living matter can arise *de novo* without the previous existence of "germs." The one is to submit the infusions to so great a temperature that all vitality must be destroyed; the other is to examine the air minutely to see whether the "germs" exist in it in sufficient abundance or not. Both these tests require delicate and costly apparatus, and great skill, experience, and patience in the use of it. Those who do not possess these requisites must be content to accept the statements of those who do, provided they have reason to believe that the experiments have been carefully conducted and conscientiously interpreted. This may be ascertained by a comparison of the results arrived at by different experimenters of repute, and by a personal examination of such minor details as may be observed by students who possess moderately good instruments. If the facts observed, as far as they go, bear out the statements made, we are justified in believing that such statements as we are not able personally to verify are honestly in accordance with apparent fact. I say apparent, advisedly. The higher the powers used the more danger there is of error in the interpretation of what we seem to see. This is all that I have attempted to do; and I am bound to say that so far as I have observed, all the initial developmental changes which Dr. Bastian describes do take place. Dr. Bastian's theory is that in a fermentable solution there is a change from a diffused imperceptible state to a concentrated perceptible state, the

form usually first assumed being that of bacteria—vibriones and torulæ—the first being the simplest. These bacteria, or specks of living matter, as they increase in size, acquire a structure, develop organs, and become animals, or vegetables of more or less complicated nature. These aggregate and form the "proligerous pellicle" of Pouchet, which is composed of a dense mass of bacteria, generally end up, with some vibriones and torulæ imbedded in a pellucid gelatinous material. As two amœba coalesce and form one, so these bacteria fuse and form organisms. It seems to me that the truth or error of these statements may be verified by most members of this Society, and that we shall not perform our duty as members of it, or take any respectable rank among kindred societies, if we confine ourselves to the exhibition of petty things, and neglect the highest objects of microscopic study.

Mr. G. F. Chantrell, hon. secretary, said he considered Dr. Bastian's book one of the most valuable contributions to biology that has yet been made. It is a book that should be in the hands of every microscopist. It teems with strange and marvellous facts in infusorial life. Dr. Bastian has resuscitated the observation of several almost forgotten writers, and has verified some of their facts, which were little noticed at the time, and to which he has added a large number of his own observations. He agreed with Dr. Bastian that we are but on the threshold of our acquaintance with the marvellous heterogenetic transformations, the discovery of which already affords material for revolutionizing the old foundations of botanical and zoological science;—that the path opened must be followed by faithful and competent observers, true students who are willing zealously to watch and wait through eager hours whilst nature unfolds her secret processes, unblinded by any existing theories, and content to regard them as useful and modifiable aids to further progress. It was in the spirit of such a student he offered the following observations. He described the remarkable seed of the collomia plant, and the results he had obtained by keeping a single seed with a drop of distilled water in a live box. Almost immediately he had observed life, using a power of 5 to 700 diameters, and in course of a few days the glass became crowded. A single seed was kept under observation for above a month (the water evaporated being supplied occasionally), and during the whole time paramecia abounded. After a while a remarkable change took place; the paramecia had developed into six or seven different forms, of which he exhibited drawings. These died off, leaving a ring of dead substance, out of which soon appeared spots of granular matter, and these afterwards developed into perfect amœba. Dr. Bastian states in the preface to his book, that the spontaneous generation theory rests upon the possibility of the *de novo* origin of bacteria, and that evidence of the most convincing character shows that bacteria are killed by a temperature of 140° Fahrenheit. Mr. Chantrell quoted, in opposition to this, Dr. Crace-Calvert's experiments, showing that the greater portion of microscopic life was destroyed by a temperature of 200° F., but that black vibrios were not killed by 300°, and then instanced his own observations upon boiled vegetables. He had fre-

quently taken small portions of cooked vegetables which had been boiled for not less than half an hour, and invariably (with a power of 760 diameters) found life, and, in twenty-four hours afterwards, bacteria in abundance. From cooked vegetable marrow, on the second day, he had found ciliated infusoria, a monad, which appeared to have come from the tissues. On the fifth day he observed a fungoid growth set up, no doubt induced by the want of air. After describing a number of other observations, most of which were illustrated by drawings, he said that in all his examinations of cooked vegetable marrow, turnip, carrot, celery, potato, &c., all alike show these minute organisms. Again, in examining cooked hare, he had found these minute trembling specks, similar in every respect to those seen in the vegetable, and alike accompanied by bacteria. How strange that the animal and vegetable kingdom should assimilate so closely! Mr. Chantrell then remarked on the singular property that bacteria have of uniting and forming filaments. In the mature plant, or animal, it is ever on the move, coiling itself up and uncoiling; the movement of the spirillum is as yet a mystery. By an attentive study of boiled vegetables in glass troughs much may be learned. In the last few weeks he had been able to confirm several of Dr. Bastian's observations as well as others quoted by him, and he trusted that the members would follow up this study of home microscopy during the long evenings of winter, and they would learn to appreciate Dr. Bastian's valuable contribution. In the last few weeks he had learnt more of infusorial life by the aid of this book than in months of previous study. He said he was content to go working steadily on, neither declaring in favour of one hypothesis or the other. He feared the conclusions were too hasty; there is sadly too much tendency to set up theories and stick to them. We want a class of observers who will watch these extraordinary changes with the same fidelity that the telegraph clerks watch every movement of the magnetic needle of the Atlantic cable at Valentia, and, as the Christmas story has it, be, indeed, "slaves of the lamp." We want slaves of the microscope, who will faithfully record every change in these remarkable evolutions of infusorial life.

A discussion followed, in which Mr. Newton, Dr. Inman, and Dr. Drysdale took part; and on the motion of Mr. R. Hamilton, F.R.C.S., the discussion was adjourned to the December meeting.

INDEX TO VOLUME VIII.

A.

Acarelli, Notes on New. By J. G. Tatem, 263.
Achromatic Objectives for the Compound Microscope, Remarks on the Nomenclature of. By Col. Dr. Woodward, 66.
Agassiz, Prof., on Tornaria, the young condition of Balanoglossus, 276.
Allman, Prof., on the Structure of Graptolites, 31.
American Lobster (*Homarus Americanus*), the Early Stages of the, 131.
Amphipleura pellucida, Photographing, 204.
Angular Aperture, How to Measure, 276.
Archer, Mr. W., on a Minute Nostoc with Spores, 90.
——, Mr. William, Observations on Collema, 145.

B.

Balanoglossus, Tornaria is the young condition of. By Prof. Agassiz, 276.
Balantidium coli, the Anatomy of. By M. Wising, 186.
Beale, Dr. L. S., on the Nerves of Capillary Vessels and their Probable Action in Health and Disease, 55.
—— on the Active Part of the Nerve Fibre, and on the Probable Nature of the Nerve Current, 178.
Berggren, Dr., on Microscopic Organisms found in Inland Ice, 142.
Bicknell, Edwin, Further Remarks on Tolles' $\frac{1}{8}$th and Powell and Lealand's Immersion $\frac{1}{16}$th, 13.
Biscoe, Prof. T. D., on the Stomata of Leaves, 31.
Blood Cells, Influence of Quinine on the White, 93.
Bog Mosses. By Dr. Braithwaite, 3, 157.
Bone, Diseased, and Giant Bone Cells, 134.
—— Cells, Giant, 134.
Brady, Mr. H. B., on Saccammina Carteri, a Foraminifer from the Carboniferous Limestone of Northumberland, 33.
Braithwaite, Dr. Robert, on Bog Mosses—a Monograph of the European species. Part V., *Sphagnum rubellum*, 3. Part VI., *Sphagnum neglectum*, 157.
British Association, the, 203.

C.

Cancer, the Minute Anatomy of Two Cases of. By Col. Dr. Woodward, 109.
Capillary Vessels, the Nerves of, and their Probable Action in Health and Disease. By Dr. L. S. Beale, 55.
Carmine Staining the Tissues, the Best Mode of, 37.
Carruthers, William, F.R.S., on the History, Histological Structure, and Affinities of Nematophycus Logani, Carr., an Alga of Devonian Age, 160.
Cell Development, the Theories of. By I. N. Danforth, M.D., 121.
Cephalopoda, the Embryology of Fossil, 198.
Cerebrum, Grey Substance of the Cortex of the, 238.
Chalk, Microscopic Fossils in Irish. By Prof. Hull, F.R.S., 142.
Cholera, Is there Loss of Epithelium in? 36.
Collema, Observations on. By Mr. William Archer, 145.
Coloured Glass in Microscopy, on the Employment of. By M. Mouchet, 103.
Correspondence:—
Anderson, W., 40.
Beck, Rev. J., 95.
Bicknell, Edwin, 40.
Brakey, S. L., 247.
Mayall, John, jun., 41.
Mouchet, M., 96.
Smith, Samuel, 247.
Stodder, Charles, 39.
Tolles, Robert B., 147.
Ward, R. H., 206.
Wenham, F. H., 279.
Crow's Skull, on the Structure and Development of the. By W. K. Parker, F.R.S., 217.
—— —— Description of the Plates Illustrating, 253.

CURTIS, CHARLES, Remarks on the Homological Position of the Members constituting the Thecated Section of the Class Rotatoria, 5.

D.

DANFORTH, I. N., on the Theories of Cell Development, 121.
DAWSON, Dr., and Professors KING and ROWNEY, on the Eozoon, 198.
Desmids, American, 90.
Diatom Structure, the Nature of. By Professor WEBB, 194.
Diatomaceæ, a Conspectus of the. By Professor SMITH, 34.
Draw-tubes *versus* Deep Eye-pieces. By M. A. PRAZMOWSKI, 128.

E.

EDWARDS, Professor A. M., Notes on some Microscopic Organisms, 22.
——, Note on Silicious Incrustations from the Geysers of the Yellowstone River, Wyoming Territory, 104.
ELSBERG, Dr. LOUIS, on the Regeneration Hypothesis, 182.
Entomology, the Microscope in, 32.
Eozoon, the. Dr. DAWSON and Professors KING and ROWNEY on, 198.
Euchlanis triquetra and E. dilatata. By C. T. HUDSON, LL.D., 97.
Eupodiscus Argus and Isthmia enervis, on the Structure of the Valves of, showing that their Silicious Deposit conforms to the General Plan of Deposition in Simpler Forms. By H. J. SLACK, F.G.S., 256.

F.

Fat-cells, Whence Come? 238.
FINDLEY, GEORGE, on a Proposed Standard Micrometer Eye-piece or Eidometer; and on a Uniform and easily applied Method of Naming Microscopic Objectives, 264.

G.

Glass, Microscopic, on a Method of estimating the Optical and Actual Thickness of. By Dr. ROYSTON-PIGOTT, 269.
Graptolites, the Structure of. By Prof. ALLMAN, 31.
——, the Mode of Existence of the Dendroid, 194.
——, Some New Species of, 275.

H.

HARTLEY, W. N., Arguments against Spontaneous Generation, 138.
Histology, Human and Comparative, a Manual of. Edited by S. STRICKER, &c., 272.
HOLL, Dr. B., on the Epitheca in Fossil Sponges, 141.
Homarus Americanus, the Early Stages of, 131.
HUDSON, C. T., LL.D., Is Pedalion a Rotifer? 209.
—— on Euchlanis triquetra and E. dilatata, 97.
HULL, Professor, on Microscopic Fossils in Irish Chalk, 142.
Hydractinia, a Fossil, 140.

I.

Ice, Microscopic Organisms found in Iceland. By Dr. BERGGREN, 142.
Immersion Objectives, on Angular Aperture of. By ROBERT TOLLES, 106, 203, 243.
—— ——. By Dr. J. CURTIS, 242.
Impetigo contagiosa, a Microscopical Examination of the Skin in, 132.
Infusoria, a New Slide for Exhibiting, 242.
INGPEN, J. E., Esq., Proposal for a Standard of Comparison of the Magnifying Powers of Compound Microscopes, 253.

L.

Laminar Membrane, the precise period at which it appears, 202.
Logwood Staining of Tissues, 277.
LOWNE, B. T., M.R.C.S., Notes on the Development of the Nervous System of the Annulosa, 259.

M.

MADDOX, R. L., M.D., on some Methods for preparing the Tissues of the Frog Tadpole's Tail, 101.
Magnifying Power and Minute Magnitudes by Miniatures, on a Method of ascertaining. By Dr. ROYSTON-PIGOTT, M.A. &c., 266.
MAPLESTONE, CHARLES M., Notes on Victorian Mollusca and their Palates, 45.
Miasm viewed with the Microscope, 34.
Micrographic Dictionary, the, a Guide to the Examination and Investigation of the Structure and Nature of

Microscopic Objects. Third Edition. By J. W. Griffith, M.D. Assisted by the Rev. M. J. Berkeley, M.A., and Rupert Jones, F.G.S., 193.
Micrometer Eye-piece or Eidometer, on a Proposed Standard. By George Findley, 264.
Micro-pantograph. By Isaac Roberts, F.G.S., 1.
Microscope, Application of the, to Petrography and Mineralogy, 38.
——, a New Old Book on the, 38.
——, What work can be done with the, 146.
Microscopes, our Present Medical and Students'. By R. H. Ward, M.D., 74.
Microscopic Work, a Prize for, 203.
—— Specimens, Pathological, 274.
Molluscum contagiosum, a Skin Disease resulting from a Parasite, 32.
Monads, one of the Common. By Prof. A. H. Tuttle, 190.
——, Phases in the Life-history of, 36.
Monochromatic Sunlight, On the Use of, as an Aid to High-power Definition. By Dr. J. J. Woodward, 186.
Morse, Edward, on the Embryology of Terebratulina, 274.
Mouchet, M., on the Employment of Coloured Glass in Microscopy, 103.
Museum, the Hunterian, and its Microscopical Collection, 147.

N.

Nematophycus Logani, an Alga of the Devonian Age, on the History, Histological Structure, and Affinities of. By William Carruthers, F.R.S., &c., 160.
Nerve Fibre, on the Active Part of the, and on the Probable Nature of the Nerve Current. By Dr. L. S. Beale, F.R.S., 173.
—— Tissue, Muscle as a Constituent of, 237.
Nerves, the Degenerative Process in, 236.
Nobert's Plate. Remarks on the Resolution of the Nineteenth Band of, by certain Objectives, especially by a New Tolles's Immersion $\frac{1}{25}$th. By Dr. J. J. Woodward, U. S. Army, 227.
Nostoc with Spores, a Minute. By Mr. W. Archer, 90.
Notes on the Development of the Nervous System of the Annulosa. By B. T. Lowne, M.R.C.S., 259.

O.

Object-glasses, the Aperture of. By F. H. Wenham, 231.
Objectives, the Nomenclature of, 93.
—— and Oculars, on Uniformity of Nomenclature in regard to Microscopical. By R. H. Ward, M.D., 15.
——, Misnaming. By Mr. C. Stodder, 240.
—— ——. By Mr. F. H. Wenham, 241.
Opaque Illumination under High Objectives, 197.
Optical and Actual Thickness of Microscopic Glass, 269.
Organisms, Notes on some Microscopic. By Prof. A. M. Edwards, 22.

P.

Palates of Victorian Mollusca, Notes on. By Charles Maplestone, 45.
Paraboloid, the, an Immersion Instrument, 205.
Paralysis, Brain Tissue in general, 238.
Parker, W. K., F.R.S., on the Development of the Skull of Salmo salar, 91.
—— on the Structure and Development of the Crow's Skull, 217.
—— Description of the Plates illustrating the Development of the Crow's Skull, 253.
Pedalion, Is it a Rotifer? By C. T. Hudson, LL.D., 209.
Penis, The Histology of the, 201.
Piffard, Dr. H., on a Microscopical Examination of the Skin in Impetigo contagiosa, 132.
Pigott, Dr. Royston-, M.A., on a Method of ascertaining Magnifying Power and Minute Magnitudes by Miniatures, 266.
—— on a Method of estimating the Optical and Actual Thickness of Covering Glass, 269.
Podura Scale, the True Markings of the Test, 194.
Prazmowski, M. A., Draw-tubes *versus* Eye-pieces, 128.
Printing, Photo-mechanical, 242.
Prism, a New Erecting, 276.
Pritchard, Dr. Urban, on the Structure of the Rods of Corti, 131.
Proceedings of Societies:—
Brighton and Sussex Natural History Society, 96, 152, 206, 251.
Liverpool Microscopical Society, 286.
Margate Microscopical Society, 155.
Oldham Microscopical Society, 148, 282.
Quekett Microscopical Club, 155.

PROCEEDINGS OF SOCIETIES—*continued.*
Reading Microscopical Society, 252.
Royal Microscopical Society, 42, 248, 279.
Protoplasmic and Fungus Life, the Power of various Substances to Destroy. By Dr. CRACE-CALVERT, F.R.S., 132.
Pyæmia and Bacteria, the Connection between. By Dr. SANDERSON, 35.

Q.

Quinine and Arsenic, the Microscopic Action of. By Dr. J. J. RICHARDSON, 90.
——, Influence of, on the White Blood Cells, 93.

R.

RANVIER, M., on the Structure of Connective Tissue and Tendon, 235.
Regeneration Hypothesis, on the. By Dr. LOUIS ELSBERG, 182.
Retina, the Internal Limiting Membrane of the, 197.
RICHARDSON, Dr. J. J., on the Microscopic Action of Quinine and Arsenic, 90.
ROBERTS, ISAAC, F.G.S., on a Micropantograph, 1.
Rods of Corti, the Structure of the. By Dr. URBAN PRITCHARD, 131.
Rotatoria, Remarks on the Homological Position of the Members constituting the Thecated Section of the Class. By CHARLES CUBITT, 5.
Royal Society, the Biological Secretaryship of the, 94.

S.

Saccammina Carteri, a Foraminifer from the Carboniferous Limestone of Northumberland. By Mr. H. B. BRADY, 33.
Salmo salar, Development of the Skull of. By W. K. PARKER, 91.
SANDERSON, Dr., on the Connection between Pyæmia and Bacteria, 35.
Silicious Incrustations from the Geysers of the Yellowstone River, Wyoming Territory, Note on. By Professor A. M. EDWARDS, 104.
Skin Grafting, the Microscopic Anatomy of, 198.
SLACK, H. J., F.G.S., on the Structure of the Valves of *Eupodiscus Argus* and *Isthmia enervis*, showing that their Silicious Deposit conforms to the General Plan of Deposition in Simpler Forms, 256.
SMITH, Prof. W. H., a Conspectus of the Diatomaceæ, 34.
Sphagnum rubellum, 3.
—— neglectum, 157.
Sponges, the Epitheca in Fossil, 141.
Spontaneous Generation, Arguments against, 138.
Standard of Comparison of the Magnifying Powers of Compound Microscopes, Proposal for a. By J. E. INGPEN, Esq., 253.
STIMPSON, Death of Dr., 146.
STODDER, Mr. C., on Misnaming Objectives, 240.
Stomata of Leaves. By Prof. BISCOE, 31.
Sturgeon, the Development of the, 31.
Syphilis, the Microscopy of the Blood in, 136.

T.

TATEM, J. G., Notes on New Acarelli, 263.
Tendon and Connective Tissue, the Structure of. By M. RANVIER, 235.
Terebratulina, the Embryology of. By EDWARD MORSE, 274.
Tissues of the Frog Tadpole's Tail, on some Methods for Preparing the. By R. L. MADDOX, M.D., 101.
——, Logwood Staining of, 277.
Tolles' ⅛th and Powell and Lealand's Immersion $\frac{1}{25}$th, Further Remarks on. By EDWIN BICKNELL, 13.
TOLLES, ROBERT, on Angular Aperture of Immersion Objectives, 106.
—— ⅛th and Powell and Lealand's $\frac{1}{25}$th, Reply to Further Remarks on. By Dr. J. J. WOODWARD, 158.
——, Mr., New Optical Instrument, 205.
Triceratium, the Double Markings of. By Col. Dr. WOODWARD, 32.
TUTTLE, Prof. A. H., on One of *Our* Common Monads, 190.

U.

Uterus, Bacteria in the, 197.
——, the Structure of the, 202.

V.

Veins, Structure of Normal and Varicose, 140.

W.

WARD, Dr. R. H., on Uniformity of Nomenclature in regard to Microscopical Objectives and Oculars, 15.
——, our Present Medical and Students' Microscopes, 74.

WENHAM, Mr. F. H., on the Aperture of Object-glasses, 231.
——, Appendage to Mr. C. Stodder's Remarks on Misnaming Objectives, 241.
——, on using the ⅛th Binocularly, 279.
WISING, M., on the Anatomy of Balantidium coli, 196.
WOODWARD, Col. Dr., on the Double Markings of Triceratium, 32.
——, Remarks on the Nomenclature of Achromatic Objectives for the Compound Microscope, 66.
WOODWARD, Col. Dr., on the Minute Anatomy of Two Cases of Cancer, 109.
——, Dr. J. J., Reply to "Further Remarks on Tolles' ⅛th and Powell and Lealand's $\frac{1}{16}$th Immersion," 158.
—— on the Use of Monochromatic Sunlight, as an Aid to High-power Definition, 186.
——, Remarks on the Resolution of the Nineteenth Band of Nobert's Plate by certain Objectives, especially by a New Tolles' Immersion $\frac{1}{16}$th, 227.

END OF VOLUME VIII.

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